TARGETING EFFICIENCY AND TAKE-UP OF *OPORTUNIDADES*, A CONDITIONAL CASH TRANSFER IN URBAN MEXICO IN 2008

DPhil Degree in Comparative Social Policy, University of Oxford

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Oportunidades is a Conditional Cash Transfer (CCT) that uses a proxy means-test targeting model to select eligible households for the programme. According to the Income and Expenditure Household Survey of 2008, approximately two in every three eligible rural households participate in Oportunidades, whereas only one in every three eligible urban households receives the Oportunidades cash transfer. This research explores the factors behind this lack of take-up, the costs of participation and the implications of targeting inefficiency on the programme's impact on income poverty. It argues that a sample selection model is a pertinent tool of analysis as it informs on the distribution of cash transfers conditional on household eligibility. This conditional distribution is also used to understand the costs of participation as a latent variable. Eligible households are less likely to invest in human capital and neither the cash transfer nor the income forgone by children and teenagers are sufficient to overcome these costs of participation. By identifying a method to quantify behavioural change of households, I associate the costs of participation to the difficulties of inducing health-related behavioural change among recipients and eligible non-recipients. At an aggregate state level, targeting inefficiency is not fully explained by only looking at the budget constraints of the programme. In fact, targeting efficiency is positively associated to aggregate behavioural change and negatively associated to aggregate costs for participation at state level. Yet, targeting efficiency does not guarantee impact on income poverty and Oportunidades' highest impact on income poverty also associated with the inclusion of non-eligible households in the programme. This research reconsiders the importance of the context in which CCTs are implemented and informs on the conflicting aims of CCTs: providing income poverty relief via cash transfers and incentivizing behavioural change by conditioning the cash transfer in health and education investment.

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TABLE OF CONTENTS

1. Introduction....1
2. Framework of analysis and selective introduction to important literature
2.1 Framework of analysis....7
2.1.2 Targeting efficiency of public policies....9
2.2 Empirical research on participation in welfare benefits....15
2.2.1 Early behavioural/psychological models of take-up....20
2.2.2 Economic models of welfare take-up....20
2.2.3 Administrative - bureaucratic models....23
2.2.4 Behavioural-Structural-Scheme model....24
2.2.5 Limitations of the analysis of targeting efficiency....25
2.2.6 The case of Progresa-Oportunidades....26
2.3 How does Oportunidades target its recipients?....31
2.4 Targeting efficiency of Oportunidades in 2008....33
2.5 The two faces of poverty....36
2.6 Research questions....41
2.7 Policy aims, behavioural change and targeting efficiency....43
2.8 The latent variable of costs of participation....49
2.9 Timeframe of research....51
3. Methodology chosen to explore participation in Oportunidades and its targeting efficiency....52
3.1 The focus on the conditional probability of cash receipt and eligibility.53
3.2 The sample selection or Heckman correction method....60
3.3 A comparison between the sample selection correction, a probit and an ordinary least square models in estimating Oportunidades' participation....79
3.4 Empirical approach to elicit information about compliance with conditionality and behavioural change induced by the programme....85
3.5 Welfare participation costs as a latent variable....97
3.5.1 Formalisation of the analysis of errors....99
3.6 The assessment of targeting performance and its regional variations ..103
3.6.1 The link between methodologies of Chapters 4 and 6: the sample selection model is conditioned by the regional score....103
3.6.2 Methods for assessing targeting performance: Targeting Differential and Concentration index....107
3.6.2.1 The targeting differential....108
3.6.2.2 The Concentration Index....109
3.7 Conclusions....115
4. Targeting efficiency and the costs of health conditionality of Oportunidades in urban areas
4.1 Introduction....117
4.2 Hypothesis for non-take-up and data specification....118
4.3 Compliance with conditionality as a determinant of participation....122
4.3.1 Descriptive statistics of school conditionality....127
4.3.2 Descriptive statistics of health conditionality....132
4.3.3 Evidence of behavioural change....137
4.4 Sample selection correction model of participation in Oportunidades 145
4.5 Targeting efficiency of the sample selection correction model....160
4.6 Conclusion....163
5. What can the expected value tell us about the cost of Oportunidades?
5.1 Introduction and rationale behind the latent variable....167

5.2 The nature of measurement error in the model 173
5.3 The expected value and the cost of participation 178
5.3.1 The expected value of participation 178
5.3.2 Cost as compensation for barriers to participation 188
5.4 Cost of participation by targeting categories and vulnerable groups 192
5.5 Conclusions 206
6. Assessment of targeting efficiency by geographical areas and contribution to explain impact on income poverty 210
6.1 The regional differences of the means-test and the targeting differential 213
6.2 The targeting differential and regional targeting efficiency 221
6.3. The supply side as determinant of targeting efficiency 225
6.4 Demand side of the programme and targeting efficiency 232
6.5 Targeting differential and impact on income poverty 251
6.6 Conclusion 260
7. Conclusions and considerations for future research 263
8. References 274

APPENDICES

A Linear and probit regression estimations 286
B Data on targeting efficiency and impact on income poverty at state level 289
C Descriptive statistics of the variables of the model 291
D Schedule of entitlement according to household characteristics 295

Index of figures

2.1 Diagram for analysing targeting efficiency 10
3.1 Inverse mills ratio by regional penalization in the means-test model 67
3.2 Mean inverse mills ratio of neighbours and error term of the sample selection equation 69
3.3 Mean inverse mills ratio of neighbours and proportion of eligible households in neighbourhood 69
3.4 Mean inverse mills ratio of neighbours and proportion of households receiving cash transfers in neighbourhood. 87
3.5 Predicted probabilities of the sample selection and probit models by eligibility 81
3.6 Second step: joint probability (eligible and cash receipt) in the sample selection model, by eligibility 82
3.7 Predicted probabilities with the sample selection model and the linear regression for eligible households 83
3.8. Predicted probabilities with the sample selection model and the linear regression for eligible non-recipients 84
3.9 Predicted probabilities with the sample selection model and the linear regression for non-eligible recipients 85
3.10 Difference between predicted probabilities of probit model and joint probability of sample selection for inclusion and exclusion errors 105
3.11 Concentration curve for Oportunidades cash transfers 111
3.12 Concentration index by regions of Oportunidades and regional penalisation 113
3.13 Concentration index and mean household income per capita by states 113

3.14 Concentration index and difference between poverty and Oportunidades headcount by states....114
4.1 Distributions of monthly household income, cash transfer and income brought by teenagers, by teenage school attendance....131
4.2 Distributions of monthly household income, cash transfer and income brought by children, by children school attendance....132
4.3 Distributions of non-monetary and monetary health expenses and cash transfers by preventive health practice....134
4.4 Households that refrain from medical treatment by eligibility status....137
4.5 Proportion of households with health and school attendance higher than their neighbours, by categories of targeting....143
5.1 Path diagram of the specified model....178
5.2 Eligibility score and standardized error of model....192
5.3 Difference between linear predicted probability of sample selection model and of the linear model and costs of participation....194
5.4 Distribution of cost of participation, cash transfers and income brought by school-age people in the household, by education conditionality....197
5.5 Distribution of household health expenses, cash transfers and cost of participation, by health conditionality....200
5.6 Distribution of household health expenses, cash transfers and cost of participation by barriers to access health services....201
5.7 Distribution of cost of claiming Oportunidades conditional cash transfer....205
6.1 Map of the regional score in Oportunidades' means-test....215
6.2 Maps of proportion of eligible and cash recipient households by state....218
6.3 Household income per capita and proportion of school attendance per household by age....219
6.4 Household income per capita and proportion of households who's member had her weight measured....220
6.5 Exclusion and inclusion errors and targeting differential by state....224
6.6 Eligibility, budget allocations and targeting differential by state....229
6.7 Eligibility, income poverty and targeting differential by state....234
6.8 School and preventive health attendance and targeting differential by state....235
6.9 Proportion of eligible household that had health check-ups more frequently than their neighbours by state and targeting differential....237
6.10 Change in proportion of population enrolled in the main public health insurance Provider (IMSS) by state and targeting differential....238
6.11 Household with child school attendance higher than neighbours by state and targeting differential....239
6.12 Proportion of eligible households with higher teenage school attendance than neighbours by state and targeting differential....240
6.13 Changes in proportion of children and teenagers attending school between 2000 and 2010 by state and targeting differential....241
6.14 School-related behavioural change and income earned by children and teenagers....243
6.15 School-related behavioural changes and mean natural logarithm of income brought home by children and teenagers....244
6.16 Health-related behavioural change and mean natural logarithm of monetary and non-monetary health expenditure....245
6.17 Behavioural changes and mean costs of participation at state level....247

6.18 Mean costs of participation at state level and income brought by teenagers 249
6.19 Mean costs of participation at state level and regional penalty score....250
6.20 Inclusion errors and impact on capability poverty headcount by state....257
6.21 Exclusion errors and impact on capability poverty headcount by state....257
6.22 Targeting differential and impact on capability poverty headcount by state....236

Index of tables

2.1 Research that has estimated take-up of means-tested welfare policies....17
2.2 Indicators used to determine eligibility to Oportunidades....31
2.3 Descriptive statistics (means) of Oportunidades current targeting model....32
2.4 Households in Mexico receiving Oportunidad*es* cash transfers by eligibility, column percentages....34
2.5 Households in Mexico receiving Oportunidades cash transfers by eligibility, row percentages....34
2.6 Rural households receiving Oportunidades by eligibility, column percentages....34
2.7 Rural households receiving Oportunidades by eligibility, row percentages....34
2.8 Urban households receiving Oportunidades by eligibility, column percentages....34
2.9 Urban households receiving Oportunidades by eligibility, row percentages ..34
2.10 Income poverty headcounts (households) by eligibility....36
2.11 Income poverty headcounts (households) by eligibility and urban and rural areas....37
2.12 Income poverty headcounts (households) by eligibility, cash transfer and areas....39
3.1 Contingency table of eligibility and oportunidades cash receipt for households in urban....53
3.2 Oportunidades cash transfer by eligibility and indigenous households....57
3.3 Comparison of the performance of instrumental variables....75
3.4 Difference between linear predicted probability of sample selection model and of the linear model, by household eligibility and cash receipt....86
3.5 Average proportion of households that comply with conditionality by household eligibility and cash receipt....93
3.6 Average proportion of households that comply with conditionality by household eligibility and cash receipt....94
4.1 Average proportion of households and neighbours that comply with education conditionality by household eligibility and cash receipt 128
4.2 Average proportion of households and neighbours that had preventive health check-ups by household eligibility and cash receipt 133
4.3 School and health check-ups attendance among eligible non recipients....135
4.4 Households who refrain from medical assistance....136
4.5 Coady and Parker (2009) eligibility and participation rate in urban 147
4.6 Eligibility and participation rate in urban Oportunidades 148
4.7 Descriptive statistics of the dependent and independent variables of the model 149
4.8 Sample selection correction for urban and rural cash transfers conditional on eligibility....155

4.9 Distribution of and take-up of eligible urban households – different models 161
4.10 Targeting errors and differentials of models of take-up 162
5.1 Descriptive statistics of dependent and instrumental variables of the model 182
5.2 Expected value of cash receipt conditional on eligibility 183
5.3 Mean expected value of cash transfer by household eligibility and cash receipt 186
5.4 Mean expected value of cash transfer by eligibility, cash receipt, and rural and urban areas 187
5.5 School and health check-ups attendance among eligible non recipients 187
5.6 Mean predicted probabilities, expected values and costs of participation by household eligibility and cash receipt 195
5.7 Mean predicted probabilities, expected values and costs of participation by household eligibility and cash receipt 196
5.8 Average costs that result from the participation model, for households that comply with conditionality by household eligibility and cash receipt ... 198
5.9 Mean benefits and costs for recipients 202
5.10 Mean benefits and costs for urban eligible non-recipients 203
5.11 Mean logarithm of benefits and entitlement after considering costs of participation by household eligibility, cash receipt and conditionality 204
6.1 Scores for different geographical regions in the means-test model 214
6.2 Targeting errors and differentials for 16 states 223
6.3 Cash recipient households by eligibility 226
6.4 Proportion of eligible households and that receive cash transfers by state 230
6.5 Impacts on aggregate capability poverty measures for Mexico 253
6.6 Correlation coefficients of poverty impact and targeting measures 254
6.7 Linear regression of poverty impact on targeting measures 255

1 INTRODUCTION

Conditional Cash Transfers (CCTs) are internationally recognized as a tool used in the design of antipoverty policies. They are thought to affect the relative price of investment in human capital with the aim of incentivising such investment. As de Janvry and Sadoulet (2006) explain, the cash transfer does not only provide for income but also affect the price of health and education by making the cash transfer conditional on investing in human capital. In order to guarantee that resources reach the poorest population, many CCTs use Means-Tested Targeting (MTT) as a mechanism to select their target population. It is assumed that targeted policies are more progressive and less expensive than universal policies which are open to the entire population and do not explicitly define a target group. In reality, the capacity of the public administration may be limited to implement targeting thoroughly[1]. More fundamentally, the design of the targeting mechanisms should coherently match policy objectives and targeting instruments used for selecting the target population.

It is the task of policy makers to fine-tune objectives with policy design and implementation. Failure to do this may cause targeting inefficiency. As Atkinson (1995) explains, conditional cash transfers (CCTs) often have objectives other than handing out money to income-poor households, such as fostering investment in health and education. But a policy that aims to provide health insurance may be better targeted at the health-vulnerable population than at the income-poor population. Therefore, the analysis of targeting efficiency should assess whether targeting instruments have effectively identified their target populations, and if that targeting is compatible with the objective of poverty alleviation (Walker, 2005).

[1] . MTT relies heavily on the capacity of the public administration to gather income information –prone to measurement errors— and to implement other complex targeting mechanisms that often complement MTT (Sen, 1995).

A multiplicity of objectives is at the heart of the design of Oportunidades, a CCT implemented in Mexico and based on MTT. Oportunidades defines poverty on an income basis. Yet, instead of measuring income directly, proxy indicators of income are used for means-testing, as they could be easier to verify than income information that is often misreported. Moreover, Oportunidades provides cash transfers on the condition that children households attended school and mothers attended health talks. Conditionality represents, in fact, a targeting instrument additional to means-test as compliance with conditions cannot be taken for granted. The selection criteria to participate in Oportunidades are complex which may result, paradoxically, in targeting inefficiency.

Up to 2008, the method for selecting the eligible population in Oportunidades was questioned by low take-up rates in urban areas[2]. This research reflects on the extent to which a complex definition of poverty is at the heart of low participation in the programme. In practice, Oportunidades is allocated not only on the grounds of means-test. Certain barriers[3] prevent households from accessing the cash transfer, and the effect of the barriers is better understood if we focus on the eligibility of households.

Oportunidades, a means-tested CCT, was first designed as an experimental randomized intervention that could provide evidence of its impact in the people 'treated' with the programme. This evidence of its impact on improving health and education of poor families has been a major source of legitimacy as a public policy. However, it is not yet known what component makes the CCTs so effective. Cash transfers undoubtedly help but conditionality has proved to be an important component in improving health and education, though some limitations still hold[4].

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[2] The means-test model changed in 2009 and the income and expenditure survey of 2010 could be used to research this new targeting model. The data was available only until September 2012, so it is not included in this research.

[3] Some of the barriers in the context of implementation are administrative barriers (case management, sanctions) and transaction costs (transportation, time queuing, cost of applying, psychological cost or stigma).

[4] Conditionality of the programme improved attendance to preventive health and enrolment in secondary education, but wasn't effective in guaranteeing basic levels of health access and basic education (de Braaw and Hoddinott, 2008; Glerter, 2004).

The progressiveness of Oportunidades has also been acknowledged (Skoufias and Davis, 2001; Coady and Parker, 2009) and research argue that MTT is the instrument that guarantees allocation of expenditure among poorest population (Coady, Grosh and Hoddinott, 2004 and Castañeda *et al*, 2005)[5]. Nevertheless, there is a major challenge in increasing targeting efficiency and effectiveness of Oportunidades.

It is often assumed that eligible recipients would always demand cash transfers or welfare benefits. However, barriers to participation in antipoverty policy may arise. These barriers can roughly be accounted for by three constraints: information costs (knowledge about how to claim the benefit), transaction costs (time, transportation, conditionality and others), and outcome costs (stigma and others) may deter people from taking-up welfare benefits (Remler *et al* 2001; Brewer, 2003). This research explores some of the transaction and outcomes costs that households eligible for and recipients of Oportunidades face. It also proposes a latent variable method to identify other unobserved costs of participation. Empirically most of these costs have a mutual causality and datasets do not often contain the information needed to disentangle them.

Targeting efficiency is only one among many criteria for assessing public policy. In the case of Oportunidades, the impact of the programme on health and education outcomes has attracted most of the research questions and policy assessment. However, this research focuses on targeting efficiency for three reasons. Firstly, targeting efficiency tells us if targeting instruments have effectively identified the poor, the way the identification is carried out and the reasons behind this objective (Walker, 2005). In section 4 of chapter 2, this research shows that compliance with conditionality in addition to mean-test may determine participation in the programme and targeting efficiency. Secondly, conventional wisdom tells us that targeting efficiency may contribute to improving poverty outcomes of the programme, but as discussed in chapter 6, targeting inefficiency is associated with high impact on

[5] Castañeda, et al (2005) identified key steps to improve targeting efficiency: 'data collection processes, information management, household assessment mechanisms, institutional arrangements (clearly defined) and monitoring and oversight mechanisms' (p. v).

income poverty levels. In the end, measures of targeting efficiency point towards multiple paths to enhance income poverty reduction at local level. Thirdly, targeting efficiency depends on the definition of a target population and ultimately, on the definition of what poverty is. The target population in this research is eligible households according to the MTT. I argue that by understanding the nuances of identifying a target population we can also gain understanding on the many faces poverty can have. And it is important to consider those many faces of poverty when understanding public policy outcomes.

To undertake this research I wrote three empirical chapters and a methodological one. In the section below I describe succinctly what the readers will find in the following chapters. The second chapter presents the puzzle that triggered this thesis: the low take-up of Oportunidades in urban areas. It also introduces the topic of targeting efficiency by drawing on concepts of errors of targeting and categories of targeting, in which this research is based. Chapter 2 links the existence of targeting inefficiency with the existence of multiple objectives of Oportunidades. It also presents the MTT model used by Oportunidades and the data used to replicate it.

Chapter 3 presents and justifies the methodological decisions taken to analyse the problem of participation in Oportunidades, its costs, distributional consequences and targeting efficiency. It also aims to contrast this methodology with other models in order to better understand its strengths and drawbacks. Chapter 3 also highlights how the concepts, particularly the errors and categories of targeting are explicitly addressed by the methodology proposed, which further serves as a link between the concepts and the empirical analysis of this thesis. Chapter 3 also justifies four different methodologies: the use of the sample selection correction method to analyse the distribution of Oportunidades conditional on being eligible; the use of neighbourhood effects to elicit information on compliance with conditionality; the use of the latent variable approach to analyse the costs of participation and the use of the targeting differential to understand targeting efficiency and impact on poverty at local

level. I also compare the last methodologies with other methods in order to understand its strengths and limitations.

Chapter 4 presents the results of analysing participation of Oportunidades with a sample selection correction model, which decomposes participation into two steps. The first one estimates the probability of being eligible according to the means-test model. This information is used to estimate the probability of receiving the cash transfer conditional on the first step. It differs from previous research that had focused on marginal characteristics and average treatment effects to analyse participation. The results indicate that means-testing is not the only consideration in allocating cash transfers. Chapter 4 also analyses compliance with conditionality and behavioural change in terms of human capital investment as determinants of programme's participation. It highlights that the target population should be defined in terms of the means-test, the ability to comply with the conditionality and to attain a behavioural change prone to human capital investment. This is due to the fact that capability of complying with conditionality is not the same for all eligible households. Education conditionality has a high opportunity cost at the higher education level, whereas failure to comply with health conditionality is explained by the existence of monetary and stigmatizing costs attached to health services.

Chapter 4 leads me to conclude that the costs of participation vary for households that are classified as errors of targeting. But these costs are difficult to observe and model with the data available. Therefore, chapter 5 endeavours to portray the cost of participation as a latent variable. By doing so, important assumptions are considered and justified. The results show that the distribution of costs of participation is heterogeneous among eligible households. Costs are higher for households who face barriers to comply with health conditionality.

As discussed in subsequent parts of this research, the means-test is not the only model behind allocation of cash transfers, and it cannot explain targeting outcomes at a local level. Chapter 6 analyses two additional hypotheses to explain the variation of Oportunidades' targeting efficiency at local level. It is a fact that a factor behind targeting inefficiency is the budget constraints. Oportunidades cash transfers are not enough to cover the eligible population. But budget constraints only explain the supply-side of cash transfer allocation. If focused on the demand-side of the programme, I conclude that the ability to encourage behavioural change among recipients and eligible non-recipients is a factor behind the take-up of Oportunidades. Moreover, such behavioural change is less related to local relative prices and labour market and more associated to the costs of participation in the programme. Furthermore, Oportunidades' impact on income poverty is not necessarily associated with targeting efficiency at local level, but more importantly, summary measures of targeting efficiency cannot explain a single path to poverty reduction at local level.

This research concludes that targeting efficiency of Oportunidades in urban areas is explained less by the means-test model and more by whether or not households can comply with the conditionality. The same conditionality explains higher levels of investment among recipients, some of whom are receiving benefit even though they are not entitled to it. Barriers to complying with education and health conditionality seem to affect precisely those households that have fewer incentives to invest in human capital, which should be the main target population of a programme such as Oportunidades. Contrary to conventional wisdom, targeting efficiency is not associated with poverty impact and there is not a single path to achieve impact on poverty at local level. Targeting efficiency is only one among many criteria for assessing public policy. But by understanding targeting efficiency as a problem of the definition of a target population and the means to attain this target population I have gained understanding on the many faces poverty has, and the way they shape the complexity of anti-poverty programmes.

2 FRAMEWORK OF ANALYSIS AND SELECTIVE INTRODUCTION TO IMPORTANT LITERATURE

2.1 Framework of analysis

Conditional Cash Transfers (CCTs) have been championed as the best instruments for antipoverty policies by several international organizations. Many CCTs use Means-Tested Targeting (MTT) mechanisms to select their recipients. This means that eligibility for CCTs is often determined on the basis of income, assuming that the poorest population are in the ones with lowest incomes. MTT was then thought to allocate resources more efficiently and to be more progressive, and it was also thought that MTT was less expensive than universal benefits, i.e. policies open to the entire population without an explicit target group[6].

In reality, there are important limitations to the implementation of targeting. MTT relies heavily on the capacity of the public administration to gather income information, which is not easy to obtain and is prone to measurement errors (Sen, 1995). Moreover, in practice, MTT is often combined with targeting mechanisms other than means-testing, which also makes the process of selection more complex[7]. This complexity places a high demand on the institutional capacity of the public administration and makes it more difficult for beneficiaries to understand and be aware of their eligibility for the programme.

But implementation of targeting is not the only source of problems. The design of the targeting mechanism should be analysed carefully (van Oorschot 1995, Atkinson 1995, Walker 2005). To begin with, policy objectives should be clearly designed and should coherently match the instruments used for selecting their target population. Fine-tuning objectives and instruments has proven to be difficult for CCTs that use

[6] MTT was also thought to solve the problem of relying on the public deficit for funding universal social protection. This was in tune with the Structural Adjustment and neoclassical normative ideas about poverty. A more nuanced analysis would unveil the fact that there is no such as thing as perfect targeted or perfect universal policies. To put it differently, there is always some degree of selectivity of recipients in both targeted and universal policies.

[7] As discussed below, the very conditionality in CCTs represents a targeting instrument additional to means testing, since recipients self-select themselves when deciding to comply with this conditionality.

MTT, since their benefits are not only targeted on income but also conditional on the behaviour of recipients. As Atkinson (1995) explains, conditional cash transfers (CCTs) often have objectives other than handing out money to income-poor households, such as fostering investment in health and education. But a policy that aims to provide health insurance may be better targeted at the health-vulnerable population than at the income-poor population. Therefore, the analysis of targeting efficiency should assess whether targeting instruments have effectively identified their target populations, and if that targeting is compatible with the objective of poverty alleviation (Walker, 2005). It is the task of policy makers to fine-tune targeting objectives with policy design and implementation. Failure to do this may cause targeting inefficiency and, arguably, policy ineffectiveness.

This research concerns the participation in *Oportunidades*, an antipoverty programme implemented in Mexico and based on MTT. I analyse the factors that explain the targeting efficiency of the programme and whether targeting efficiency is associated with poverty alleviation at local level. As Walker (2005) explains, targeting efficiency is only one among many criteria for assessing public policy. I could have looked at other types of efficiency (cost-efficiency or administrative), or at its effectiveness in achieving the policy objective of poverty reduction. I have decided to focus on targeting efficiency for three main reasons. Firstly, targeting efficiency tells us if targeting instruments have effectively identified the poor, the way the identification is carried out and the reasons behind this objective (Walker, 2005). This research shows that lack of compliance with conditionality is associated with non-take-up of the programme. Secondly, conventional wisdom tells us that targeting efficiency may contribute to improving poverty outcomes of the programme. However, this research shows that targeting inefficiency may be associated with high impact on income poverty levels. In the end, measures of targeting efficiency do not explain a particular path to improve impact on income poverty at local level. Thirdly, targeting efficiency depends on the definition of a target population and ultimately, on the definition of what poverty is. I argue that by understanding the nuances of identifying a target

population we can also gain understanding on the many faces poverty can have. And it is important to consider those many faces of poverty when understanding public policy outcomes.

To undertake this research I will draw on concepts derived from public policy analysis presented in section 2.1.2. Section 2.2 aims to make a succinct summary of the empirical research that has examined claims for, and access to, welfare benefits among eligible households in developed countries and for Oportunidades. It also aims to bring to the fore some of the theoretical frameworks that underlie the empirical research on take-up of welfare benefits, as well as to discuss some of the limitations of this kind of analysis. Section 2.3 summarizes the targeting criteria of Oportunidades. Section 2.4 describes the targeting efficiency of Oportunidades in 2008, highlighting the low participation in urban areas, which triggered my interest in pursuing this research. Section 2.5 explores the two definitions of poverty derived from the discussion on targeting criteria and efficiency. Section 2.6 summarises the research questions and objectives. Section 2.7 discusses the relation between policy aims of Oportunidades and the concepts of behavioural change and targeting efficiency. Section 2.8 defines the concept of costs of participation used in chapter 5. Section 2.9 describes the timeframe of this research.

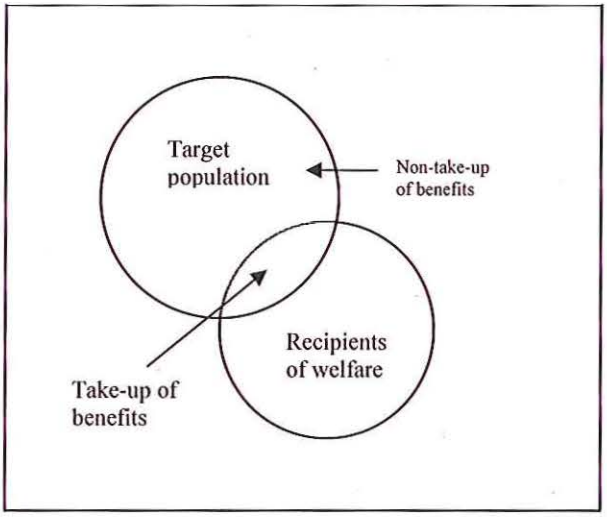
2.1.2 Targeting efficiency of public policies

As Atkinson (1995) explains, MTT is attractive because it is thought to be more efficient than universal benefits to combat poverty. This is because it is assumed that it is better to target the poor population than not to do so, and that MTT is the most progressive instrument through which to do so. In this section I will discuss the difficulties in the design and implementation of MTT that make us question the assumptions on its efficiency. I will also define the concept of targeting efficiency of public policies and identify the categories of targeting efficiency I am concerned with: participation (take-up) and non-participation (non-take-up) of welfare benefits among eligible population, and participation of non-eligible population.

It is a common place to think that MTT transfers, being allocated on the basis of income, are more progressive and efficient at reducing poverty. Atkinson (1995) questions this common place assumption when he reviews policies that succeed or failed to reach their target population with different targeting instruments. He concludes that the distinction between universal and targeting policies is not as sharp as it has been portrayed: universal policies often have some degree of self-selection of recipients and targeted policies may have broader objectives other than income-based selection of recipients.

The rate of participation in public programmes can be seen as a way of assessing the efficiency of MTT (Atkinson, 1995, p.29); and there are at least two types of efficiency. A public policy can achieve vertical targeting efficiency by selecting only people from the 'target' group. Public policies may also achieve horizontal targeting efficiency to the extent to which they select all of the population considered as 'target' group (Weisbrod, 1970 and Walker 2005, p. 180). The following diagram can be used to understand targeting efficiency.

FIGURE 2.1 DIAGRAM FOR ANALYSING TARGETING EFFICIENCY



In a way, vertical efficiency can be interpreted as a measure of how many people are receiving welfare benefits and are not part of the target population. Resources spent

on those recipients would then be characterized as a leakage of the programme, a targeting error. Cornia and Stewart (1995) conceptualized targeting efficiency precisely as a measure of targeting errors. Targeted policies may incur 'excessive coverage errors' when people who receive the programme is not part of the target population. It is also known as type-II error or 'inclusion error'. It is analogous to the concept of vertical efficiency because it is concerned with the extent to which beneficiaries are members of the target population.

Horizontal efficiency involves the coverage of the target population by the policy or programme. This type of efficiency also focuses on the people who, despite being 'targeted', have failed to claim benefits. Policies then may incur in 'failure to reach target population error', also known as type-I error or 'exclusion error', when they do not reach the entire target population. The concept of take-up of benefits is then tightly linked to horizontal efficiency, while the concept of non-take-up of benefits is then linked to the exclusion error. Policy makers that pursue reducing errors of excessive coverage may incur failure-to-reach errors (Cornia and Stewart, 1995)[8].

Cornia and Stewart (1995) argue that, from a pure cost-benefit analysis, policy makers tend to focus on inclusion errors or vertical efficiency to prevent leakage of public resources to 'illegitimate' claimants. Failure to reach target population seems to be neglected, even though it is relevant for analysing the impact of public policies. As discussed below, horizontal efficiency in the case of Oportunidades has been less explored.

The concept of target population is at the core of the definition of targeting efficiency. For the particular case of CCTs, the target population is the so called poor population, although poverty is a contested concept. Income poverty is its most popular definition, but other perspectives on poverty highlight the social exclusion or the capability

[8] For example, in the context of budget constraints, policy makers may be tempted to ration the goods and services provided, by limiting access to information and creating social stigma in order to avoid illegitimate claimants. However, this attempt to discourage illegitimate claimants would also serve to exclude eligible people from claiming. Therefore budget constraints and the pursuit of efficiency in public expenditure and targeting population may be contradictory to objectives of alleviating poverty.

deprivation aspects of the concept (Stewart, *et al*, 2007) Oportunidades defines poverty on income basis but proxy indicators of income are used to carry out the means-test, as it was thought that proxy information would be more difficult to misreport. As a result, the means-test does not identify the same population as the income poverty lines does. This research takes the target or eligible population to be the one identified by the means-test defined in the rules of operation of the programme. I justify this decision in section 3.4 and compare income poor and eligible populations in sections 2.5 and 3.2.6. It is also worth asking whether the population identified by the administrative definition of target population contains the people who are most in need (Walker, 2005), which is a line for further research.

In practical terms, we can classify our population in four categories which will be recalled throughout this thesis as the four categories of targeting efficiency. The first one is the eligible population that receives the Oportunidades cash transfer. In figure 2.1, it is represented by the take-up of benefits group. The second one is the eligible population that do not receive the Oportuniades cash transfer or the errors of exclusion represented as non-take-up of benefits in figure 2.1. The third group is the non-eligible population that receives Oportunidades cash transfers, the so-called inclusion errors. The fourth group is the rest of the population, comprising non-eligible non-recipients of cash transfers.

It cannot be assumed that eligible recipients would promptly participate in welfare benefits, and targeting mechanism such as MTT may incentivise or deter participation. From the design, policies with many policy objectives may find it difficult to define a cohesive target population. As Atkinson (1995) explains, conditional cash transfers (CCTs), specifically to families with children in developing countries, often have objectives other than handing out money to income-poor households. A policy that aims to provide health insurance may not be targeting individual income but health vulnerability. It is the task of policy makers to fine-tune objectives with policy design

and implementation. Failure to do this may cause targeting inefficiency (Atkinson, 1995).

Therefore, the first task that an analysis of targeting efficiency has is to assess whether targeting instruments have effectively identified their target populations, and if that targeting is compatible with the objective of poverty alleviation (Walker, 2005). This is what chapter 6 aims to do. A multiplicity of objectives is at the heart of the design of Oportunidades, it defines income poverty through a combination of proxy indicators. Moreover, it provides cash transfers on the condition that children in households attended school and the cash recipient (mainly children's mothers) attended health talks. The selection criteria to participate in Oportunidades are complex which results, paradoxically, in targeting inefficiency.

Secondly, targeting inefficiency can also result from the barriers to the implementation of policy. These barriers can roughly be accounted for by three constraints: information costs (knowledge about how to claim the benefit), administrative costs (time, transportation costs of conditionality and other costs), and outcome costs (stigma) may deter people from taking-up welfare benefits (Remler, *et al,* 2001).

Therefore, the second task an analysis of targeting efficiency has is to investigate the most important barriers that the target population faces when participating. This research attempts to do so in chapter 4 by proposing two innovative methodologies discussed thoroughly in sections 2 and 3 of chapter 3. Firstly, by looking at the conditional rather than marginal distributions, it acknowledges the contribution of MTT to targeting outcomes and identifies the four categories of targeting efficiency with different probabilities of participating in the programme. Secondly, it also explores the extent to which compliance with conditionality differs among the four categories of targeting and influences targeting efficiency.

Thirdly, as Sen (1995) warns, the analysis of how targeting is implemented should incorporate the social, economic and political context in which they are put into practice[9]. MTT has its own implementation costs and limitations for claimants and administrators. Empirically, most of the barriers explored in chapter 4 have mutual causality and datasets do not often contain the information needed to disentangle them (Brewer, 2003). This research considers that costs of participation in welfare are unobserved and models them using a latent variable approach. Section 5 in chapter 3 justifies the assumptions of the latent variable model and chapter 5 presents the distribution of these costs which it is argued is behind the non-take-up of targeting efficiency.

Given all the limitations of targeted policies, universal policies may seem more appealing. However, universal policies may also face limitations if resources are scarce. Therefore, it is worth acknowledging that analysing targeting efficiency can provide insight into the practical implementation of public policy, and this type of analysis should not exclude policies other than the ones based on MTT.

As mentioned above, targeting efficiency is only one among many criteria for assessing public policy. I decided to focus this research on targeting efficiency due to three main reasons. Firstly, targeting efficiency tells us if targeting instruments have effectively identified the poor, the way the identification is carried out and the reasons behind this objective (Walker, 2005). In section 4.3 and section 4.5.3, this research shows that compliance with conditionality determines participation in the programme and targeting efficiency. Secondly, conventional wisdom tells us that targeting efficiency may contribute to improving poverty outcomes of the programme, but as discussed in chapter 6, targeting inefficiency is associated with high impact on income poverty levels. In the end, measures of targeting efficiency point towards

[9] Additional limitations on the implementation of targeting are produced by the political economy of targeting. MTT may not readily attain the support of the rest of the population that do not benefit from them, or may be difficult to implement in a divided social context. Targeting instruments may also encourage changes in the behaviour of the target population. Costs and barriers may prevent the target population from participating in CCTs, which then limits the efficiency of MTT.

multiple paths to enhance income poverty reduction at local level. Thirdly, targeting efficiency depends on the definition of a target population and ultimately, on the definition of what poverty is. The target population in this research is eligible households according to the MTT. I argue that by understanding the nuances of identifying a target population we can also gain understanding on the many faces poverty can have. And it is important to consider those many faces of poverty when understanding public policy outcomes. In the following section, I describe how targeting is actually implemented in *Oportunidades*, a CCT put into practice in Mexico.

So far the essential concepts of this analysis have been discussed. Section 2.2 surveys important literature that explores participation in CCTs and other antipoverty programmes in developed countries and Mexico. Section 2.3 provides an exploration of the targeting procedures used by Oportunidades. Section 2.4 below presents the disaggregation of targeting efficiency by rural and urban areas for 2008, and the puzzling low participation in urban areas that triggered the interest in this research. Section 2.5 explores the two definitions of poverty derived from the discussion on targeting criteria and efficiency. Section 2.6 summarises the research questions and objectives. Section 2.7 discusses the relation between policy aims of Oportunidades and the concepts of behavioural change and targeting efficiency. Section 2.8 defines the concept of costs of participation used in chapter 5. Section 2.9 describes the timeframe of this research.

2.2 Empirical research on participation in welfare benefits

The majority of research on take-up is based on welfare programmes in the United Kingdom and the United States. This is due to the fact that means-testing has been preferred as targeting instrument in countries with liberal welfare states (van Oorschot, 1995). There is a traditional debate in which welfare systems are portrayed as a choice of either/or universal or targeted policies (Graham, 2002). It is worth noticing that many welfare programmes that are targeted by MTT are cash transfers; there are only

few programmes that deliver public goods and services and that are means-tested. However, it should be noted that the delivery of goods and services, which is sometimes pictured as universal, entails certain way of selectivity of recipients. Meanwhile, targeted policies generally use more than one targeting method. This is interesting because, as discussed in the next section, MTT in Mexico defines entitlement not only for cash transfers but also for education and health services.

Moreover, most of the empirical research on take-up has been carried out using household surveys (Brewer, 2003). There had been fewer attempts to use administrative data sources and procedures for analysing take-up of benefits. Unfortunately, due to the different nature of administrative and survey data, it takes an enormous effort to cross-check both sources of information to find out if there is a coincidence between the definitions of take-up followed by researchers and administrators. A third source of information is the one that beneficiaries have about their own entitlement; and this kind of data is seldom collected and used for studying take-up rates.

In this following section I summarize research that has previously explored factors behind take-up of benefits. Most of the approaches have generally focused on either individual or structural factors. Given the amount of information available at individual level, Keane and Moffitt (1998) argue that individual characteristics that determine eligibility explain 90% of benefit allocation. Given little proportion of allocation remains unexplained, it is difficult that structural factors, such as costs of participation, contribute significantly to explaining benefit allocation.

Yet, this research understands that some individual characteristics are also determined beyond the individual realm of action. Eligibility and, more fundamentally, compliance with conditionality, are determined by individual decision making but also by administrative features, such as the means-test model or the presence of school and health centres in the communities. It also considers that individual agency determines how such institutional arrangements work for each individual. In this

sense, this research is placed nearer to the Behavioural-Structural-Scheme models presented in section 2.2.4, in which the interaction between structural and behavioural factors is analysed on the setting of the particular scheme that is being studied.

In general, eligible households are assumed to have similar characteristics, based on the eligibility score. Yet, eligible non-recipients find it more difficult to comply with conditionality of the programme, which may exclude them from the programme; evidence is provided in section 4.3. In this sense, this research is placed nearer to the Behavioural-Structural-Scheme models presented in section 2.2.4, in which the interaction between structural and behavioural factors is analysed on the setting of the particular scheme that is being studied. As it is argued in section 3.2.4, Van Oorschot's model (1995) considers behaviour and structural factors in a particular context is a starting point of this research on participation rates of Oportunidades. This research acknowledges that differences in participation may be due to individual preferences but may also be due to structural factors that facilitate compliance for some eligible households. It also argues that those eligible non-claimants face the highest costs of not investing in human capital. Given the structure of benefits, individuals still shape their participation in policy.

TABLE 2.1 RESEARCH THAT HAS ESTIMATED TAKE-UP OF MEANS-TESTED WELFARE POLICIES

Authors	Country and year	Source of information	Benefit	Estimated take-up
Kerr (1982)	UK 1979	Random sample of pensioners by the DHSS and interviews	Supplementary Pensions	90
Moffitt (1983)	USA 1976	MPSID	Aid to Families with Dependent Children (AFDC)	45
Blundell, Fry and Walker (1988)	UK 1985	Family Expenditure Survey	Standard Housing Benefit	50-72% rent
Noble, *et al*, (1992)	UK 1988-1989.	House and Community Charge Benefits dataset	Family Credit	62-86
Fry and Stark (1993)	Between 1984 and 1990 in the UK	Family Expenditure Survey	Housing Benefit Supplementary Benefit Family Income Supplement Income Support Family Credit	72 84 43 77 34
Van Oorschot (1995)	Netherlands, 1975-1981 Germany 1976-1981	National Survey National Survey	Housing Benefits Sozialhilfe It also includes secondary data from other benefits.	45-76 36-79
Duclos (1997)	UK 1985	Family Expenditure Survey	Supplementary Benefit	71 (80 correcting for measurement errors)
Daponte, Sanders and Taylor (1998)	USA 1993	Food Distribution Research Program	Food Stamps Program	75
Nesbitt and Neary (2001)	UK 2001	89 qualitative interviews with Pakistani and Bangladeshi minorities and white men.	State Earnings-Related Pension Scheme (SERPS) or occupational pension	Minorities have lower take-up of pension scheme due to intergenerational solidarity
Hancock, *et al*, (2004)	UK 1997and	Family Resources Survey	Income Support	64 for at least one

	2000		Housing Benefit Council Tax Benefit	of the benefits.
Hirst (2005)	UK 1994	Benefits Agency database	Disability living allowance	65
Dornan (2006)	UK	Family Resources Survey	Minimum Income Guarantee	Caseload (survey data) 58.6 Expenditure (administrative data) 76.7
Brewer, *et al*, (2006)	UK 1995-2003	Family Resources Survey	Working Families' Tax Credit	62-74
Zantomio, Pudney and Hancock (2010)	UK 2000-2001	Family Resources Survey	Minimum Income Guarantee	68-76
Cornia and Stewart (1995)	Mexico 1983		Urban Tortilla (Food ration) Food supplementation Urban milk programme	27% 24% 44%
Coady (2001)	Mexico 1997	Socioeconomic Characteristics Household Survey (*ENCASEH*)	Progresa-Oportunidades	75%
Gutierrez, Bertozzi and Gertler (2003)	Mexico 2001-02	Urban Evaluation Survey	*Oportunidades*	73%
Orozco and Hubert (2005)	Mexico 2003 USA 2002-03 Brazil 2003 Chile 2000	*ENCASEH* Local unified registries World Bank Socioeconomic Characteristics Household Survey *CASEN* 2000	Progresa – Oportunidades TANF Bolsa Escola Cash SUF	60% 52% 27% 27%
Skoufias and Di Maro (2006)	Mexico 1997-99	*ENCASEH*	Progresa-Oportunidades	65%
Angelucci and Attanasio (2009)	Mexico 2003 2004	Urban Evaluation Survey	Oportunidades	51.8% 53.9%
Azevedo and Robles (2009)	Mexico 2006	National Household Survey on Income and Expenditure (*ENIGH*)	Oportunidades	48.4%
Coady and Parker (2009)	Mexico 2002	ENCASEH Urban Survey	Oportunidades	53%

2.2.1 Early behavioural/psychological models of take-up

Corden (1995) highlights the importance of Kerr (1982) and Ritchie and Matthews (1982) early models to explain the claim of benefits. Both research analyses UK cash transfers during the 1970s. Their research was centred on the claimants, analysing the factors behind individual decision making regarding claim of welfare benefits. Kerr (1982) analysed the importance of information available to individuals by drawing a sample of pensioners in the UK and conducting a small experiment. Ritchie and Matthews (1982) accounted also for triggering events beyond individual control that determine welfare take-up and found that awareness of being entitled to rent allowance benefit was also very important.

Some of the behavioural assumptions have been carried out into economic models, which also draw assumptions from rational economic behaviour. These models are the basis of most of the research in take-up, as we shall see from below.

2.2.2 Economic models of welfare take-up

Certain models are based on economic theories and individual or household data, analysed with econometric techniques, to predict take-up. Information at individual level provides a nuance picture of characteristics that are related to income —for instance education, health, housing— and that may also determine and predict take-up of welfare. According to Corden (1995), an econometric approach to welfare take-up has been developed from cost-benefit and rational choice models of analysis. Among the econometric analysis of take-up, Brewer (2003) identifies at least three different categories that vary in complexity, conclusions and implications of their analysis. The first one encompasses research that has modelled the probability of take-up based on information of individual characteristics.

Blundell, *et al* (1988) considered individual characteristics and social factors —such as peer group influences— that were correlated to their decision to claim housing benefits

in the UK in the 1980s. Hancock, *et al* (2004) found that pensioners who take-up different means-tested benefits in the UK in 2002 (income support, housing and council tax benefits) are generally the ones with greater entitlement. Daponte, Sanders and Taylor (1998) randomly selected treatment and control groups that had access to food stamps in the USA and found that information plays a key role in determining take-up.

A second group of economic models tries to understand the joint decision of taking-up welfare and of working. This is because MTT creates incentives for people to change their behaviour and, for instance, reduce the time they spend working for a wage. People may be deterred to participate in the labour market because an extra amount of wage might reduce their eligibility to welfare transfers. Or it may well be that the opportunity cost of time devoted to work has increased.

The type of information required for these models is seldom available: surveys should ask about sources of income, preferences around work, leisure and welfare, awareness of their own eligibility and unsuccessful claims. These studies are difficult to carry out and represent a further step into disentangling the channels through which individual characteristics determine income and take-up. Moffitt (1983) analysed the Aid to Families with Dependent Children (AFDC) in the USA and considered the labour supply of households that claim welfare benefits. He found that there are costs that deter households from claiming benefits[10]. Brewer, *et al* (2006) conducted a similar model for the Working Families' Tax Credit in the UK, and found that the fact of claiming welfare didn't prevent people from increasing their labour supply.

A third group of studies have tried to estimate and correct errors derived from applying general economic models. Errors may occur at the moment of identifying eligibility and entitlement to welfare; or errors can be present in the assessment of administrators, beneficiaries and even researchers themselves. Modelling errors requires more complex information. According to Brewer (2003), Pudney (2003) and Duclos (1997), measurement errors bias take-up estimations especially when using probabilistic rather

[10] He found that those are lump-sum costs that do not increase with increasing benefits.

than linear regression models. This bias may be even greater than that due to missing information.

Duclos (1997) uses a parametric model and administrative datasets to correct for these errors[11]. But modelling errors is not the only way of dealing with them. As Pudney (2003) states, Duclos (1997) model treated all errors in the same way, but errors have different distributions across income. Pudney (2003) suggests checking for consistency on household income information. Another strategy is to use external validation of the analyst's estimations by contrasting them with qualitative information.

It is difficult for econometric models to incorporate political economy factors in the analysis. Surveys contain individual data, models are based on individual assumptions; there is little room for incorporating social variables[12]. An additional drawback of the econometric models is that they tend to focus on demographic or 'objective' information and they seldom consider attitudes and perceptions (Dornan, 2006).

To summarize, many economic models conclude that individual characteristics, correlated with low income, also correlate with take-up of welfare. This is the kind of conclusion we would expect from programmes that use MTT to select recipients. However, as Relmer, *et al* (2001) and Corden (1995) explain, it is difficult to disentangle causality of individual characteristics and take-up. Some factors are closely related to characteristics that are not being observed, which leaves wide room for interpretation. For instance, education may be positively related with take-up. This might be due to the fact that education provides information on eligibility of potential beneficiaries. But it may well be the case that it is related to certain income level that beneficiaries have in order to cover the cost of applying to welfare; or it may be that education is a condition to gain access to the programme. Therefore, analysis of

[11] As discussed below in the methodology section, Duclos (1997) uses a structural equation to address the fact that two different people are assessing entitlement: administrators and analyst. Simultaneity arise when undertaking welfare benefit (dependent variable) is jointly determined by several variables —some of which are already difficult to observe and measure. If we don't have enough information about them, we can model them as part of the error term.

[12] However, Moffitt (1983) acknowledged that social or contextual variables contribute less to explaining uptake than individual variables.

individual characteristics should be complemented with clear analysis of the political economy of MTT.

2.2.3 Administrative - bureaucratic models

Other realm of studies incorporates the interaction between administrative agencies, geographical and social variables, as well as quantitative and qualitative methods. The research of Corden and Craig (1991) was one of the first to consider the extent to which administrative structure determines welfare take-up. They explain that there are informational and process barriers that prevent eligible people from claiming benefits. These barriers are 'information supply, service provision, application forms, subsequent negotiations, administrative links between benefits, accuracy in decision-making, policing the system' (Corden, 1995).

Hirst (2005) takes Corden (1995) as a basis and analyzes claims of disability living allowance and argues that take-up is also determined by local administrators that apply criteria for entitlement in different ways. Hancock, *et al* (2004) analysed take-up of Income Support, Housing Benefit and Council Tax Benefit in the UK and understood that entitlement and the first claim for welfare triggers claims for subsequent benefits. They concluded that an automatic process of entitlement would improve take-up —just as universal benefits might do.

Zantomio, *et al* (2010) analysed the behaviour of pensioners that take-up the Minimum Income Guarantee in the UK, and they complement a traditional econometric analysis with interviews with pensioners and qualitative analysis. They found out that reform increased take-up after controlling for individual characteristics.

Despite the important contribution of bureaucratic models of Corden and Craig (1991), Corden (2005) and Zantonio, *et al* (2010), their analysis has important limitations. The administrative process described is dominated by a rational assessment of cost and benefits. It seems as if perfect rational planning could fine-tune targeting methods and delivering procedures. Recipient agency has no role in deciding whether to take or not

take-up benefits[13]. Nevertheless, as showed more recent research, claimants have agency in deciding whether or not to take-up welfare benefits (Dornan, 2006). Therefore, some other studies have incorporated both structural and psychological determinants of take-up in specific policy schemes.

2.2.4 Behavioural-Structural-Scheme model

Van Oorschot (1995) presents a model that also incorporates the administrative structure of bureaucracy and considers the specific circumstances in which the scheme is put into practice. As Dornan (2006) explains, rules and targeting mechanisms may contribute to encouraging or discouraging take-up in both the whole welfare-state structure and individual behaviour. Another important contribution of Van Oorschot (1995) is to understand that take-up varies across time and space; some households may even go constantly in and off welfare.

In his model, Van Oorschot (1995) combines econometric models of behavioural factors and structural analysis of take-up in West Germany and Netherlands during the 1970s and early 1980s. This eclectic approach considers the agency claimants have when deciding whether to participate in welfare benefits. On the one hand, structural factors (for example targeting mechanism) induce behavioural change of welfare claims. On the other hand, given structure, rules and mechanisms, individuals still shape their participation in public policy through social constructions. Therefore, costs and barriers to welfare in one specific social and political context may also be constructed in different ways by researchers, policy makers and claimants in different contexts. As discussed below, Van Oorschot's model (1995) is a starting point of this research on participation rates of Oportunidades.

If household characteristics and macro administrative features have frequently been *foci* of research, this is not the case for factors at community level. Community

[13] Stigma may be regarded as an administrative mechanism for inducing cost and screening truly entitled claimants. However, the fact that claimants have perceptions towards stigma from welfare is an example that shows the degree of freedom of choice when deciding whether or not to apply to benefits.

characteristics, geographical features and group memberships (i. e. ethnic identity) have been less researched as causes of welfare benefits uptake. Noble *et al* (1992) accounted for cultural differences, welfare rights and socialization of knowledge about entitlement as factors determining claims for welfare benefits. There is contentious evidence when analysing the role of cultural groups and welfare participation. Nesbitt and Neary (2001) stress that minorities (particularly from Asia) in the UK tend to rely more on family assistance than on state benefits. Edin and Harris (1999) argue that black females on welfare in the US have fewer alternatives to welfare benefits because they have fewer networks to rely on in times of hardship, which leads them to fall into welfare dependency.

2.2.5 Limitations of the analysis of targeting efficiency

One important limitation of targeting efficiency analysis is that it does not fully explain policy outcomes. Ravallion (2009) measured the targeting efficiency of *Di Bao*, a conditional cash transfer to the urban poor in China. He found that there was no relationship between measures of targeting efficiency and total impact on reducing poverty. He argues that the argument for improving targeting efficiency as an instrument to fighting poverty is not as straight forward as it has been pictured, and policy outcomes are mediated through aspects of policy design, implementation and social context.

Therefore, better targeting would not immediately translate into poverty reduction, and increasing take-up may not necessarily increase policy effectiveness. The benefit may be too small to have an impact on poverty. Or it may well be that the target population defined by policy makers and administration is not the one truly in need, in which case policy will have a lower impact on poverty. These errors of design should be acknowledged, in addition to the constraints in implementation.

Why is targeting efficiency important for policies? It is important because CCTs does have an impact in dimensions other than income. It is therefore important to understand the impact of take-up —and non-take-up— on social dimensions —health, education,

social networks— (Dornan, 2006). An additional point to take into consideration is, as Dornan (2006) argues, that universal provision (or elimination of means-tested mechanisms) may not necessarily solve the low take-up of benefits because each mechanism has its own dynamics. In the same sense, increasing the amount of benefits may not necessarily improve take-up rates, though it would certainly have an effect in policy outcomes. This is why three spheres of analysis —individual characteristics, structural features and policy design and implementation in a specific context— will be explored.

So far I have presented a basic set of concepts for analysing design and implementation of MTT in order to understand horizontal targeting efficiency. We had also explored the empirical research on take-up carried out mostly in relation to cash transfers in the UK and the USA. Different perspectives have identified different factors behind non-take-up. Some of them have focused on how recipient's behaviour determines take-up; some others have studied structural and political economy constraints. However, all of them have limitations in explaining take-up. Policy objectives and implementations should also be analysed in a specific context. In the next section I will explore the empirical research carried out to understand take-up of Oportunidades, a CCT that uses MTT and that is implemented in Mexico since 1995.

2.2.6 The case of Progresa-Oportunidades

In Mexico during the 1980s, in the light of increasing public deficit and Structural Adjustment Programmes, public expenditure shrunk. MTT was set to replace universal programmes in order to reduce the number of people eligible to cash transfers and public services and, therefore, to cut down the public deficit. Moreover, universal schemes in Mexico had not been successful in reaching the poorest population. MTT was therefore championed as the best instruments for antipoverty policy, arguing they were more efficient at allocating benefits precisely to the lowest income deciles.

Research on how MTT may improve the efficiency of anti-poverty programmes has been conducted by The World Bank and other international organizations (Coady, Grosh and Hoddinott, 2004 and Castañeda, *et al*, 2005)[14]. It has found that MTT are progressive instruments since most of the expenditure goes to the poorest population. And it focused on analysing vertical targeting efficiency, being it the extent to which CCTs beneficiaries are also from the poorest quintiles of the income distribution.

However, extensive experience of MTT in developed countries has pointed out important drawbacks of these schemes. MTT defines poverty in an income dimension and it assumes that, with perfect information, public interventions could meet each of the population needs. But income information is not always easy to assess and is also prone to errors (Sen, 1995). The very complexity of the means-test may deter people from claiming benefits. Or it may well incentivise fraud or change in behaviour in order to comply with a means-test. And although MTT targets people in the income dimension, it is also used to entitle people to public services on dimensions other than income, such as health and education (Atkinson, 1995).

Progresa-Oportunidades, a means-tested CCT, was first designed as an experimental randomized intervention that could provide evidence of its impact in the people 'treated' with the programme. The evidence of its impact on improving health and education outcomes has been its major source of legitimacy. However, it is not yet known what component makes the CCTs so effective. Cash transfers undoubtedly help but conditionality has proved to be an important component in improving health and education, though some limitations still hold[15]. The vertical efficiency of the *Oportunidades* has also been acknowledged (Skoufias and Davis, 2001; Coady and Parker, 2009). Nevertheless, there is a major puzzle in terms of MTT and the increasing targeting efficiency and effectiveness of the programme.

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[14] Castañeda, *et al* identified key steps to improve targeting efficiency: 'data collection processes, information management, household assessment mechanisms, institutional arrangements (clearly defined) and monitoring and oversight mechanisms' (p. v).

[15] Conditionality of the programme improved attendance to preventive health and enrolment in secondary education, but wasn't effective in guaranteeing basic levels of health access and basic education (de Braaw and Hoddinott, 2008; Glerter, 2004).

The rate of participation in Oportunidades is different in rural and urban areas. During a first stage, Oportunidades was implemented in rural areas by randomly selecting 'treatment' and 'control' communities. Almost all the population in those rural communities were eligible to the programme[16]. Due to its randomized nature, entitlement, access and take-up of Oportunidades were not issues in this first stage. Participation rates reached 97% in rural areas. Eventually, the programme was extended to urban areas and its allocation was no longer random but based on the degree of marginalization of communities. Coincidentally, take-up rates of the programme decreased significantly to approximately 50% (Angelucci and Attanasio, 2006; Azevedo and Robles, 2009). This significant change questions the fine-tuning of targeting instruments.

Some research has explored individual characteristics of recipients —the so called demand-side factors— that are related to low take-up of Oportunidades in urban areas. This kind of research focuses on features of policy design: conditionality to invest on health and education and the incentives generated by cash transfers. It assumes that policy design is implemented homogenously and is able to incentivise investment in human capital of households that benefit from cash transfers at the margin. This research argues that by looking at the distribution conditional on eligibility we may understand heterogeneity in conditionality and human capital investment.

Skoufias and Di Maro (2006) analysed the patterns of labour supply and leisure time of people in rural areas and found that it didn't change with the take-up of Oportunidades. Hence, they conclude that conditionality of the programme is fine-tuned and it contributes to reducing poverty by increasing income without decreasing labour supply. On the contrary, Vaz (2009) understood that, while labour supply did not change, leisure time increased among Oportunidades recipients, especially for women in rural areas. In this research I assume that no changes of behaviour have been induced in Oportunidades

[16] At national level, 78% of the population in rural communities were entitled to the programme (Coady, 2006).

recipients in urban areas[17], which is an important assumption for the model as discussed in section 3.2 and 3.3 of chapter 3.

Gutierrez, Bertozzi and Gertler (2003) analyse the different targeting methods of Oportunidades. They found that certain household characteristics are correlated with take-up of the programme: educated-female-headed households with high number of members —especially children— and smaller properties are more likely to claim benefits. Angelucci and Attanasio (2006) found other characteristics that explain take-up of Oportunidades: higher poverty index, fewer assets and rooms per person, receipt of other governmental programmes, with more children of school age, and lower education of head of household. However, they also found that low take-up in urban areas is correlated with children's employment and they conclude that incentives offered to households in urban areas are not enough to take children out of work and put them into schools[18].

The previous research is based on investigating the Local Average Treatment Effect (LATE) of households participating in Oportunidades. Angelucci and Attanasio (2006) are cautious in mentioning that LATE methodology may result in biased estimations if low take-up rates imply that information is gathered only on the ones who are more likely to participate[19]. In fact, as one of the examiners pointed out, LATE identifies an average causal effect that assumes all the population eligible to participate complies with the programme and participates. Its usage is problematic when participation is a decision that individuals make. The limitation is that the choice is not at random, but may be influenced by the design of the programme or its deployment. Some control population may change their behaviour in order to participate while some treatment population may

[17] This assumption could be questioned if households have time to adapt their characteristics to the model. Yet, given the fact I am using a one-year-span survey, I would not be able to understand better behavioural responses. I assume behavioural responses, other than the ones the programme aims, are not present. Attanasio *et al* (2008) argues that behavioural responses for Oportunidades tend to be low or non-existent. See also footnote 36.

[18] Living in an area with high concentration of health centres is also correlated with claim of benefits but this is a structural characteristic and not of the household, as discussed below.

[19] Instrumenting treatment areas randomly selected relaxes the assumption of self-selection assuming that the error term of the estimation will be randomly distributed.

decide not to do it. This would bias the estimation of the average treatment effect on the treated.

Attanasio, *et al* (2008) also recognize that although other types of modelling would be more accurate, LATE framework allows them to find average determinants of participation in the presence of data constraints. Yet, Heckman and Vytlacil (2007a: 4835) argue that treatment effects methods offer limited prospective analysis in comparison with structural methods that make strong assumptions on individual behaviour which can be used for policies simulation and forecasting. In the light of possible selection bias, I propose a different method to estimate participation in Oportunidades, as discussed in chapter 3.

Administrative and structural features —the so-called supply-side factors— have also been explored. Skoufias and Davis (2001) conclude that MTT is better suited to identify the poorest people than other programmes, but they are sceptical about the cost of the targeting method that are high and may deter people from taking-up the program. Orozco and Hubert (2005), based on Gutierrez, *et al* estimations[20], confirm a high non-take-up rate but highlight implementation processes that determine target efficiency such as data collection, information management, household assessment mechanisms, institutional arrangements and monitoring and oversight mechanisms.

Azevedo and Robles (2009) argue that the mean-test —based on statistical analysis of income that has changed significantly after it was firstly determined with household information of 1995— is not accurate any longer and could be improved to increase take-up. Coady and Parker (2009) divided the claim process in three steps: knowledge of the programme, application to the programme, and acceptance to the programme. They conclude that MTT —or the acceptance step to the programme— is the less effective

[20] Gutierrez, Bertozzi and Gertler (2003) also considered structural determinants of take-up. They found that MTT is more difficult in localities with larger population, larger poverty headcount and deeper poverty. In fact, MTT is improved when mixed with other targeting methods —geographic and on-demand— in urban areas

method to identify households near to the poverty line, yet MTT is successful to identify the poorest households.

Demand and supply side factors have been explored reaching the conclusion that both play a role in take-up. This research also recovers demand and supply, behavioural and structural factors and considers the specific way in which Oportunidades is implemented. It argues that structural factors prevent behavioural change among eligible non-recipients of welfare. As discussed in section 4 of chapter 3, eligible non-recipients find it more difficult to comply with conditionality of the programme, which may exclude them. And chapter 5 argues that they also face the highest costs of not investing in human capital. Yet, some eligible households decide acknowledge administrative barriers and still take-up the benefit and comply with conditionality, while others do not. Given the structure of benefits, individuals still shape their participation in the policy. Van Oorschot's model (1995) is a starting point of this research on participation rates of Oportunidades.

This section aimed to show that Oportunidades is part of an international surge of MTT and the aspects of Oportunidades this research is concerned with. It also presented a succinct account of some earlier explorations of Oportunidades' targeting efficiency. The following sections describe the nature of the problem and the empirical facts on participation that motivated this research.

2.3 How does Oportunidades target its recipients?

Oportunidades' target population is households living under extremely poor conditions. Oportunidades selects its beneficiaries in at least three stages. The programme firstly uses geographical targeting to select treatment areas, defined as highly marginalized areas that yet had access to health centres and schools, so the recipients can comply with the conditionality[21]. Secondly, the programme selects poor households by proxy means-

[21] This also implies that extremely isolated areas that do not have access to these basic services, which are arguably extremely poor, are also excluded from the programme. When the programme was first implemented in rural areas, treatment and control areas were selected randomly. However, as it expanded

testing targeting. Assessment of household eligibility was more difficult and costly to carry out in urban areas. Unlike rural localities, urban eligible households have to make a claim for Oportunidades in the local verification centre[22]. This certainly implies a more important role of self-selection of households into verification of their eligibility. Household who did not think themselves as eligible or simply did not know about Oportunidades were less likely to claim the benefit. To overcome some of these problems, there was arguably sufficient advertisement in more deprived urban blocks[23]. If households were found to be eligible at the Oportunidades centre, officials would then corroborate the information given to them *in situ*. After being enrolled into the programme, eligibility verification was carried out every three years.

Oportunidades proxy means-test does not gather income information but it is based on proxy indicators that are thought to better capture income levels than self-reported income information[24]. The proxy indicators chosen were the ones that, using a discriminant methodology, could maximize the distance between the group under the poverty line and above it. This methodology was used for both rural and urban areas from 1997 to 2008[25]; it gives higher eligibility score to rural areas. In 2009, new variables were incorporated in the means-test and in 2010 two different models, one for rural and one for urban areas replaced the previous one in order to reflect differences in income standards. Until 2008, the poverty profile of Oportunidades comprised households with the following characteristics:

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into urban areas, coverage has expanded to virtually all Mexico. Only 10 municipalities located in Mexico City and 1 located in Guadalajara did not receive it, presumably because of their affluence.

[22] Households in eligible rural communities were assessed almost universally: a census was carried out in each community and means-test scores determined eligibility. In order to check upon means-test, the community was called into an assembly where the list of beneficiaries could be amended if the community decided that there were errors of exclusion and inclusion. Universal assessment and participation of the community were more difficult and costly to carry out in urban areas. Coady and Parker (2009) find out less awareness of Oportunidades among eligible urban households (37.68%) compared to rural ones (74.52%).

[23] Cars were driven around while loud speakers on top of them were publicising the programme.

[24] Interviewees have incentives to lie about their income or expenditure if eligibility to cash transfers is at stake. Housing conditions and demographic information is arguably more difficult to misreport (Levy and Rodriguez, 2004)

[25] Gutierrez, Bertozzi and Gertler (2003) and Coady and Parker (2009)

TABLE 2.2 INDICATORS USED TO DETERMINE ELIGIBILITY TO *OPORTUNIDADES*

Variable	Weight
Constant	-1.579
Overcrowding: more than 2.5 persons per room	0.139
Demographic dependency: number of younger than 17 and older than 64 years old divided by the number of people between 17 and 63 years old.	0.176
Female head of household	-0.020
Households with children aged 0-11	0.255
Age of the household head	0.005
No social security benefits	0.475
Head of household with 0 years of education	0.380
Head of household with 1-5 years of education	0.201
Dwelling with shared or no bathroom	0.415
Dwelling with unshared bathroom w/ no water connection	0.220
Dwelling with dirt floor	0.475
Household without gas or electrical stove	0.761
Household without refrigerator	0.507
Household without washing machine	0.127
Household without car or truck	0.159
Dwelling rural area	0.653
14 different regional variables	

Sources: Oportunidades (2010) and Azevedo and Robles (2009), Annex 3.

The cut-off point is 0.69 and households above that threshold are considered eligible. When Oportunidades was created (and its name was Progresa), there were around 40 different models to assess eligibility which varied depending on the regional context (Gonzalez-Flores, *et al,* 2012). With the expansion of the programme, Oportunidades model was unified in urban and rural areas. Yet, it acknowledges that income poverty is not measured in the same way in different regions as the relative prices of food and labour change according to the context. Therefore, the eligibility score assigns heavier weight to households in rural areas and in poorer regions. This is further discussed in chapter 6.

2.4 Targeting efficiency of Oportunidades in 2008

Following 2008 methodology, I identify households in the ENIGH 2008 that received cash transfers from Oportunidades between February and October 2008, and the characteristics that made them eligible. The following table presents a comparison of the figures obtained and the estimation of Azevedo and Robles (2009) and Oportunidades (2010). Figures are roughly similar, which is reassuring for this analysis.

TABLE 2.3 DESCRIPTIVE STATISTICS (MEANS) OF OPORTUNIDADES CURRENT TARGETING MODEL

	Coefficients for each indicator at national level		Mean for each indicator (for urban areas only)		
	Oportunidades[a]	2008[b]	2008[b]	2006[c]	2001[c]
Age of head of household	0.2313	0.24	47.65	45.2	46.5
Female head of household (%)	-0.0037	-0.005	26	19.6	26.1
Households with children aged 0-11 (%)	0.2823	0.24	88	97.2	85.9
Head of household with 0 years of education (%)		0.04	7	7.9	5.6
Head of household with 1 to 5 years of education (%)		0.04	17	16.7	13.9
Addition of the two rows above	0.1011	0.08			
Does not have access/right to medical service (%)		0.54	24	43.4	44.7
Does not have access/right to social security (%)	0.2673	0.23	41		
Demographic dependency (%)	0.1350	0.147	67	72	69.3
Overcrowding indicator: # of household members/# of rooms	0.2565	0.0114	1.2	2.2	1.6
Dwelling with dirt floor (%)	0.0456	0.034	1.4	2.4	2.5
Dwelling with shared or no bathroom (%)	0.1032	0.05	7.7	7	5.4
Dwelling with unshared bathroom without water connection (%)		0.06	18.7	14.4	15.8
Addition of the two rows above	0.1032	0.115			
Household without car or truck (%)	0.1067	0.09	54	60.2	52.3
Household without gas or electric stove (%)	0.0893	0.08	5	3	5
Household without refrigerator (%)	0.1319	0.09	12	14.2	11.2
Household without washing machine (%)	0.0595	0.06	41	34.3	24.7
Rural areas	0.141	0.1347	0.2159		
Regions	-0.3769	-0.38479			

Sample size 26, 706, 397 households
Sources:
a) Oportunidades (2010) at national level.
b) Own calculations based on ENIGH 2008 and Oportunidades' methodology.
c) Azevedo and Robles (2009).

Yet, we confirm, as Azevedo and Robles (2009) did that some of the demographic characteristics of Oportunidades eligibility profile have changed over the years. The proportion of head of households with 1 to 5 years of education has increased, but so has the proportion of non-educated head of households. Access to medical service has improved but not the access to social security. Some living standards such as overcrowding, demographic dependency, solid floor or access to water have improved while others have worsened. As I discuss in section 5 of this chapter, the change in the eligibility profile may have eroded the statistical relation between income poverty and eligibility. This erosion is one of the causes of a mismatch between the cash transfer

allocation and income poverty headcount at local level. I argue that this mismatch is also caused by the multiple objectives Oportunidades has: to alleviate income poverty and to promote investment in human capital. And I also argue that this mismatch is correlated with targeting efficiency outcomes at local level in section 4 of chapter 6.

Returning to our concepts of targeting efficiency, we can disaggregate the 2008 Mexican population into our four categories of targeting: eligible households who receive Oportunidades cash transfers, eligible non-recipents, non-eligible households who receive Oportunidades cash transfers and non-eligible non-recipients. This information is presented in tables 2.4 and 2.5

TABLE 2.4 HOUSEHOLDS RECEIVING OPORTUNIDADES CASH TRANSFERS BY ELIGIBILITY, COLUMN PERCENTAGES.

	Eligible	Non- eligible
Oportunidades	54.34%	7.01%
No Oportunidades	45.66%	92.99%
Total number of households	5,917,460	22,545,552

Sample size: 28,463,012 households
Source: calculations based on ENIGH 2008 and governmental methodology to measure poverty.

TABLE 2.5 HOUSEHOLDS RECEIVING OPORTUNIDADES CASH TRANSFERS BY ELIGIBILITY, ROW PERCENTAGES.

	Eligible	Non-eligible
Oportunidades	67.05%	32.95%
No Oportunidades	11.42%	88.89%
Proportion of households	20.8%	79.2%

Sample size: 28,463,012 households
Source: calculations based on ENIGH 2008 and governmental methodology to measure poverty.

These figures are disaggregated into rural and urban areas in the following tables:

TABLE 2.6 RURAL HOUSEHOLDS RECEIVING OPORTUNIDADES BY ELIGIBILITY, COLUMN PERCENTAGES.

	Eligible	Non- eligible
Oportunidades	65.39%	31.87%
No Oportunidades	34.61%	68.13%
Total number of households	3,931,801	2,280,557

Sample size: 6,212,358 households
Source: calculations based on ENIGH 2008 and governmental methodology to measure poverty.

TABLE 2.7 RURAL HOUSEHOLDS RECEIVING OPORTUNIDADES BY ELIGIBILITY, ROW PERCENTAGES.

	Eligible	Non-eligible
Oportunidades	77.97%	22.03%
No Oportunidades	53.31%	46.69%
Proportion of households	63.29%	36.71%

Sample size: 6,212,358 households
Source: calculations based on ENIGH 2008 and governmental methodology to measure poverty.

TABLE 2.8 URBAN HOUSEHOLDS RECEIVING OPORTUNIDADES BY ELIGIBILITY, COLUMN PERCENTAGES.

	Eligible	Non- eligible
Oportunidades	32.46%	4.21%
No Oportunidades	67.54%	95.79%
Total number of households	1,985,349	20,265,305

Sample size: 22,250,654 households
Source: calculations based on ENIGH 2008 and governmental methodology to measure poverty.

TABLE 2.9 URBAN HOUSEHOLDS RECEIVING OPORTUNIDADES BY ELIGIBILITY, ROW PERCENTAGES.

	Eligible	Non-eligible
Oportunidades	43.01%	56.99%
No Oportunidades	6.46%	93.54%
Proportion of households	8.92%	91.08%

Sample size: 22,250,654 households
Source: calculations based on ENIGH 2008 and governmental methodology to measure poverty.

Table 2.4 shows that a significant proportion (45.7%) of eligible people does not receive Oportunidades. In table 5 we can see that, in rural areas, this proportion is lower than the national average, 34.6%. Meanwhile, according to table 2.8, 67.5% of people entitled are not receiving this cash transfer in urban areas. It is my aim to explore the reasons behind these differences. In the next section I describe the research question that surged from this initial motivation and how they shape the objectives of this research.

Self-selection of households in urban areas can be problematic[26] as Coady and Parker (2009) find out there is less awareness of Oportunidades among eligible urban households (37.68%) compared to rural ones (74.52%). Differences in participation between rural and urban areas were so striking that the Oportunidades administration took steps to change the proxy-means-test model in 2009. By 2010 there was a different selectivity model for urban and for rural areas. For the moment, these different models lie outside the scope of this analysis.

2.5 The two faces of poverty

It is now important to understand the relationship between income poverty and eligibility as two different concepts that aim to identify the most deprived households. Oportunidades, despite of targeting income poverty, does define an eligibility model that

[26] As mentioned, eligible household in rural areas were assessed as part of a census-style exercise, whereas eligible urban households were expected to request assessment.

considers others dimensions and an eligibility threshold of 0.69. The concept of eligibility may not necessarily identify the same deprived households as the income poverty does.

The Mexican government has defined three poverty lines to measure income poverty. The lowest is the alimentary poverty line and it identifies households who do not have monthly income per capita enough to attain a basic food basket. The capability poverty line identifies households who cannot attain the basic food basket and additional proportional expenses for education and health services. I must highlight that this poverty line coincides with the rationale of Oportunidades, which is to incentivize investments in food, health and education[27]. The highest poverty line is the patrimony one that identifies households who cannot attain the food basket and proportional expenses to purchase health, education, clothing and transportation. Each poverty line is included in a higher poverty line, so all the alimentary poor households are also capability poor and income poor households. Table 2.10 provides information on household headcount of income poverty disaggregated by their eligibility in the Oportunidades means-test model.

TABLE 2.10 INCOME POVERTY HEADCOUNTS (HOUSEHOLDS) BY ELIGIBILITY

	Alimentary poverty line (%)	Capability poverty line (%)	Patrimony poverty line (%)
Household Headcount	14.5	20	39
Income poor who are eligible at the same time	57	50	39
Non-eligible income poor	42	50	61
Eligible households who are income poor at the same time	40	51	74
Eligible non-income poor	60	49	26

Source: Own calculations based on ENIGH (2008) and Oportunidades (2010)..

Firstly we see that the match between income poverty and eligibility is far from perfect. Roughly half of alimentary or income poor households are also eligible for Oportunidades. But for the highest poverty line, only 39% of income poor households

[27] In fact, the design of Oportunidades in 1995 preceded the establishment of the income poverty lines in 2000. Oportunidades poverty line served as reference for designing the capability poverty line.

are eligible for Oportunidades. And more than 60% of the income poor households are not eligible for Oportunidades. The panel in the second half of the table shows the proportion of eligible households who are income poor at the same time. Forty per cent of eligible households are also income poor, but ¾ of eligible households are considered poor by the highest (patrimony) poverty line. At higher poverty lines, the match between both concepts of deprivation seems to improve. However, there are striking differences if we disaggregate the mismatch in table 2.10 by urban and rural areas, as it is done in table 2.11 below.

Firstly, we note higher headcount of income poverty in rural areas, despite of having a lower poverty line. Secondly, the proportion of income poor households who are eligible at the same time is much higher and consistent across poverty lines in rural areas. Between 80 and 86% of income poor are also eligible in rural areas, and only between 20% and 15% are not eligible. However, only between 34% and 21% of urban income poor households are also eligible for Oportunidades, leaving ineligible in between 1/3 and ¾ of urban income poor households.

But eligibility is not prevalent among rural income poor. As we see from the panel in the second half of the table, the proportion of eligible households who are also income poor hardly varies across urban and rural regions. Most of the variation comes from the increase in the poverty line. At higher poverty lines, we have more eligible households who are also income poor. Interestingly, for the capability poverty line, which coincides with the rationale of Oportunidades, the identification is almost at 50%, and it was also true for the weighted average of urban and rural areas as shown in table 2.10.

TABLE 2.11 INCOME POVERTY HEADCOUNTS (HOUSEHOLDS) BY ELIGIBILITY AND URBAN AND RURAL AREAS

Poverty lines	Alimentary poverty (%)		Capability poverty (%)		Patrimony poverty (%)	
	Urban (875 pesos)	Rural (614 pesos)	Urban (1073 pesos)	Rural (726 pesos)	Urban (1755 pesos)	Rural (1114 pesos)
Headcount	10.28	30	15.6	35.8	34.4	55.4
Income poor and eligible at the same time	34	86	30	84	21	80
Non-eligible income poor	66	14	70	16	78	19
			100%			

Eligible households and income poor at the same time	39	40	53	52	82	70
Eligible non-income poor	61	60	47	47	18	30
			100%			

Source: Own calculations based on ENIGH (2008) and Oportunidades (2010).

The means-test was tuned based on an income poverty line in rural areas, but this does not mean necessarily that it identifies better income poverty in rural areas. In fact, what may be behind is that it is more likely to be eligible in rural areas. The eligibility score assigns 0.653 points to rural areas, which puts rural households nearer to the eligibility threshold of 0.69.

Many reasons could be behind this mismatch, among which we can mention the fact that the demographic composition of poor households may have changed since 1995 when the model was designed. But certainly a more important reason is the fact that the objective of Oportunidades is not only to hand-out cash to poor households. The policy aim is to tackle the intergenerational reproduction of poverty by enhancing investment on human capital, In order to identify low investments in human capital we may need more tools than an income poverty line. When discussing conditionality in chapter 3, section 4 I argue that eligibility may help us to identify households that underinvest in human capital.

Oportunidades has been praised to be the most progressive programme in Mexico, as more than ¾ of the cash transfers are allocated to the first two quintiles of the population (see Chapter 2 and World Bank, 2004 for discussion). Previously I discussed that between 1/3 and 1/5 of income poor households receive Oportunidades cash transfer. This confirms the progressiveness of the programme that the World Bank has acknowledged. However, Oportunidades also excludes eligible income poor households. Between ¼ and 1/5 of income poor households are excluded despite of being eligible. I break down the categories of eligibility and cash receipt by income poverty status in urban and rural areas in table 2.12 below. For displaying the income poverty headcount, I have chosen the capability poverty line. Arguably, the capability poverty line is a good

benchmark for Oportunidades as, conceptually, it defines households without sufficient income to satisfy alimentary, health and education basic needs[28].

We see that most of the rural income poor households are eligible and receive the cash transfers. In contrast, in urban areas most of the income poor households do not receive the cash transfer, although they are not eligible. In urban areas, only a few income poor households receive cash transfers. And the proportion of income poor households excluded is higher in rural areas than in urban ones, this being understandable given the fact that eligibility is higher in rural areas.

TABLE 2.12 INCOME POVERTY HEADCOUNTS (HOUSEHOLDS) BY ELIGIBILITY, CASH TRANSFER AND AREAS

	Alimentary poverty (%)		Capability poverty (%)		Patrimony poverty (%)		Non-income-poor (%)	
Households	Urban	Rural	Urban	Rural	Urban	Rural	Urban	Rural
Eligible recipients	12	60	11	60	7	56	1	24
Eligible non-recipients	22	26	20	25	14	24	2	19
Ineligible recipients	8	6	8	7	7	8	2	15
Ineligible non-recipients	58	8	61	8	72	12	95	58
				100%				

Source: Own calculations based on ENIGH (2008) and Oportunidades (2010).

If exclusion errors are high below the poverty line, it may indicate the difficulties of reaching the income poorest households. Moreover, urban households that receive cash transfer represent 19% of urban capability income poor, while urban excluded households represent 20% of the capability income poor households. A similar case is observed in rural areas, although most of the income poor and eligible households (60%) already receive the programme to begin with. Even if cash transfers from non-eligible recipients were received by eligible recipients, they wouldn't be enough to cover all of the eligible households in either urban or rural areas.

[28] In fact, Oportunidades' poverty line preceded and served as reference for conceptualizing the capability poverty line. Cortes, Banegas and Solis (2007) also use it as a reference for analysing Oportunidades. The line used is 1072.73 MXP for urban areas and 725.7 for rural ones (approximately 52.6 and 35.6 GBP respectively). This gives a poverty headcount of 22.98% of the population and 20.06% of households under the poverty line. The household figures fall into the confidence interval of the figures published in Coneval (2010, p. 84), which are of 25.08% of population and 20.07% of households under the capability poverty line. Differences may be due to the imputation of income which has not been followed in my analysis.

The relationship between income poverty and eligibility human capital deprivation is an interesting line of research. Indeed, the eligibility model of Oportunidades is built upon a particular perspective that relates income, health and education with very practical implications, being the most important the fact of receiving a monthly cash transfer. A research on participation of eligible households is then justified even if limited in its analysis of costs of participation.

As mentioned before, this is research is not precisely about the extent to which income poverty is targeted by the programme. This research focuses instead on allocation among eligible population according to the rules of the programme and the rational of targeting those who underinvest in human capital. Eligible households would greatly benefit if Oportunidades could incentivize a change in their behaviour, although it is not clear whether the programme is succeeding in doing so.

There is a mismatch between income poverty lines, eligibility score and Oportunidades cash transfers. This mismatch occurs when the poverty household headcount is higher than the proportion of households eligible for Oportunidades and getting the cash receipt. This would pose a potentially high local demand that the policy makers cannot solve solely with Oportunidades. This isfurther discussed in section 4 of chapter 6.

2.6 Research questions and research objectives

2.6.1 Modelling of participation in Oportunidades

The first research question investigates for factors behind participation in urban Oportunidades conditional on being eligible. It identifies three subsequent research questions:

- To what extent is means-testing a factor behind participation in urban Oportunidades? Which is translated, for methodological purposes, into: to what extent is there a sample bias in cash recipients due to the eligibility model?

- To what extent is participation in urban Oportunidades associated to the ability to comply with the conditions of the programme under the condition of being eligible?

This thesis explores the factors that determine participation in Oportunidades by focusing on the distribution of cash transfers conditional on being eligible and complying with human capital investment required by the programme. In a way, it differs from previous research that focuses on the marginal characteristics and average treatment effects.

2.6.2 Modelling the latent variable of cost of participation in Oportunidades.

A second question examines whether costs of participation in Oportunidades are particularly affecting eligible households who do not take-up Oportunidades in urban areas. The questions are:

- How do the costs of participation for eligible non-recipients compare to other recipients of cash transfers?
- How do these costs vary with the conditionality of the programme and barriers to public services?
- Can the costs of participation explain the probability of participating in Oportunidades?

In order to address the last questions, a latent variable methodology is applied to analyse the costs among the four different categories of targeting that divides our population. This latent variable is constructed as the expected value of cash receipt conditional on being eligible. The concept of costs if further discussed in section 2.8 below. The analysis concludes that households who do not comply with conditionality face the greatest costs of participating in Oportunidades.

2.6.3 Targeting efficiency and impact on income poverty by geographical areas

A third question explores the variation in targeting efficiency at local level and its contribution to explaining income poverty reduction. Two specific questions are:

- What explains the variation in targeting efficiency at local level?
- Does targeting efficiency explain the impact Oportunidades has in reducing income poverty at local level?

Building on Ravallion's (2009) research, these last couple of questions conclude that increasing targeting efficiency does not necessarily yields higher impact on income poverty. In fact, it is inclusion errors what drives impact on income poverty in the poorest areas. This highlights that there is no clear path to argue that targeting efficiency would lead towards impact on income poverty

In conclusion, the three empirical chapters set out for this research should contribute to the re-evaluation of theories of poverty behind the design of welfare programmes. Inconsistencies in policy objectives and implementation may be limiting targeting efficiency of Oportunidades, without damaging its impact on poverty. The following chapter 3 presents and justifies the methodological decisions taken to analyse the problem of participation in Oportunidades, its costs, distributional consequences and targeting efficiency. Chapter 4 presents the results of analysing participation of Oportunidades with a sample selection model. Chapter 5 endeavours to portray the cost of participation as a latent variable and to show that the distribution of costs of participation is heterogeneous among eligible households. Chapter 6 analyses three different hypotheses to explain the variation of Oportunidades' targeting efficiency at local level and it also shows that targeting efficiency is not necessarily associated with Oportunidades' impact on income poverty at local level.

2.7 Policy aims, behavioural change and targeting efficiency

Oportunidades policy aims are stated in the rules of operation of the programme which state that:

> 'The Programme has as an objective to support families that live in extreme poverty conditions with the aim of increasing the capabilities of their members and of

> expanding the opportunities of wellbeing by means of improving their education, health and food attainment, in addition to contributing to the link public services and development programmes that promote the betterment of their socioeconomic conditions and quality of life' (author's translation from Oportunidades, 2007, 2.1).

The expansion of opportunities of wellbeing occurs in the realms of education, health and food attainment. Oportunidades has an in-kind component that provides access to health, school and food supplements. Yet, the programme provides cash that secures access to food and pays for the opportunity cost of children attending school. The monthly cash transfer that increases according to the school level and gender of children and the number of people aged 65 years or older. Table D1 summarises the schedule of entitlement according to household characteristics.

The programme has two implicit policy aims: one of fighting income poverty in order to improve socioeconomic conditions and one of encouraging investment in human capital in order to prevent poverty by increasing the capabilities of the poorest households. At first sight, these two noble aims would not seem to be at odds. But each of them yields a different perspective on targeting efficiency and on its assessment. In this research I explore the targeting efficiency as a targeting differential, and I contrast this perspective against vertical efficiency of the programme previously explored by The World Bank (2004) and Cortes *et al* (2007).

The concept of targeting efficiency entails a target population which, as stated by Walker (2005), can be defined in different ways. In the case of an antipoverty programme, the target population could arguably be identified as those in the lowest quintiles of the income distribution[29]. In this case, targeting efficiency would refer to a vertical targeting efficiency, as explained in section 2.1.2 above. Traditionally, research on Oportunidades' effectiveness has focused on this type of targeting efficiency, highlighting that the programme in fact reaches the income poorest households (World Bank, 2004), and this is further discussed in section 3.6.2.2 below. However, the target population may also be identified as those who classify as eligible according to the rules

[29] Poverty was defined as income poverty until 2008. The first figures on multidimensional poverty were obtained from a 2008 income and expenditure survey and published in 2009.

of the programme summarised in section 2.3 above. In such case I would be referring to a horizontal targeting efficiency, as discussed in section 2.1.2.

I argue that it is pertinent to define the target population as the eligible households. It is assumed that the eligibility score will identify the population that lacks from access to health, education and assets. Since the programme aims to increase the capability set of the target population, the eligibility score would arguably identify those who would benefit the most from investing on health and education. In section 3.6.2.1 I pose the targeting differential as the most suitable measure of targeting efficiency.

There are policy implications of such methodological decision. If the aim of the programme is to incentivise human capital investment, and the target population is the one identified as eligible by the eligibility score, the efficiency assessment would concentrate on such target population. A different conclusion may be reached if the target population is identified as those in the lowest deciles of the income distribution. If so, targeting efficiency would be assessed from a vertical perspective, as discussed in section 2.1.2. More fundamentally, by explicitly targeting households according to their position in the income ladder, we would be implying that the aim of Oportunidades is income redistribution. And this aim is not expressed in the rules of the programme included above in the first paragraph of section 2.7.

It is pertinent to question the relation between income poverty and investment in human capital. The extent to which those two faces of poverty coincide has led to assume that the main barrier to investing in human capital is the lack of income. In fact, Oportunidades builds upon such assumption as the entitlement increases with the level of education of children. This mechanism assumes that income prevents human capital investment and that the cash hand-out will break the barrier to human capital investment. The cash-transfer is then assumed to generate the behavioural change towards human capital investment.

With the benefit of hindsight, I argue this assumption does not hold for all eligible households. In section 2.5 I provided information on the differences among income poverty and lack of human capital. Some income poor households are also eligible to Oportunidades due to 'human capital poverty' measured by the eligibility score. But some non-income poor households are also identified as human capital poor by the virtue of being eligible. It could be argued that households with lowest levels of human capital have the greatest benefit from Oportunidades. Yet, it is not straightforward to confirm that all income poor lack of human capital. If behavioural change towards human capital investment was prevented only due to the lack of income, we would see a perfect overlap between those two categories. Moreover, the cash-transfer would then be enough to break such underinvestment.

But we did not see the overlap of categories in section 2.5 above. Moreover, evidence in section 2.4 shows that eligible households underinvest in human capital even though they could receive or are receiving a cash transfer to incentivise such investment. I am puzzled by this continued underinvestment. If such eligible households were reached by the programme and induced to invest in human capital, targeting efficiency would certainly improve. In addition to this, the impact of the programme and the achievement of its objectives would arguably be enhanced by virtue of reaching who could benefit the most from the cash transfer.

Given the imperfect overlap of income poverty and human capital underinvestment, there is scepticism on the lack of income being the main reason behind human capital underinvestment. It becomes difficult to assume that income poor families would be readily willing to invest in human capital once they receive the cash transfer. This is why it is important to understand the factors behind the lack of participation in the programme.

Being behavioural change an important element in the policy aims of Oportunidades, it is to be considered in our analysis. As the concept refers to change in behaviour over time, it implicitly incorporates a time element in which behaviour changes from a

previous period to the present. Information at different points in time would ideally be the gold standard to measure such behavioural change. But even with nuanced information of participants' past decision making, the measurement of behavioural change is not as readily as the observed behaviour.

The programme implicitly targets those who have not been able to invest in human capital due to budget constraints. But it is very difficult to distinguish this target population from those who would have invested in human capital without the existence of the cash transfer. De Janvry and Sadoulet (2006) and Baird *et al* (2011) also problematize the concept of conditionality. For them, conditionality should concentrate in inducing behavioural change among households who are less likely to attend school. Then, cash transfers should not pay for what households were already going to invest in human capital without the cash transfer. It is not straightforward to measure such counterfactual of behavioural change, as if you ask to people you may get biased responses (Baird *et al*, 2011).

Even if we observe changes of behaviour in different points in time, this information would not necessarily disclose the fact that some household would have invested in education and health without the cash transfer. I argue that some heuristic device is necessary to elicit such information. In particular, I argue that the group effects model of Manski (1993) can provide such understanding of behavioural change.

Manski (1993) poses the assumption that individuals tend to behave in the same direction as their most proximate group. In this sense, individuals would cluster together according to their level of human capital investment. If households in low human capital cluster show a higher level of human capital investment than their peers, they could have arguably experienced behavioural change. However, if households show similar levels of human capital investment than their peers, it may well be assumed that such households were likely to invest to such human capital levels. Such information is important, as it could contribute to discern those who would have invested in human capital regardless the cash transfer. The efficiency of the cash transfer then lies in the

extent to which we observe an individual behaviour different from that from their peers in the presence of a cash transfer.

The definition of peers is then important. In this research, I propose that the group of peers are the households that inhabit the same primary sampling unit. I have labelled such group as 'the neighbourhood', since the primary sampling unit is defined in the ENIGH 2008 as the houses located in the same block, or the closest 250 households if the block has more than 250. A key assumption of the primary unit of sampling is that households contained in each unit share a similar socioeconomic status. A similar socioeconomic status is the ground for assuming a similar level of human capital investment.

The pertinence of socioeconomic status as a proxy for human capital investment could be questioned as well as the homogeneity of such human capital among socioeconomic groups. I ground my assumption on the fact that the indicators of socioeconomic status are informative of human capital investment. The indicators inform about the quality of housing, access to public health services, education and employment. Other relevant peer references could plausibly exist among groups that share similar ethnicity, kinship, gender or age. Yet, I argue that homogeneity based on socioeconomic status is quite pertinent assumption for identifying the group of peers that have similar investments in health and education. As I have explained before, Oportunidades is built upon the idea that income constraints are behind the lack of investment in health and education.

In this introduction it is important to clearly explain the rationale upon which the usage of neighbourhood's decision is going to be a valid comparison. I have argued above that by looking at neighbours' characteristics I may elicit information on behavioural change. Yet, it is important that such neighbours' decisions are also properly identified. For this, I follow Zeitlin (2011) who has operationalised the measurement of neighbourhood effects. According to Zeitlin (2011), neighbour's decision have to be properly instrumented in order to identify the variation in neighbourhood's take-up that is independent of neighbourhoods' eligibility score.

2.8 The latent variable of costs of participation

The definition of cost of participation in welfare programmes is key to explain the lack of participation. From the Economics perspective, opportunity cost of consuming certain good is measured by the utility forgone that would have arisen in an alternative consumption scenario. In this sense, welfare participation as an economic activity entails an opportunity costs measured in terms of those activities forgone when welfare benefits are claimed. One of the first attempts to measure the cost of welfare participation were Moffitt (1983) and Duclos (1995) who conceptualised it as the administrative procedures claimants go through, or transaction costs (attaining information, eligibility self-assessment, time, transportation costs, among other). While Moffitt (1983) saw these as fixed costs, Duclos (1995) assessed the additional costs of miss-assessment of eligibility. Moffitt (1983) and Blundell, Fry and Walker (1988) focused on the knowing how participation changes proportionally to entitlement.

Hernandez *et al* (2007) distinguish themselves from previous research and put entitlement into context. They challenge previous assessment of welfare costs that assumed rationality and knowledge of participants. They consider self-selection of participants given other economic activities they have and their preferences. In contrast to previous concept of costs as the change in participation with respect to change in entitlement, they conceptualised costs as the net value of benefits or the benefit relative to income. In this sense, lack of participation arises when costs are larger than benefits; whereas participation is experienced when opportunity costs of participation is smaller than benefits.

Hernandez *et al* (2006) consider costs of participation as the compensating variation approach (2006, p. 10), because claim of benefits will be observed only if benefits compensate costs relative to income. Put it differently, if benefits relative to income are lower than the costs of participation, then cost will not be compensated by the net benefits and claim of welfare will not be observed. Hence, the lack of participation also provides information about the costs of welfare benefits. Hernandez *et al* (2006)'s

definition can factor in multiple sources of costs relative to the income and benefits of each household, including transaction costs but also unobserved psychosocial costs (stigma and personal shame of participation) and the costs of compliance with conditions (attending to school and health centres, employment opportunities forgone). This definition also considers a monetary value of costs in relation to entitlement, income and characteristics for each household.

Using a different methodology, Manchester and Mumford (2012) disentangled two different sources of cost: a fixed psychological cost for two different programmes and a time cost that varies according to the conditions set by each programme. Yet, I find it problematic to sharply disentangle both psychological and time costs. For instance, they argue that the hassle of paying for food purchases with coupons is a time cost, but it has also been argued to be a psychological cost in the stigmatising effect of coupons.

In this research, I follow the definition of cost proposed by Hernandez *et al* (2007) as their methodology is suitable for analysis the income information I have. Their approach derives costs from the expected value of cash transfer, and there is no need for explicit valuation of stigma and distaste from welfare on a monetary scale. Put it differently, the costs are implicitly valued, rather than explicitly. Then, this analysis may not disentangle psychological from transaction costs, which is one drawback. I try to compensate it by looking at the correlation between such costs and sources of psychological costs.

Yet, a direct quantification of costs would not be except of problems. Manchester and Mumford (2012) mention that such analysis would entail measurement error and misreporting, and imputing non-participants' costs would still be necessary. As Hernandez *et al* (2007) acknowledge, there is no single agreed method of arriving to such estimation. The method used in this research considers the opportunity costs of participation on Oportunidades as the forgone utility or net benefit of participation relative to household income and context, which vary on individual basis. Participants face lower relative opportunity costs of participation, whereas non-participants face

higher relative opportunity costs. This analysis is informative for further assessment of the cost of Oportunidades and income poverty, which is carried out on chapter 6. Last but not least, this definition of costs is also in best tuned to the aim of considering individual determinants in addition to the administrative barriers and budgetary constraints participation.

2.9 Timeframe of research

The information used to carry out this research is the National Income and Expenditure Household Survey (ENIGH for its acronym in Spanish) in Mexico in 2008. A single cross-section survey data does not contain the richness of multiple observations across time, which is an appealing feature of the administrative data used in multiple impact evaluations of Oportunidades (Coady, Parker and Martinelli, 2009; De Janvry and Sadoulet, 2006). Yet, using the ENIGH 2008 has the advantage of being statistically representative of the entire population, which is fundamental to estimate participation in the programme. Meanwhile, the administrative data that is traditionally used to evaluate Oportunidades is not representative of non-eligible households (Azevedo, et al, 2009 and Angelucci and Attanasio, 2006). This is further discussed in section 4.2 below.

It is important to mention that the methodology used for identifying the target population in rural and urban areas was consistent from 2001 to 2008. In 2009, new variables were incorporated in the means-test. And in 2010 two different models replaced the previous one. Hence, information after 2008 would not be comparable in terms of the eligibility score.

Although the usage of ENIGH 2008 implies a single observation point in time, and this is arguably limited, I sustain that ENIGH 2008 is the best data to explore the estimates of participation in Oportunidades and unveil factors that prevent participation that could not be explored with datasets that were designed to specifically evaluate outcomes among participants of the programme

3 METHODOLOGY CHOSEN TO EXPLORE PARTICIPATION IN OPORTUNIDADES AND ITS TARGETING EFFICIENCY

The objective of this chapter is to exemplify the methodology that is used in the following chapters to assess participation in Oportunidades in urban areas, its costs and distributional consequences. It also aims to contrast this methodology with other simpler models of welfare participation, in order to better understand its strengths and drawbacks. The chapter further serves as a link between the theoretical part and the empirical contributions of this thesis. Last but not least, it outlines policy implications of this analysis, which are further developed in subsequent chapters of this thesis.

In order to avoid the risk of being repetitive, this chapter does not aim to discuss the results of the analysis but to justify methodological decisions and to make explicit its implications for the analysis. In the first section I justify the benefits of changing our analysis from the marginal distribution of Oportunidades cash transfer to the distribution of Oportunidades conditional on being eligible. I argue that looking at the conditional distribution contributes to shedding light on the non-take-up of eligible households, a problem which wouldn't be central to the marginal distribution of cash transfers. In the second section I present the sample selection correction model as the methodology that explicitly considers the distribution of Oportunidades conditional on being eligible. I do so in two steps: the first step models the means-test eligibility, and the second step uses that information to estimate the probability of receiving the cash transfer. The sample selection method also offers the possibility of explicitly analyzing the results in categories of targeting efficiency. Section 3 compares the linear regression and the probit methods with the sample selection correction method in order to understand its strengths and limitations.

In section 4 I discuss the methodology to elicit information about compliance with the conditionality of the programme, as this information is not present in the survey and it is

essential to understand if conditionality is at stake when some eligible households are excluded from the programme. In section 5 I present the methodology of latent variable to analyse the cost of participation, the assumptions behind this methodology and the implications they have for this analysis. Section 6 presents the way in which my empirical results are determined by the different regional scores of the means-test model. This empirical relationship has important implications for the assessment of targeting efficiency, but it is not determining as factors other than means-test influence cash transfers' allocation. I also discuss two different methods to evaluate targeting efficiency and the way in which targeting differential explicitly gives the same weight to the four categories of targeting. This made me to prefer it over the concentration index, discussed in this section.

3.1 The focus on the conditional probability of cash receipt and eligibility.

A first approach to the study of targeting people in poverty is a rather simple table that classifies the households that receive the cash transfer in eligible and not eligible according to the definition of eligibility of the programme. For urban areas, such table gives the following results:

TABLE 3.1 CONTINGENCY TABLE OF ELIGIBILITY AND OPORTUNIDADES CASH RECEIPT FOR HOUSEHOLDS IN URBAN AREAS IN MEXICO

	Non-eligible	Eligible	**Total**
No Oportunidades	19,411,429	1,340,974	20,752,403
Oportunidades cash transfer	853,876	644,375	1,498,251
Total	20,265,305	1,985,349	22,250,654

Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

In the relationship between eligibility and cash receipt we can wonder whether the distribution of these two attributes is independent and, if not, what determines the frequencies in each of the cells of table 3.1. Karl Pearson's Chi-square test tells us that the marginal distributions of both attributes are not independent but are associated[30]. Apart from the marginal distribution of the attributes we could also think of a joint distribution of cash transfers and eligibility, in which the distribution of cash transfers is

[30] The chi-square coefficient is 2800000 and I reject the null hypothesis with a 0.000 level of confidence.

conditional on the distribution of eligible households. I will now discuss both of these different ways of thinking about association.

The traditional linear regression model $y = \alpha + \beta x + \varepsilon$, considers cash transfers a response of the linear association of, let say, eligibility. Using the Ordinary Least Squares method I obtain a coefficient $\beta = 0.282$. This indicates that a change from 0 to 1 in eligibility increases the likelihood of getting the cash transfer in 0.282 times.

However, what is the meaning of 0.282 if a typical households can just observe whether they got the money (meaning 100%) or not (0%)? This is one of the main reasons why we justify the use of other methods that are interpreted as probabilities of receiving the cash transfer. A binary response variable —0 and 1—can be analysed with binary choice models. One of the most basic ones would be a linear probability model, which works particularly well for (0,1) scores.

A probit model assumes a normal distribution in the cumulative distribution function of the probability of getting the cash receipt y*. This model has also inspired the idea that depicting cash receipt as a being (0,1) variable is only an imperfect observation of an underlying continuous latent variable of the probability of getting the cash receipt. From the probit model we obtain a $\beta = 1.27$ meaning that the underlying response on cash receipt increases by 1.27 standard deviations for eligible households.

The marginal effect, that is the change in the probability of cash transfer for a discrete change in the variable entitled, is 0.282, which is equal to our β coefficient estimated by the linear regression. In this sense, both methods are congruent in estimating the marginal effects. Yet, for probit models, the marginal effects have always a reference value at which they are estimated. It is normally at the mean value of the distribution, but it may also be at 0, the median value or any other value the variable eligible takes. In this example, the variable 'eligible' only takes either 0 or 1, but other variables used to

estimate participation in Oportunidades will have different marginal effects depending on the value we consider.

The aim of explaining why some eligible households take up the benefits and others do not fully justify the use of models different from the traditional linear model. If I pose that the problem of non-take-up is conceptually different from ineligible cash receipt, I may gain understanding of the problematic of take-up of welfare benefits. I also benefit from the fact that the categories of conditional probabilities also coincide with the categories of targeting efficiency defined in section 2.1.2. Those are eligible cash receipt, eligible non-take-up of cash, ineligible cash receipt and non-eligible non-take-up of cash. If we focus on conditional probability, the model will acknowledge the last 4 categories and this could be handy for outlying the implication of the model in terms of targeting efficiency. An additional ground for this division is that non-eligible take-up has still an eligibility score which is positive and close to the eligibility threshold, although I discuss the comparability of eligible and ineligible households in section 3of this chapter[31].

It is possible to censor non-eligible households, whether they are recipients or not of Oportunidades. I can assign a value of 0 or missing value to those ineligible claims and run an OLS estimation of a linear regression. If a missing value is assigned, there is a very strong correlation between eligibility and cash receipt, and a linear regression cannot be estimated. If 0 is assigned to non-eligible recipients, I obtain a marginal effect of 0.3246 which is higher than the 0.282 marginal effect of eligibility on the whole uncensored sample. 0.3246 is precisely the proportion of eligible households that get the cash transfer. Yet, it is very difficult to justify why I have 'arbitrarily' assigned 0 to non-eligible claimants. That is an additional reason why I have explored the sample selection correction method. This method acknowledges that we do not observe all possible cash

[31] Among non-eligible claimants, the 50% with highest eligibility score and closer to the eligibility threshold (i. e. the most deprived 50 per cent considering eligibility as the definition of deprivation), have a mean eligibility score of 0.4 when the threshold is 0.69.

transfer allocation because households have to be eligible in order to get the cash transfer. This would mean that the sample of recipients is censored due to eligibility and eligibility is not independent of the distribution of the error term.

Before getting deeper on the sample selection correction method, I would like to exemplify the analysis with indigenous households. Households that mentioned that at least one of their members spoke an indigenous non-Spanish language (from now on indigenous households) represent 6.36% of urban households[32]. Given that this particular group of households have faced limitations on access to income and human capital opportunities for a long time, it is desirable that they are part of the target population. Yet, the Oportunidades means-test does not consider the fact of speaking indigenous language as an attribute to be targeted. Nevertheless, it is expected that this particular group has a high probability of being eligible in the means test. In fact, more than a third (39%) of indigenous urban households are eligible for Oportunidades, when only slightly more than fourth (29%) of eligible urban households are indigenous households.

An OLS model provides a marginal effect of 0.12 for the probability of indigenous urban households to get the cash transfer, after controlling for eligibility[33]. The odds of getting the cash transfer are between 3 and 3.4 times if you live in an indigenous urban household than if you are not, but in this occasion, a linear probability model does not yield the same results. The marginal effect of indigenous households in the probit model is much lower, being 0.096[34]

Table 3.2 below summarizes the frequencies. The two estimations, OLS and probabilistic, are both driven by the large number of non-indigenous households, which

[32] This proportion is higher in rural areas, reaching 22.6% of rural households.
[33] As discussed before, indigenous language and eligibility are correlated. When put together in this model, the marginal effect of eligibility decreases from 0.282 to 0.258.
[34] The R^2 of the OLS is 0.137 whereas the Pseudo-R^2 in the linear models is slightly higher than 0.19.

represent 93.6% of the total number of households. The marginal effect of an OLS regression is driven by the column 'Total (a)' in table 3.2 (shaded with diagonal lines).

TABLE 3.2. OPORTUNIDADES CASH TRANSFER BY ELIGIBILITY AND INDIGENOUS HOUSEHOLDS

			Cash receipt		
		Indigenous Household	**No**	**Yes**	**Total (a)**
Eligible Household	No	**No**	18,597,835	733,456	**19,331,291**
		Yes	813,594	120,420	**934,014**
		Total (b)	19,411,429	853,876	20,265,305
	Yes	**No**	1,091,569	412,234	**1,503,803**
		Yes	249,405	232,141	**481,546**
		Total (c)	1,340,974	644,375	1,985,349

Source: Author's calculations based on ENIGH (2008) and Oportunidades (2010)

Yet, the fact that eligibility and being an indigenous household are correlated results in a smaller marginal contribution of the attribute of being indigenous to cash receipt among indigenous people. It is not surprising to get such a low marginal effect, because after controlling for eligibility, being indigenous has only a mild additional effect. Not only are most of indigenous households eligible, a quarter of them receive the cash transfer.

In order to see the joint effect of being indigenous and eligible household, I censor non-eligible urban households, which has the effect of omitting the cash transfers received by ineligible households. It also has the effect of considering explicitly eligible non-takers of cash transfer. This is the information contained in the last three rows of the table 3.2. I observe that the odds of getting the cash transfer decrease from 3.4 to 2.4 for eligible indigenous urban households, but the marginal effect increases to 0.207. The odds of getting the cash transfer among non-eligible indigenous urban households actually increase from 3.4 to 3.7. This is one of the reasons why a regression using the entire sample of cash recipients yields a smaller marginal effect attributable to being

indigenous, which is even more important among non-eligible households. Moreover, if I focus only on eligible households, and censor non eligible cash receipt, I obtain marginal effects similar to the OLS methodology. When doing so, I cannot isolate a specific effect of eligibility as I am only looking at eligible households. Yet, I captured the effect of being indigenous household conditional on the fact of being eligible to begin with. The marginal effect of being an indigenous household is 0.2 among eligible urban households if estimated with the linear probability models, higher than the 0.12 uncensored OLS estimation. Yet more than a third of eligible indigenous urban household do not receive the cash transfer. Does this mean that the attribute of being indigenous does not explain cash receipt? As we shall discuss latter, by looking only at eligible urban households we can portray the mechanism under which being indigenous influences cash receipt and take up. The sample selection correction methodology does this.

The understanding of the conditional distribution of poverty is not new but it has sometimes been overlooked because its analysis engenders wider confidence intervals. For instance, the methodology of poverty mapping that allows to project survey information onto a census data draws on the conditional distribution of poverty[35]. This can only be done by relying in at least two important assumptions. The first one is what Tarozzi and Deaton (2009) summarize as the measurement of predictors being identical in the survey as in the census. The second assumption is called area homogeneity or conditional independence, which is closely linked to the assumption of programme evaluations. The assumption establishes simply that the conditional distribution of income in the survey area given the household characteristics is the same in a larger census region that contains the survey area. Phrased as it is done in programme

[35] More specifically, it draws on the concept of expected value that is also the basis of the analysis in chapter 5.

evaluation, it establishes that treatment is independent of potential outcomes (Tarozzi and Deaton, 2009).

Randomized Control Trials rely heavily on this assumption to carry out impact evaluation. Given the fact that treatment is allocated randomly, the researcher may assume that the fact of being treated is independent of variables observed and of other unobserved characteristics. Moreover, it is also assumed that the outcome of the treatment is homogenous among the treated, as the only differences among being treated or not is the random assignment of the treatment. Having these assumptions in place, the precise contribution of the treatment on the outcome of the treated can be estimated, conditional on the variation of observed characteristics.

There are grounds to question the homogeneity of treatment and cluster effects. Chapter 3 questions the fact that treatment outcomes are similar for eligible and non-eligible households given the fact that eligibility conditions take-up and conditionality compliance for Oportunidades recipients. Manski (2001) highlight that most programme evaluations focus on justifying the assumption for statistical inference, especially on self-selection on treatment, rather than on the heterogeneity of treatment among treated. He hypothesises that this is done in order to maintain statistical precision of estimates. Interestingly, Tarozzi and Deaton (2009) confirm that area homogeneity assumption is an empirical question and that there might be area heterogeneity in the relationship between predictors and outcomes. In a nutshell, the presence of unobserved heterogeneity across areas tends to underestimates the Mean Squared Forecast Error (MSE), giving as a result large standard errors. In fact, Tarozzi and Deaton (2009) suppose that large standard errors are a reason for avoiding this problematic, coinciding with Manski (2001) in highlighting the prevalence of statistical precision over conservative assessment.

I argue that sample selection correction method is a better approach to the problem of participation in public programmes. Firstly, it allows us to portray the concept of take-up

and non-take-up among eligible households. Secondly, it is consistent with our concept of targeting: it defines different estimations for eligible households who receive cash transfers, for those who do not and for ineligible households who receive them. This is due to the fact that it focuses on the distribution of cash transfers conditional of being eligible. Thirdly, it overcomes some technical problems of using traditional methods. For instance, if I focus on the whole distribution of households, results are driven by the largest group which does not receive the cash transfer. If I censor the sample to eligible households, I may not be considering particular factors affecting non-eligible cash receipt, but I gain insight on the eligible non-receipt. The sample selection methodology allows us to see effects of joint distribution of attributes that determine targeting efficiency. This is a closer perspective to our problem of non-take-up, rather than considering average marginal effects that may fail to consider the non-take-up problem. In the next section I justify the steps of the sample selection method.

3.2 The sample selection or Heckman correction method

In this section I present the sample selection correction model as the methodology that explicitly considers the distribution of Oportunidades conditional on being eligible. In a nutshell, this method consists in two steps. The first step models the probability of being eligible according to the means-test model of Oportunidades. The second step uses this sample of eligible households to estimate the probability of receiving the cash transfer conditional on being eligible. It is based on the assumption that we cannot observe the full distribution of Oportunidades because the cash receipt is conditional on being eligible. I discuss how this assumption is tested.

The existence of sample selection is not the only justification for using this method. The sample selection method also offers the possibility of framing our results with reference to categories of targeting efficiency: eligible receipt, eligible non-receipt of cash transfer, ineligible receipt, and ineligible non-receipt of cash transfer. Then, this methodology

explicitly considers the problem of non-take-up of eligible households, whereas other methods based on marginal distributions would fail to consider explicitly the problematic that is the focus of this research. Being aware of the fact that this is not the only method to approach participation, I present a comparison between these and other methods in section 3.3

I am interested in studying the receipt of cash transfer conditional on being eligible to understand the targeting outcomes of the programme. Awareness, compliance with conditionality and transaction cost entailed by the programme are considered but only after the condition of being eligible. Self-selection or *endogenous sample selection* occurs when the characteristics of claimants are correlated to an independent variable x that, in this case, is a vector of the indicators that determine eligibility[36]. In such a case, the dependent variable of Oportunidades cash receipt is not observed accurately because we only observe recipients who are selected into the treatment and we do not observe other type of recipients that do not have the same characteristics of those who are selected into the treatment. Sample selection can happen on different grounds, in this research I argue that sample selection may occur on the grounds of the characteristics that define eligibility in the means-test targeting model.

James Heckman proposed in 1976 a two-step model that corrects for non-random sample selection. In the first step of the model estimates the probability of being part of the sample for which the outcome is observed. This estimation is based on observed variables and an unobserved inverse mills ratio. The sample selected and the inverse mills ratio is used in a second stage to estimate the probability of observing the outcome of interest. In

[36] Another example of self-selection is when the receipt of the welfare is correlated with the availability of the programme within the region studied (Wooldridge, 2009: 495). Furthermore, self-selection may arise from preferences for work and leisure time. As Brewer (2003) explains, this can be controlled for by modelling a utility function that accounts for income, hours of work, entitlement, and take-up rates. Information about time allocated to labour and leisure is available in the dataset, but labour and leisure preferences may go beyond time allocation and may include availability of work, over and under employment. Therefore, labour preferences are treated as an omitted variable in this model.

the case of this research, I first model whether households are eligible according to the means-test model, and then I use the inverse mills ratio to correct for sample selection in in a second step to estimate the probability of getting the cash transfer conditional on being eligible.

The sample selection correction method considers whether people who claim benefits are actually different from the ones who do not claim. Assuming perfect targeting efficiency, self-selection of recipients should be present on the grounds of the characteristics that make them eligible. This would indicate that it is the means test model which determines both eligibility and allocation of cash transfers. The ones who claim are supposed to be eligible and the ones who do not claim are supposed not to be eligible. I argue it is important to test for sample selection to know the extent to which means test determines allocation of benefits, as well as the importance of other factors behind the lack of take-up and targeting efficiency of Oportunidades.

This methodology was used by Heckman and Smith (2004) to determine welfare participation. They establish at least five stages that determine take up: 1) the probability of being eligible; 2) the probability of being aware of the programme given one is eligible; 3) the probability of applying given one is eligible and aware; 4) the probability of being accepted given one has applied; and 5) the probability of being formally enrolled once one has been accepted into Oportunidades (Heckman and Smith, 2004, p. 251). Formally, each probability is conditional on the probability of the previous step:

$$
\begin{aligned}
&\Pr(\text{participation}) = \Pr(\text{eligibility}=1 \mid x) * \\
&\Pr(\text{awareness}=1 \mid \text{eligibility}=1, x) * \\
&\Pr(\text{application}=1 \mid \text{awareness}=1, \text{eligility}=1, x) * \\
&\Pr(\text{acceptance}=1 \mid \text{application}=1, \text{awareness}=1, \text{eligibility}=1, x) * \\
&\Pr(\text{enrolment}=1 \mid \text{acceptance}=1, \text{application}=1, \\
&\text{awareness}=1, \text{eligibility}=1, x)
\end{aligned}
$$

Ideally, one would want to have information on progression to each of the latter five stages. However, due to limited information, Heckman and Smith (2004) transformed the

5 steps into a 4-step model for its empirical implementation. Coady, Martinelli and Parker (2013) also limited their analysis to a 3-step model according to the information available.

In the case of this research, the dataset used is not administrative but rather a National survey. This meant that the information available on the steps of the programme is further limited. I was only capable of using a two-step model as I only have information available on eligibility and cash receipt. Although limited, this information is on participation of eligible and non-eligible households. The first step is considered to be the eligibility, whereas the second step is considered to be cash receipt or participation. The two steps are linked together by the probability of observing both of them, following the chain rule, as indicated below:

$$\Pr(participation) = \Pr(\Pr(receipt = 1|eligible = 1), \Pr(eligible = 1))$$

$$\Pr(eligibility = 1|X) * \Pr(receipt = 1|\, eligibility = 1, X)$$

In which Pr (eligible=1) is the selection equation or first step and Pr(receipt=1eligible=1) is the allocation equation or second step.

The conditional probability is also linked to our four categories of targeting efficiency mentioned in section 2.4. In practical terms, we classify our population in four categories which will be recalled throughout this thesis as the four categories of targeting efficiency. The first one is the eligible population that receives the Oportunidades cash transfer. In terms of the model, that population is comprised by all eligible households identified in the first step that receive a cash transfer in the second step. The second category of targeting efficiency is comprised by eligible population that do not receive the Oportunidades cash transfer, which is also known as the errors of exclusion or non-take-up of benefits. The third group is comprised by those households who despite of being classified as non-eligible in the first step, they end up receiving an Oportunidades cash transfers, the so-called inclusion errors. The fourth targeting category is the remaining population, comprising non-eligible non-recipients. Hence, the analysis of conditional

probability is a good analytical approach to knowing the factors that determine the probability of cash receipt conditional on households being eligible.

The model defines the population as being comprised of three components:

A: eligible households
B: households that receive Oportunidades.
C: non-eligible households that do not receive Oportunidades.

The majority of Mexican population belongs to group C (more than 4/5 of urban households in Mexico). B comprises eligible and non-eligible recipients. Households in A may or may not get the cash transfer. The model uses A to estimate the probability of being eligible. Then it uses that probability to obtain the inverse mills ratio or correction for the hazard of being selected in the sample for A, B and C. This is included as explanatory variable in the estimation of the probability of cash receipt, in which B have the observed positive outcome. After parameters have been estimated, A, B and C have an expected probability of a positive outcome (getting the cash transfer) based on the parameters of the second step.

The Heckman model is based upon the following assumptions:

a. The joint distribution of the error terms in the selection and conditional equations is normal. This is a strong but necessary assumption as we are using a probit functional form that has a normally distributed density function.
b. The error terms of both equations are independent on the independent variables and there are no problems of collinearity.
c. The variance of the selection equation is normally distributed. This is also needed because the first step applies a probit model, which has a normally distributed density function.

The empirical model of eligibility of households is determined by characteristics that inform about the physical and human capital of the household and the region in where they live. The eligibility model considers that rural areas and less affluent areas in the country should receive a higher eligibility score to increase the number of eligible households in those areas. This empirical characteristic would be supported by the idea

that poor households surrounded by households with similar characteristics would, in principle, have higher chances to be income poor.

$$\Pr(eligible = 1|x_i) = \alpha + \zeta x_i + \xi_i$$

Eligibility is thought to be determined only by means-test, assuming there are no behavioural responses to this model[37]. If I model cash transfer conditional on eligibility,

$$\Pr(y_i = 1|eligible = 1, x_i) = \alpha + \beta_x x_i + u_i$$

According to the rules of operation, cash receipt only occurs when households are eligible. I can think of this problem as a truncated distribution in which we will not observe cash receipt if eligibility is 0. Therefore, bias arises because the mean expected value of error term of getting the cash transfer is not 0, conditional on individual or groups eligibility.

$$corr\ (u_i, \xi_i) \neq 0$$

In order to correct such a bias, I have to estimate eligibility and include an instrumental variable that determines eligibility but does not determine allocation of cash transfer.

$$\Pr(eligible = 1|\ x_i) = \alpha + \zeta x_i + \zeta z_i + \xi_i$$

By instrumenting for z_i, I can think of the unexplained remaining part of eligibility, the error term ξ_i, to follow a standard normal distribution and estimate the inverse Mills ratio. The inverse mills ratio is the ratio of the probability density function to the cumulative distribution function that Heckman (1976) proposes to take account of the selection bias.

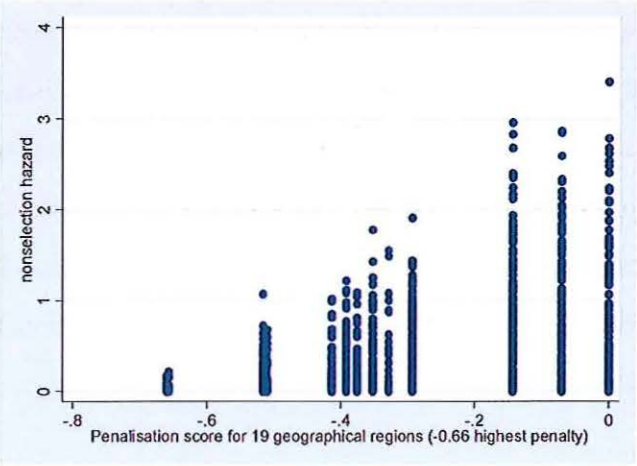
For Heckman, the inverse mills ratio could be thought of as having information on omitted variables that cause the selection bias and that can be used to produce unbiased

[37] This assumption could be questioned if households have time to adapt their characteristics to the model. Yet, given the fact I am using a one-year-span survey, I would not be able to understand better behavioural responses and I rather assume they are not present. Attanasio *et al* (2008) argues that behavioural responses for Oportunidades tend to be low or non-existent.

estimation of cash receipt. Zeitlin (2011) proposes to think of the mills ratio as to convey information of unobserved connections between the eligible households and the ones that get the cash transfer. Eligible households would all share low levels of physical and human capital that make them poor. They would all be likely to receive the conditional cash transfer but we know *apriori* that some of them do not take up the benefit. Therefore, the inverse mills ratio can inform of the omitted variables that may link eligible households and may make them more likely to get the cash transfer.

In the particular case of Oportunidades, the inverse mills ratio varies according to the regions that define eligibility. As discussed in chapter 6 that addresses the geographical differences in the means-test, households get a differentiated eligibility score depending on where they are located. 0.653 points are added to the eligibility score of rural households (2500 or less inhabitants). Yet, households get penalised if they inhabit more affluent regions in which it is statistically more difficult to distinguish between poor and non-poor households. The poorest region in Mexico, central Chiapas, gets no penalisation. Empirically, the inverse mills ratio varies according to this regional penalisation, we can see Figure 3.1 in which the mills ratio shows more variance as the eligibility score increases.

FIGURE 3.1. INVERSE MILLS RATIO BY REGIONAL PENALIZATION IN THE MEANS-TEST MODEL



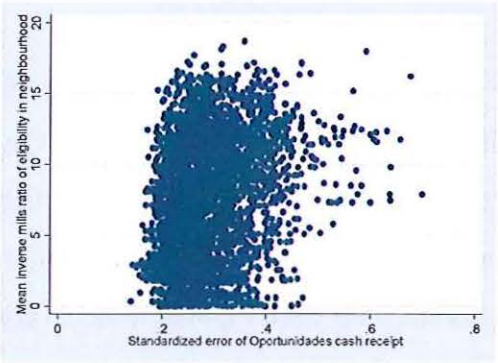
Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

Zeitlin (2011) argues that we can instrument the individual group affiliation with mean inverse mills ratio of the peers of this individual. I use this method to instrument for individual eligibility and it is justified as follows. An instrumental variable z_i would be a variable that determines eligibility but do not determine cash receipt. Therefore, it must not be correlated with the error term of the cash allocation equation u_i. The eligibility of neighbours is thought to be correlated with individual eligibility, as poor households would be clustered together and this is also assumed by the means-test model. If we use the average inverse mills ratio of eligibility for the neighbourhood, we will be conveying information on the link between eligible households and neighbourhoods populated by many eligible households. Then, $z_i = \bar{\lambda}_{j(i)}$. This will also convey information on the link between eligible households in less deprived neighbourhoods. Therefore, each household will be instrumented by the selection hazard of its neighbours, to understand the degree to which their eligibility is explained by the place in which they live in addition to their own characteristics.

This information is thought to serve as an instrument because neighbourhood's eligibility does not determine whether household receive cash transfers or not. Eligible households are entitled to claim cash receipt regardless where they live. It is assumed that individuals cannot influence the neighbours' rate of cash receipt. There is an important caveat, in the sense that over time households may find out the characteristics of their neighbours that make them eligible and recipients. They may also try to adapt their behavior to such characteristics. In such a case, neighbours' characteristics would no longer be independent of allocation of cash transfer. This situation is more plausible in rural areas, where communities are more likely to share information. Yet, it is less likely that this occurs in urban areas, given the complexity, variety and concentration of population in the neighbourhood.

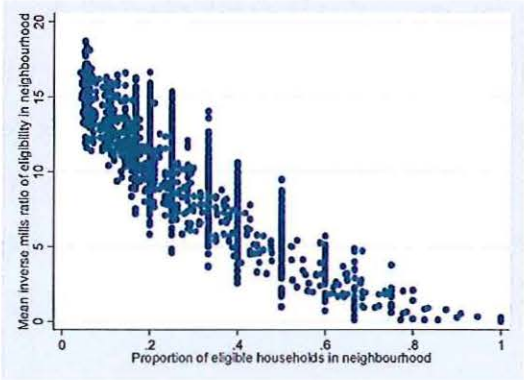
There must not be partial correlation between the error term u_i and z_i the instrument. After running the models I obtain this Figure 3.2 in which we see that mean inverse mills ratio of the neighbours is not correlated with the household error term of the estimation of cash receipt[38]. Figure 3.3 shows that the mean inverse mills ratio of the neighbours is informative of the proportion of eligible households in the neighbourhood, but Figure 3.4 shows the instrument is less informative of the proportion of households in cash receipt in the neighbourhood.

FIGURE 3.2 MEAN INVERSE MILLS RATIO OF NEIGHBOURS AND ERROR TERM OF THE SAMPLE SELECTION EQUATION



Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

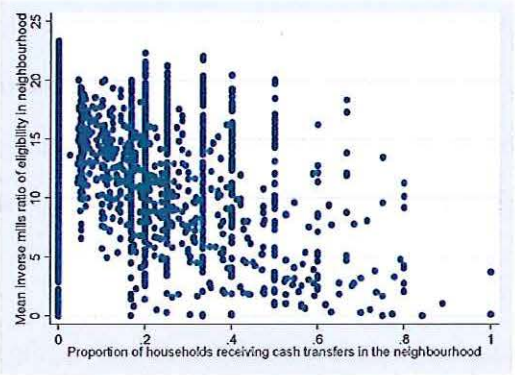
FIGURE 3.3 MEAN INVERSE MILLS RATIO OF NEIGHBOURS AND PROPORTION OF ELIGIBLE HOUSEHOLDS IN NEIGHBOURHOOD



Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

[38] The Pearson correlation coefficient between the instrument and the standard error of the cash receipt equation is 0.516, but its correlation is higher with, for instance, the individual mills ratio (0.711) and the proportions of eligible households (-0.89) and households that got the cash receipt (-0.61).

FIGURE 3. 4 MEAN INVERSE MILLS RATIO OF NEIGHBOURS AND PROPORTION OF HOUSEHOLDS RECEIVING CASH TRANSFERS IN NEIGHBOURHOOD.



Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

This instrument has the characteristics that make it possible to include it in the selection equation but to exclude it from the cash receipt model. In this way, we will be able to derivate household's inverse mills ratios that will act as correction for the selection bias. But I still have to redefine the cash receipt equation. It firstly now includes the individual inverse mills ratio that will correct for selection bias. It should also consider the fact that the probability of cash receipt may be determined by the proportion of the neighbours who get the cash transfer. The mean of the inverse mills ratio of the neighbourhood will be included in the model of individual eligibility. As it has the characteristics of an instrument of eligibility, this mean of inverse mills ratio will also be excluded from the model of individual cash receipt. Specifically, the mean inverse mills ratio of the neighbors is thought to determine eligibility although it does not determine cash receipt. This is due to the means-test model that 'rewards' characteristic of poor households that commonly cluster together (like households without access to public water connection) and that also penalises wealthy regions (as discussed in Chapter 6).

It is important to mention that identification of the selection of eligibility equation is only possible because there is a variation in the eligibility of households beyond the individual characteristics. This variation is introduced by the regional penalisation score. This

means that two identical households living in different regions will have a different eligibility score. There exist variation in eligibility across similar households, and this is what makes it possible the identification of the model. This discontinuity design across regions has been important in the analysis of other cash transfers such as Chile Solidario or the Subsidio Unico Familiar in Chile (Aimor, *et al*, 2010).

The identification of the second step equation requires the presence of an additional instrumental variable that determine selection —and hence correlated with the independent variable that explain eligibility— but that does not determine participation —and hence it is not correlated with the error term of the equation. This instrument can help us to identify the equation by assuming that the error term will be normally distributed disregarding the heterogeneous effect of the selection equation. Wooldridge (2009) and Heckman and Vytlacil (2007a) express that we seldom can test the assumption of lack of correlation with the error term of the equation, and we have to use intuition or introspection to assume that the instrument is not correlated with the explanatory variable.

The instrumental variable for the first equation is the mean inverse mills ratio of the neighbourhood in which each household lives[39]. This instrument is based on the identification strategy of Zeitlin (2011). He explains that the mean inverse mills ratio in the neighbourhood is a source of information of household's own participation that cannot be explained by observed individual characteristics. In Zeitlin (2011), both the inverse mills ratio and the residual of the second equation, convey information about unobserved connections, apart from individual and neighbourhood characteristics, that determine take-up. By using these unobserved connections as an instrument, and assuming they are not related to the selection nor the outcome equations, we can validly

[39] Coady and Parker (2009) use the proportion of eligible households in the block as an instrument because treatment was allocated randomly. In the ENIGH 2008 dataset, I do not have information about the random assignment of treatment.

identify the model. According to Imbens and Wooldridge (2007) the inclusion of this residuals 'control' for the endogeneity of, for example, eligibility and cash receipt.

In my model, however, the residuals proved to be not statistically significant in the individual regression, probably because they provide information similar to the household inverse mills ratio, which is already included as control. The proportion of eligible households in the neighbourhood proved to be an statistically significant control variable for endogeneity and it is also correlated to the residual of this first neighbourhood level model

I now have reconsidered the cash receipt model above to:

$$\Pr(y_i = 1|eligible = 1) = \alpha + \beta_i x_i + \beta_j \bar{x}_j + \rho\lambda_i + u_i$$

Finally, the model also includes the household inverse mills ratio λ_i to correct for selection bias and the u_i error term. λ_i corrects for selection bias given the fact that $corr\ (u_i, \xi_i) \neq 0$. Sample selection bias implies that the error term of the selection equation is correlated with the error term of the participation equation. And this correlation can only exist if a coefficient is different to 0 if $\rho \neq 0$. Therefore, a Wald test on the null hypothesis that $\rho=0$ also test whether there is sample selection bias.

The correlation between the errors could also arise from a lack of identification of the models. If there are observable determinants of the model that are not included, this would cause that the mean value of the error term is different from zero, as the unexplained variance of the model does not occur at random. Lack of identification in a Heckman correction model can also arise from a non-valid exclusion restriction in the selection model. For example, a previous version of this model used the monthly entitlement that households would get as cash receipt; which was used as instrument in Coady and Parker (2009) and Coady, Martinelli and Parker (2013). The argument behind

this variable is that entitlement is determined by demographic characteristics, such as having dependents in the household, that a household wouldn't be able to change in a short period of time. It is thought that if it cannot be changed, it cannot influence cash receipt. However, the argument of this exclusion restriction becomes weaker as we expand the period of time the programme has been implemented. Households may change or adapt their characteristics in order to increase their entitlement. In such a case, entitlement is not a good instrument for eligibility that is not correlated to cash receipt. If this is the only instrument employed, eligibility would lack of identification as a part of it (increasing the dependency rate) would still be explained in part by the household decision of getting the cash receipt. Error of eligibility and cash receipt equation would then be correlated, implying self-selection of sample but also lack of identification.

When I regressed the proportion of Oportunidades cash receipt in neighbourhood –in order to generate the instrument $z_i = \bar{\lambda}_{j(i)}$– I was able to reject the null hypothesis with a p-value 0.0492. This supports the idea of correcting for endogenous selection at neighbourhood level. As Zeitlin (2011), this is a necessary condition for $\bar{\lambda}_{j(i)}$ to be a meaningful instrument.

Yet, when the model includes $z_i = \bar{\lambda}_{j(i)}$ as instrument, self-selection cannot be confirmed. This means that the identification of the first step has changed by including the instrument $z_i = \bar{\lambda}_{j(i)}$. This also means that the identification of the second step changes when explanatory variables beyond the eligibility model have been included. In such a case, the relation between the residuals of the selection and second-step regressions has no particular pattern. This indicates that the first step occurs independently from the second step. This also indicates that coefficients of the selection and allocation models are not related and that there is less evidence of selection bias (Cuddeback, *et al*, 2004).

I could not find an infallible method to select the instrument that best suit the counterfactual of the exclusion restriction: to be a factor that affect selection but not allocation of the cash transfer. Hence, I tested the performance of 7 different variables as instruments of the same model one each time. Firstly, I must mention that I considered the evaluation of instruments in the traditional Heckman selection model, for which the dependent variable is not a dichotomic variable of 0 and 1 but a continuous variable. In this case, the dependent variable is the natural logarithm of the cash transfer received.

$$\mathrm{Ln}(y_i|eligible = 1) = \alpha + \beta_i x_i + \beta_j \bar{x}_j + \beta_\lambda \lambda_i + u_i$$

I chose such model, firstly, because it is also the basis of the expected value of cash receipt discussed below in section 3.5 as the basis of the methodology of chapter 5. In addition of this convenience, the information offered by this model is more powerful as it reports the coefficients rho (ρ), sigma (σ) and beta-lambda (β_λ). As mentioned above, rho indicates the correlation between residuals of the first and the second equation. Sigma is the standard deviation of the residuals of the model, that is, the difference unexplained by the predicted value. Beta-lambda is the coefficient by which the inverse mills ratio from the selection equation enters in the second-step equation. And given the fact that the inverse mills ratio is a function of the residuals of the first equation, these three coefficients are related to each other as follows: $\rho\sigma = \beta_\lambda$

The relation is the same in the probit version of the Heckman sample selection model. By definition, the residuals of both first and second step are assumed to be normally distributed. This makes sigma equal to 1, and hence, rho is also equal to the coefficient of the non-selection hazard or mills ratio. To put it differently, rho indicates the extent to which the mills ratio contributes to explaining the unobserved (or residual) variation in the outcome of interest.

Additionally, when the Heckman selection sample is not assumed to have a normal distribution, sigma is positive and may vary. In such a case, I can think of rho as $\rho = \frac{\beta_\lambda}{\sigma}$. In this case, rho is also the ratio of the coefficient beta-lambda on sigma. Then rho is a ratio informative of the extent to which the effect of the inverse mills ratio (beta-lambda) explains the variation in the residuals of the model (sigma). This places rho as a measurement of the fit of the selection equation.

When rho is not different from 0, we could think that the mills ratio does not contribute to explaining the variation of the whole model. In the probit version of sample selection, given the fact that residuals are normalized and sigma is equal to 1, rho and beta-lambda may not be greater than 1 either. Their variation then is low and the extent to which they can explain the variation of the whole model is quite limited. Yet, the variation in the linear version of the Heckman selection equation may be greater than 1, and so rho and beta-lambda. This makes it more plausible to have a significant rho in the latter than in the former model.

Table 3.3 summarizes the main features that I evaluated to consider the explanatory power of each instrumental variable. There are two broad criterions to judge each performance which are summarized as the power to fit the residuals and the lack of explanation of the outcome. The fit of residuals is given by the size and significance of the coefficients rho (ρ), sigma (σ) and beta-lambda (β_λ). The lack of explanation of the outcome is important as the exclusion restriction specifies that a good instrument would determine the selection but not the outcome of the model. Hence I included each instrument in a similar but simpler OLS linear regression and report the coefficients for each of them.

TABLE 3.3 COMPARISON OF THE PERFORMANCE OF INSTRUMENTAL VARIABLES

Instrumental variable	Level of aggregation	Explanation of residuals					Lack of explanation of outcome	
		σ	β_λ	ρ			Coeff. in linear regression	T-test
Mean inverse mills ratio in Oportunidades region	Region	2.28	-0.13	-0.055	0.10		-0.04	0.000
Mean inverse mills ratio in the state	State	2.36	-0.18	-0.076	0.02	**	-0.03	0.001
Mean inverse mills ratio in the municipality	Municipality	2.28	-0.16	-0.072	0.03	**	-0.01	0.072
Mean inverse mills ratio in the neighbourhood	Neighbourhood	2.28	-0.18	-0.077	0.03	**	0.01	0.439
Speaks an indigenous language	Individual	2.28	-0.15	-0.066	0.05	**	0.11	0.003
Utility gain from participation at constant risk ratio (Coady, Martinelli and Parker, 2013)	Individual	2.28	-0.24	-0.103	0.00	***	-0.49	0.000
Natural logarithm of entitlement (Coady and Parker, 2009)	Individual	2.30	-0.13	-0.056	0.10		-0.26	0.000

Source: Author's calculations based on ENIGH (2008) and Oportunidades (2010)

Four of the instrument follow a similar approach to the one presented earlier. They aggregate the inverse mills ratio at different levels, being region of Oportunidades the largest one, state, municipality and neighbourhood. Three additional instruments were constructed at individual level. One determines whether the household speaks an indigenous language. The second resembles the instrument used by Coady, Martinelli and Parker (2013, p. 7). They defined the utility gain from participation, which is a function of the entitlement divided by the household income and assessed at a risk parameter. In this case, I used the simplest risk parameter of 1. The third one is the natural logarithm of the entitlement as used in Coady and Parker (2009).

In the third column of table 3.3 we can see that sigma is very similar for all of them, which speaks about the stability of the model. Moreover, beta-lambda is the largest (-0.24) for the instrumental variable suggested by Coady, Martinelli and Parker (2013), and the second largest beta-lambda is for my preferred instrument at neighbourhood level (-0.18). The rho coefficient is significant in this Heckman model for most of the instrumental variables tested, being again the largest for the utility gain and the second

largest for the mean inverse mills ratio. As mentioned, the rho coefficient indicates not only the correlation of residuals but also the extent to which beta-lambda explains the variation in the model. Finally, as mentioned, I included the variables in a linear regression to understand their contribution to explain the outcome of natural logarithm of cash transfer. I would expect coefficients not different from 0 for these instruments. But only the mean inverse mills ratio at neighborhood and municipality levels have coefficients not different from 0, as the column of t-test value indicates. Those two are the ones who perform as exclusion restriction in this particular model. Hence my choice for the mean inverse mills ratio at neighbourhood level as a preferred instrumental variable.

As a caveat, I must mention that for this linear regression test, and in addition to the inverse mills ratio at household level, I used two additional instrumental variables at neighborhood level, as Zeitlin (2011) suggest, such as the predicted probability of cash receipt in neighbourhood and the residuals of the predicted probability of cash receipt at neighbourhood level.

As discussed before, unobserved factors may determine take-up and assumptions are built on the distribution of such factors. I argue that unobserved information causes selection bias in the sign-up decision. Yet, as Wooldridge (2013) points out, there are at least two sources of unobserved information: the eligibility step and the allocation step. And it is important to understand whether unobserved factors behind eligibility are also correlated with the unobserved information of the allocation step. If such correlation exists, sample selection bias also occurs and the sample selection method should correct for the bias.

However, it may well be that both unobserved factors are independent from each other, in which case the use of sample selection method is not justified. One way in which sources of unobserved information may be independent is if the unobserved information arise from measurement error. In such case, I would observe eligibility in an imperfect way

and the residuals from the eligibility equation would include the measurement error. This measurement error could plausibly be independent from the unobserved information that affects allocation of cash transfer. If so, sample selection bias may not exist.

Apriori, I could understand that measurement error of the eligibility score may be very small. In section 2.3 I have discussed that proxy indicators for eligibility were selected on the virtue of being easily verifiable in order to minimize measurement error. An additional source of measurement error arises from the fact that Oportunidades' administrators have different information that is not available to me and on which they base their eligibility decision. However, I received the model of eligibility selection method from the administrators (Oportunidades, 2010), from which I assume I am modelling eligibility as close to the administrator's procedure as possible.

The use of the Heckman selection model also contributes to test for correlation between both sources of unobserved information. As mentioned in section 3.2, the model includes the household inverse mills ratio λ_i to correct for selection bias. Sample bias would only exists if the correlation between u_i and ξ_i is different from 0, and this correlation is measured by the parameter rho (ρ). If $\rho \neq 0$ then this correlation is plausible and selection bias could be assumed to exists. A Wald test on the null hypothesis that $\rho = 0$ also test for sample selection bias. This Wald test rejects the null hypothesis of the model of expected benefit, as explained in section 5.3, and it is rejected with a p value of 0.06 for the model in section 4.4. We see that there is evidence of correlation between errors, which indicates sample bias.

As explained in section 3.2, the correlation between the errors could also arise from a lack of identification due to a non-valid exclusion restriction in the selection model. For this model, the exclusion restriction is based on the assumption that the instrument is correlated with the unobserved information on eligibility but it is independent (or at least not correlated) with the step of cash receipt. On table 3.3 I presented information on the

validity of exclusion restriction for the instrument used in this model, so I can rule out the lack of identification as a reason for having a rho parameter different from 0. Hence, I assume that this information also contributes to sustain the assumption of sample selection and correlation between unobserved information in both the eligibility and cash receipt steps that goes beyond the existence of measurement error.

I now explore briefly what other simpler models would give as a result and the relationship with the results obtained by the sample selection correction. I aim to justify the use of the Heckman correction methodology on the value added that it has when explaining targeting inefficiency.

3.3 A comparison between the sample selection correction, a probit and an ordinary least square models in estimating Oportunidades' participation.

As discussed above, the existence of sample bias is not the only ground in which I justify the use of sample selection method to study participation in Oportunidades. This method also offers the possibility of framing our results with reference to categories of targeting efficiency: eligible receipt, eligible non-receipt of cash transfer, ineligible receipt, and ineligible non-receipt of cash transfer. This methodology explicitly considers the problem of non-take-up of eligible households, whereas other methods based on marginal distributions would fail to consider explicitly the problematic that is the focus of this research. We can also gain insight on the problem by being explicit on how the sample selection relates to other methods to approach participation. This section analyses the differences between probabilities predicted with different methods to approach participation, arguing that the differences of predicted probabilities are also a useful tool to understand the errors of inclusion and exclusion and, therefore, targeting efficiency.

The sample selection method also offers the possibility of framing our results with reference to categories of targeting efficiency: eligible receipt, eligible non-receipt of

cash transfer, ineligible receipt, and ineligible non-receipt of cash transfer. Then, this methodology explicitly considers the problem of non-take-up of eligible households, whereas other methods based on marginal distributions would fail to consider explicitly the problematic that is the focus of this research. Being aware of the fact that this is not the only method to approach participation, I present a comparison between my preferred method and the linear and probit method in this section. Tables of the models that result from the linear and probit methods can be found in the Appendix A at the end of the thesis.

Analysing differences between probabilities from different methods has proved to be useful in understanding the implications of the following chapters. I discuss the relationship between the differences of probabilities and regional targeting outcomes in section 6 of this chapter. I also discuss the relationship between the differences of probabilities and the latent cost of welfare participation in chapter 5.

Given the fact that the fifth and sixth empirical chapters build upon the methodological decisions in the fourth chapter that have been just discussed, it is important to state how the previous steps lay the foundation for the subsequent methodology and what are the implications and limitations of this kind of analysis.

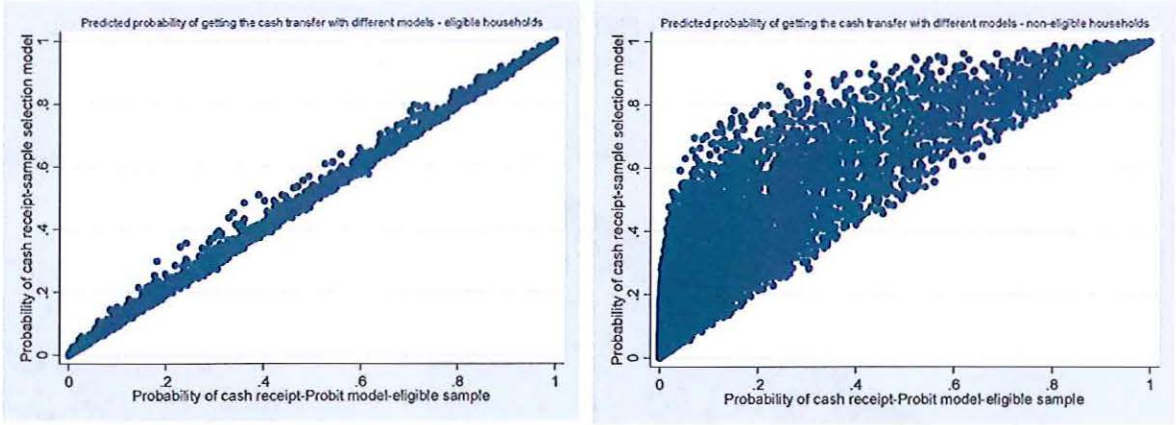
The first difference between the sample selection model and the linear regression model is the functional form. While the linear regression assumes that only the residuals follow a normal distribution, the probit model assumes that ours is a sample of a normal distribution. Given the underlying difference, it may be useful to recall a comparison between a simple probit model and the sample selection correction we have used.

I have discussed that I am looking at the conditional distribution of eligibility and cash transfers. The sample in the estimation plays an important role and different samples will certainly produce different results. A probit regression of urban households' sample of

cash receipt on the same independent variables, including the inverse mills ratio, yields different result. The coefficients and marginal effects of the simpler probit model are much higher than the ones of the sample correction[40]. A probit regression only on eligible urban households yields coefficients similar to the ones of our sample selection correction model. Yet, the marginal effects are not the same.

Figure 3.5 plots the predicted probabilities by the sample selection and the probit model. The panel on the left plots only eligible households, the panel on the right plots only non-eligible households. We see that the predicted probabilities estimated for eligible households are very similar by the two different methods of estimation. Nevertheless, the predicted probabilities differ for non-eligible households. In fact, the sample selection model predicts higher probabilities for almost all ineligible households. This is due to the fact that the probit model which censors non-eligible households is underestimating the probability of getting the cash transfer conditional on not being eligible.

FIGURE 3.5 PREDICTED PROBABILITIES OF SAMPLE SELECTION AND PROBIT MODELS, BY ELIGIBILITY

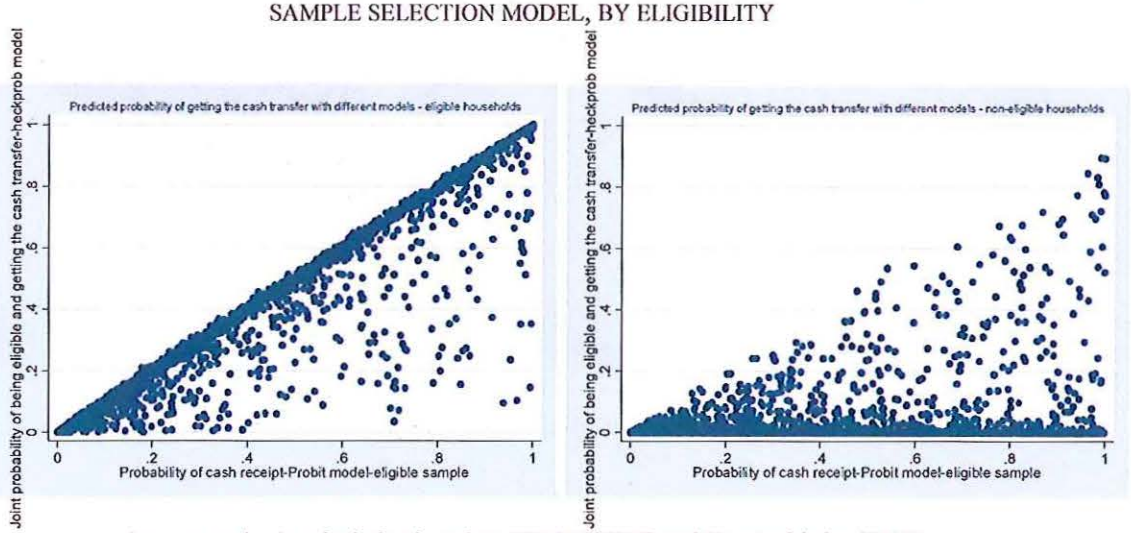


Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

[40] Results are available upon request.

As mentioned before, sample selection is composed by two probabilities: the probability of being eligible and the probability of getting the cash transfer. Below, figure 3.6 plots the results of the second step in which we observe the joint probability of both being eligible and receiving the cash transfer. The left-hand-side panel plots values for eligible households. We see that the majority of eligible households have predicted joint probabilities that are similar in both models. However, there are few eligible households for which the joint probability is lower than the probability the probit model predicts. Those households do not get the cash transfer despite of being eligible, and this is why the joint probability of both events is lower. The probit model, then, overestimates cash receipt in cases when an eligible household is excluded from the programme. The right-hand-side panel plots the same joint probability for non-eligible households. We see that the joint probability of cash receipt and being eligible is even lower for households who are not eligible. Some non-eligible households still get the cash transfer and this is why a probit model estimates a higher probability of getting the cash receipt although they in fact have a lower joint probability because of the non-eligible claim.

FIGURE 3.6 SECOND STEP: JOINT PROBABILITY (ELIGIBLE AND CASH RECEIPT) IN THE SAMPLE SELECTION MODEL, BY ELIGIBILITY



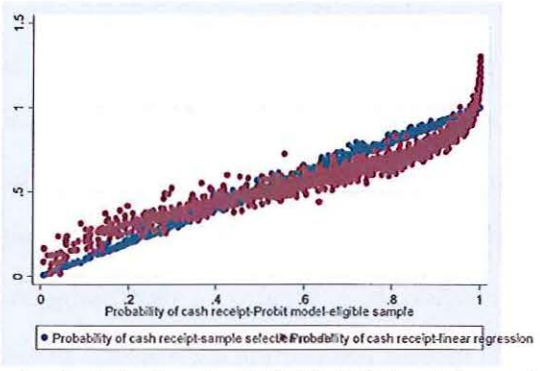
Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

From this plots we can see the link between the sample selection correction and the analysis of targeting errors. By analyzing participation in welfare as a problem of conditional distributions, we can also explicit acknowledge the steps in which cash transfers are allocated. It also explicitly considers the 'errors of targeting' in the different probabilities that are obtained as part of the steps in the model. This can be used as a tool of analysis of targeting efficiency. I argue that, in addition to identify sample selection, the sample selection model offers the possibility of identifying errors of targeting, which would not be distinguished by only considering the marginal distribution of cash transfers. This tool has not been fully exploited in the literature up to now.

As mentioned before, from a probit model we can obtain similar marginal effects as the ones we would obtain with a linear regression estimation. In this case, a linear regression of the sample of urban households of cash receipt on the same independent variables, including the inverse mills ratio, yields different results. In general, marginal probabilities of a simpler linear model are much higher than the ones of the sample correction. Yet, a linear regression that considers a sample of eligible households yields marginal probabilities generally lower to what we get from the sample selection.

In the following figures, we can visually compare the predicted probabilities of the linear model in red color with the marginal probabilities of sample selection correction in blue color. Figure 3.7 plots the values obtained with both methodologies for eligible households. We see that there is little difference between both estimations.

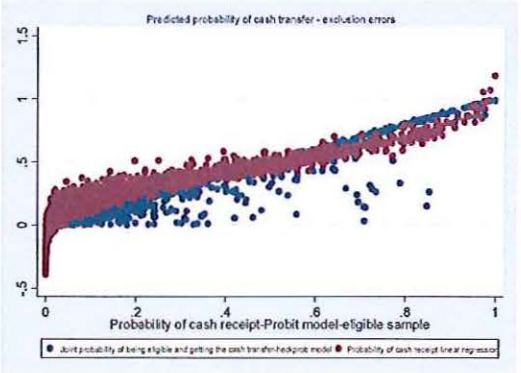
FIGURE 3.7 PREDICTED PROBABILITIES FOR SAMPLE SELECTION AND LINEAR REGRESSION FOR ELIGIBLE HOUSEHOLDS



Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

It is important now to understand precisely how the sample selection methodology shapes the categories of targeting efficiency that we will be conveying in this research. Figure 3.8 plots the values for households in exclusion error, that is, eligible and non-recipient households. We see that predicted probabilities are very similar for the few excluded households that are at the median of both distributions —for practical purposes in the centre of the graph. Yet, excluded households who have a lower probability of getting the cash transfer (at the bottom left corner) have their probability overestimated by the linear method (in red), while the sample selection estimates a lower probability of cash receipt. Similarly, excluded households with higher probability of cash receipt have transfer (at the top right corner) have a slightly lower probability in the linear method (in red).

FIGURE 3.8 PREDICTED PROBABILITIES WITH THE SAMPLE SELECTION MODEL AND THE LINEAR REGRESSION FOR ELIGIBLE NON-RECIPIENTS

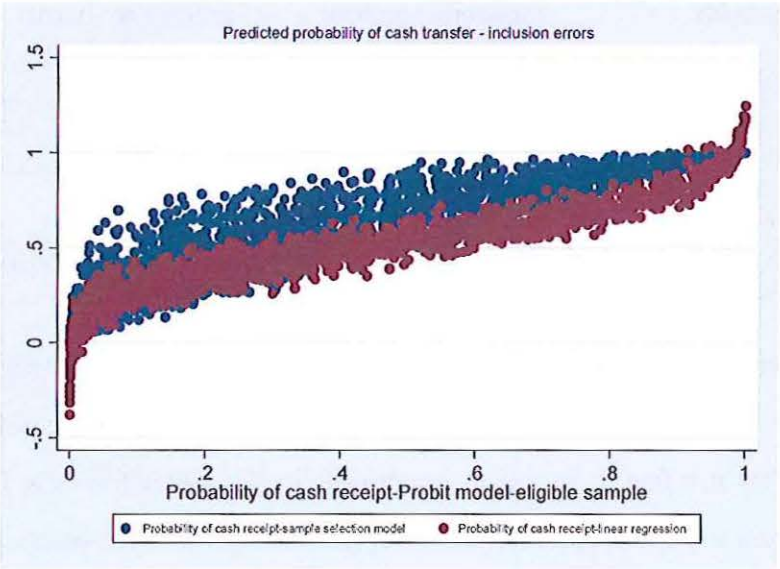


Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

Figure 3.9 plots the predicted probability from sample selection (in blue color) with the linear method (in red color) for households who are inclusion errors, that is, ineligible recipients of cash transfers. We see that the expected probability of getting the cash transfer is higher with the sample selection method than with the linear method.

I confirm the pertinence of using sample selection method as tool for analysing errors of targeting. The expected probability of eligible recipients is not very different if predicted with different methods. Yet, the probabilities of households in targeting errors are larger or smaller when estimated with the sample selection method than with the linear or with the simple probit model. In particular, it has been interesting to use the difference between the linear regression and the sample selection method to understand targeting efficiency. I can understand that the larger the difference, the higher the risk of being incurring in a targeting error.

FIGURE 3.9 PREDICTED PROBABILITIES WITH THE SAMPLE SELECTION MODEL AND THE LINEAR REGRESSION FOR NON-ELIGIBLE RECIPIENTS



Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

Table 3.4 presents the distance or difference between the linear probability predicted using the sample selection correction and the one predicted using the linear model. The

distance differs according to the category of analysis. Firstly, we see that the greatest difference between methods is for the non-recipients, regardless whether they receive the cash transfer or not. The smaller differences are for ineligible recipients. This is coherent with our discussion about the sample selection: this method privileges the understanding of the conditional-on-eligibility distribution rather than on the marginal one. For eligible recipients, difference tends to be positive; meaning that at values above the median, sample selection predicts higher probability than linear method. For eligible excluded households, difference tends to be negative; meaning that at values below the median, sample selection predicts lower probability than linear method. For non-eligible recipients, the difference is mostly negative because they have a lower sample selection probability than a linear one. For ineligible non-claimants the difference is, on average, negative.

TABLE 3.4 DIFFERENCE BETWEEN LINEAR PREDICTED PROBABILITY OF SAMPLE SELECTION MODEL AND OF THE LINEAR MODEL, BY HOUSEHOLD ELIGIBILITY AND CASH RECEIPT

Households	Mean linear probability-sample selection method	Mean linear probability-linear method	Mean difference
Eligible cash-recipients	0.94	0.7	0.24
Ineligible cash recipients	0.35	0.48	-0.12
Eligible non-recipients	-0.79	0.3	-1.1
Non-eligible non-recipients	-1.57	0.04	-1.61

Source: Own calculations based on ENIGH (2008) and Oportunidades (2010). Figures are different between each other with a level of confidence of 0.05.

Although there are arguments to object the use of sample selection correction method in this analysis, I have justified how it contributes to understand the problem of targeting efficiency. Firstly, it frames our results in a probability distributed from 0 to 1. This functional form will be useful to further investigate the existence of a latent variable of cost of participation. Secondly, it incorporates explicitly the conditional distribution of cash transfer and eligibility, which also allow us to test for sample selection of eligible households. Thirdly, its results can be readily use for analyzing the different categories of

targeting efficiency and targeting errors. This could be useful to convey lessons for improving policy implementation. Evidence of sample selection bias is not definitive; but it is only one of the several grounds on which this methodology is justified.

Analysing differences between probabilities from different methods has proved to be useful in understanding the implications of the following chapters. In this section we have discussed how different methods predict different probabilities of participation. Moreover, the differences between predictions are also a useful tool to understand the errors of inclusion and exclusion and, therefore, targeting efficiency. I discuss the relationship between the probabilities of getting the cash transfer and the latent cost of participation in Chapter 5 below. I also discuss the relationship between the cost of participation and targeting outcomes at regional level in Chapter 6. But before discussing other methodologies, I convey the last part of the methodology of Chapter 4: the conditionality of the programme.

3.4 Empirical approach to elicit information about compliance with conditionality and behavioral change induced by the programme.

Targeting efficiency is not only determined by the means test, but also by the conditions the programme imposes on recipients of the cash transfer. Oportunidades may exclude those who do not comply with the conditions[41] and eligible recipients may also exclude themselves from the programme due to the conditionality and behavioural change requested by the programme. It is argued that conditionality improves targeting efficiency through promoting self-selection of the poorest recipients. In the presence of limited information, the administrator may offer cash transfers under condition that imposes costs only affordable by those whose relative benefit is higher. Whether health and education conditionality of Oportunidades increases transaction costs are empirical questions that are posed to be addressed in this research.

[41] Localities that are far away from a school or a health centre are excluded from the programme.

A rigorous approach to conditional cash transfers has to acknowledge that the target population is the one who would not have otherwise invested in health and education. It is in fact difficult to isolate the effect of conditionality in such a rigorous scenario. There is almost no information to test hypothesis on this particular counterfactual. Facing a data constraint, tools from other analysis will be used to build the counterfactual. In particular, the analysis of how take up varies according to groups will prove useful for this endeavour. As explained by Zeitlin (2011), membership to a group may influence take-up and the theoretical and methodological underpinnings have been analysed by Manski (2001). He understands that the propensity of individuals to behave in a particular way may be assumed to be related to the behavior of other members of their most proximate group.

Being behavioural change an important element in the policy aims of Oportunidades, it is to be considered in our analysis. As the concept refers to change in behaviour over time, it implicitly incorporates a time element in which behaviour changes from a previous period to the present. Information at different points in time would ideally be the golden standard to measure such behavioural change. But even with nuanced information of participants' past decision making, the measurement of behavioural change is not as readily as the observed behaviour.

The programme implicitly targets those who have not been able to invest in human capital due to budget constraints. But it is very difficult to distinguish this target population from those who would have invested in human capital without the existence of the cash transfer. De Janvry and Sadoulet (2006) and Baird et al (2011) also problematize the concept of conditionality. For them, conditionality should concentrate in inducing behavioural change on the households who are less likely to attend school. Then, cash transfers should not pay for what households were already going to do. It is not straightforward to measure such behavioural change (Baird et al, 2011).

Even if we observe changes of behaviour in different points in time, this information would not necessarily disclose the fact that some household would have invested in education and health without the cash transfer. In particular, I argue that some heuristic device is necessary to elicit such information. I argue that the group effects model of Manski (1993) can provide such understanding of behavioural change.

Manski (1993) poses the assumption that individuals tend to behave in the same direction as their most proximate group. In this sense, individuals would cluster together according to their level of human capital investment. If households in low human capital cluster show different level of human capital investment than their peers, they could then be argued to show a degree of behavioural change. However, if households show similar levels of human capital investment than their peers, it may well be assumed that such households were likely to invest to such human capital levels anyway. Such information is important, as it could contribute to discern those who would have invested in human capital regardless the cash transfer. The assumption that cash transfer causes investment in human capital would then be plausible if we observe an individual behaviour different from what is expected from their peers.

The definition of peers is then important. In this research, I propose that those peers for comparison to human capital investment are the households that inhabit the same primary sampling unit. I have labelled such group the neighbourhood, as a primary sampling unit is defined in the ENIGH 2008 as the houses located in the same block, or up to 250 households if the block of households is more numerous. A key assumption of the primary unit of sampling is that households contained in each unit share a similar socioeconomic status. Hence, my assumption of proximity is therefore based on such homogeneity. Other relevant peer references could plausibly be ethnic, kinship, gender or age groups. Yet, I argue that homogeneity based on socioeconomic status is quite pertinent assumption for identifying the group of peers that have similar investments in

health and education. As I have explained before, Oportunidades is built upon the idea that income constraints are behind the lack of investment in health and education.

In this introduction is important to clearly explain the rationale upon which the usage of neighbourhood's decision is going to be a valid comparison. I have argued above that by looking at neighbours' characteristics I may elicit information on behavioural change. Yet, it is important that such neighbours' decisions are also properly identified. For this, I follow Zeitlin (2011) who has operationalised the measurement of neighbourhood effects. According to Zeitlin (2011), neighbour's decision have to be properly instrumented in order to identify the variation in neighbourhood's take-up that is independent of neighbourhoods' eligibility score.

There are at least three different hypothesis of the relationship between individual and neighbourhood characteristics and it is important to discuss them. Manski (1993) conceptualizes correlated effects of neighbourhoods as characteristics that individuals share and vary together without a causal link between them. Exogenous or contextual effects are different, they surge when individual characteristics mould the characteristics of their neighbours. They are also distinctive from endogenous effects, in which individuals adapt their behaviour towards an outcome of interest that then shapes that particular neighbourhood characteristic.

Each of the three different categories of neighbourhood variables included has a different working hypothesis behind. The first group highlights the correlated effects in neighbours. The hypothesis is that some neighbourhoods are more homogeneous in terms of public water connection or the presence of solid floor, and that is why households with such characteristics are clustered together. The best example of this hypothesis is the means-test model. It regards that rural households share the characteristics that make them income poor for the simple fact of being in a rural environment. But if we want to focus on exogenous characteristics, we would approach the variables in the second group

with a different hypothesis in mind. Households that make frequent use of the health services may be located in neighbourhoods with access to health centre in which they do not suffer discrimination, and so neighbours would also tend to make use of health facilities. In this case, the hypothesis is that households use health centres because of the exogenous placement of the health facility, or of doctors who can speak indigenous language, and not because of a particular programme.

Yet, if we want to explore the endogenous effect, the hypothesis is similar to the one that underpins the conditionality of Oportunidades. The theory of change behind the programme is that households should have more incentives to invest in health and education in order to escape poverty. If eligible households decide to go to the monthly health check-ups in order to get the cash transfer, we may observe that their neighbours do as well and we would hypothesize that there is an endogenous effect of health centre attendance in which the cash transfer induces higher levels of health check-ups in the neighbourhood.

The distinction is important for the correct interpretation of the hypothesis and its policy implications. As Zeitlin (2011) rightly asserts, drawing on Manski (1993), it is the endogenous effect the one which has interest for public policy. In the presence of exogenous or contextual effect, the individuals do not make a decision on neighbourhood membership. But the presence of endogenous effects would imply a decision to change behaviour or member of a group in order to pursue an outcome. If a household send its children to school and so do its neighbours, we may witness only a correlated or a contextual effect. But if an Oportunidades eligible household decide to send its children to school in order to get the cash transfer, and so do its neighbours, we would be in the presence of an endogenous effect of school attendance. In this endogenous effect, the cash transfer induces change of behaviour -human capital investment- and arguably the

break of intergenerational transmission of poverty. And it is the endogenous effect what policy makers are interested in, especially in the context of conditional cash transfer.

Following Zeitlin (2011), I argue that neighbours information can contribute to overcome data limitations on conditionality. The neighbourhood is defined as the primary sampling unit of the ENIGH 2008, which agglomerate as few houses located in the same block, and up to 250 households as a complete geographical area[42]. The model that identifies neighbours' decision making includes as dependent variable the mean cash receipt in the neighbourhood $\theta\bar{y}_j$, which is explained by independent variables that include:

a) the mean value of each characteristic of the eligibility score in the neighbourhood,

b) the distance between each individual characteristic and the mean value of such characteristic considered above, and

c) the inverse mills ratio of eligibility to correct for sample selection bias of eligibility.

The resulting residuals and inverse mills ratio from the latter estimation are then averaged at neighbourhood level. Such neighbourhood averages are the values used in the final models estimated in sections 4.4 and 5.3.1 of this research.

More importantly, our threshold against which behavioural change is judged are the following: the proportion of children and teenagers attending school in the neighbourhood, and the proportion of neighbours attending health centres and preventive health check-ups. These variables have been included in the analysis under the hypothesis that they portray the behaviour in neighbourhoods and that, by comparing them to individual behaviour, they may allow us to draw conclusion on the compliance of conditionality. None of them are included in the eligibility step, as they are not thought to

[42] In a few cases in which the town was formed by less than 250 households, the primary unit sample was the town rather than the block.

influence eligibility but rather cash allocation. As discussed before, neighbourhood eligibility only influences households' eligibility; it is assumed it does not influences households' decision of claiming the benefit one is entitled to[43].

It is also important to analyse compliance of conditionality among the groups that shape our analysis. Table 3.5 summarises the mean proportion of households that have either children or teenagers in school age[44] and that send them to school, as well as the mean proportion of households in which at least someone had a preventive health check-up in a period of a year before the survey. By looking at the second column I can start by asserting that school attendance at primary school level is high in Mexico, especially in urban areas. For some authors this questions the pertinence of delivering a cash transfer conditional on a behaviour that is already achieved by some households (Attanasio *et al*, 2008) and for which research has proved that conditionality is not binding to comply with the condition of sending kids to primary school (De Braw and Hoddinot, 2005). In the fourth column we see very high proportions of households in which at least one member had her weight measured in a visit to the doctor in the year previous to the survey. School attendance for teenagers cannot be taken for granted. What is striking is that eligible non-takers show lower proportions in all the three conditions of the programme, and that this figures are different even if a confidence interval of 0.05 is considered. Moreover, ineligible recipients of Oportunidades show the highest higher proportion of primary school attendance and high secondary school attendance that is not statistically different

[43] Influence of neighbours on individual's eligibility could occur indirectly and informally through means such as flows of information or lower cost of transaction. This would imply that a household would be able to decide upon acknowledging eligibility or not. In this research, the mechanism of acknowledging eligibility relies on the means-test model of Oportunidades, rather than in household decision making. Duclos (1995) offers an example of eligibility being part of household decision making. Yet, this research acknowledges neighbours influence on information and transaction cost but as part of the cash transfer receipt model.

[44] In Mexico, the starting age of compulsory education is 4 years old, although for Oportunidades cash transfer is conditional on education until third grade, when children are around 9 years old. Compulsory education lasts for 11 years to cover pre-primary, primary and lower secondary education, until pupils are around 15 years old. Oportunidades offers support to students that continue higher secondary education for three additional years, when pupils are around 18 years old. The limit age to which Oportunidades offers support is 21 years old. UNESCO (2013) and Oportunidades (2010).

from the level of the non-eligible non-recipients. Oportunidades cash recipients have higher proportion of preventive check-ups than the rest of the households. This tell us that conditionality compliance differs across groups and we can hypothesize that conditionality might represent a burden for eligible non-recipients.

TABLE 3.5 AVERAGE PROPORTION OF HOUSEHOLDS THAT COMPLY WITH CONDITIONALITY BY HOUSEHOLD ELIGIBILITY AND CASH RECEIPT

Households	Mean proportion of school-age children that attend school (std. error)	Mean proportion of teenagers that attend school (std. error)	Proportion in which at least one member had her weight measured in the previous year (std. error)
Eligible cash-recipients	0.89	0.66* (0.02)	0.96
Ineligible cash recipients	0.94* (0.006)	0.74* (0.02)	0.97
Eligible non-recipients	0.83* (0.01)	0.54* (0.02)	0.87* (0.01)
Non-eligible non-recipients	0.9	0.8	0.93* (0.002)

Source: Own calculations based on ENIGH (2008) and Oportunidades (2010).
* Statistically different from the rest of mean estimates at a 0.05 level.

I now explore if eligible households are more likely to comply with conditionality to begin with, or if non-eligible households have similar probability of compliance, and the analysis of the neighbourhoods in which households live will help us to understand these nuances. The assumption behind it is that we would expect households to live in neighbourhoods with similar behaviour. If the rates of compliance are similar among neighbours, we could think that there has been no behavioural change among recipients. Hence, conditionality would make no difference for households who comply with it, and that there would be no incentives for a behavioural change of households who do not comply with it. Table 3.6 present the mean proportion of conditionality compliance of the blocks in which each category of household lives. We can see, firstly, that there is no statistical difference between the means presented in the second and fourth columns, implying that the neighbourhoods for all categories of households are similar in terms of primary school attendance and preventive care. Secondary school attendance is

significantly higher in blocks where non-eligible non-recipient households live, but this proportion of 79% is not different from the secondary school attendance observed by such households, which is around 80%. Yet, it is important to highlight that, in general, non-recipient households' neighbourhoods show higher levels of investment in education, and this difference is statistically significant.

TABLE 3.6 AVERAGE PROPORTION OF HOUSEHOLDS THAT COMPLY WITH CONDITIONALITY BY HOUSEHOLD ELIGIBILITY AND CASH RECEIPT

Households	Mean proportion of school-age children that attend school	Mean proportion of teenagers that attend school (std. error)	Proportion in which at least one member had her weight measured in the previous year
Eligible cash-recipients	0.87	0.71	0.93
Ineligible cash recipients	0.89	0.73	0.93
Eligible non-recipients	0.87	0.66	0.91
Non-eligible non-recipients	0.9	0.79* (0.02)	0.93

Source: Own calculations based on ENIGH (2008) and Oportunidades (2010).
* Statistically different from the rest of mean estimates at a 0.05 level.

Comparing ineligible households in tables 3.5 and 3.6 we conclude that while they live in blocks similar to the rest of the population in terms of compliance with conditionality, they show higher levels than their neighbours in primary school attendance and preventive health check-ups. In the meantime, eligible households that are excluded from the programme have statistically significant lower achievements than their neighbours, which tend to be similar to other neighbourhoods in statistical terms. This would support the hypothesis that conditionality may contribute to the exclusion of eligible non-recipients. This evidence is not strong enough yet to establish a causal effect in which conditionality leads to exclusion of households, but the statistical relation is of concern.

It is important to mention that the levels of preventive health check-ups for eligible recipients is statistically higher than the neighbours in their blocks, which would imply that health conditionality has at least more perceivable outcomes than the schooling conditionality. This could lead to question the importance of conditionality of the programme. Yet, the evidence that ineligible households seem to bind to education

conditionality to higher rates than their neighbours is puzzling, since this does not occur with the rest of the population regardless their category of receptions. This evidence could support the importance of the conditionality in contributing to enhance investment in human capital for households that receive Oportunidades. However, it also highlights that eligible households are investing less in human capital, and that eligible excluded households are investing less than their own neighbours, which is particularly worrying.

This also highlights the importance of eligibility as a category of analysis. Being Oportunidades an antipoverty programme, we would expect it targets households under the poverty line. Yet, Oportunidades also seeks to enhance human capital and eligibility is design to identify income poor households through proxy indicators on human capital. Even though I am not testing the relationship between human capital deprivation and income poverty directly, the evidence presented above suggest that there may be other grounds to justify the use of proxy indicators of human capital as a tool for targeting beyond its relationship with income poverty. With this evidence, I argue that eligibility as defined by Oportunidades may contribute to identify households who are less likely to invest in human capital, beyond the fact that they are income poor. Many reasons may be behind this, but I argue the most important is that eligibility model explicitly considers human capital dimensions and not income per se. For instance, eligibility targets low education of the head of household, which may cause both low income and low educational achievements of children.

This evidence does not fully test the existence of a causal link between the eligibility model and underinvestment on human capital. A causal hypothesis that builds upon the endogenous effects of Manski (1993), would suggest that households that are eligible would realize the benefits of complying with the conditionality in exchange of the cash receipt. What we see in fact is that cash recipients seem to be aware of the benefits of investing in health. Eligible and ineligible households that get cash transfers are investing

more than their neighbours and more than other households in health, which is a positive evidence of an endogenous effect. Moreover, ineligible households who receive cash transfers invest in education more than their neighbours, more than eligible households and to levels similar or higher than non-eligible non-recipients. I assume that they are aware of the benefits of programme because they invest more than their neighbours, which would support an endogenous effect of the cash transfer. Eligible recipients certainly invest in education more than other eligible peers who do not get the cash receipt, but the question remains on why they do not invest more than their neighbours or than ineligible households. An additional concern is that if it is only conditionality what is at stake when some eligible households are excluded.

Methodologically, neighbourhood characteristics proved to be a valid instrument for identification of the model, as well as to contribute to a better understanding of some mechanisms through which conditionality may discourage participation in Oportunidades. Information on school attendance and preventive health check-ups at household and neighbourhood levels was used to answer the question of why some households decide to join the programme and comply with conditionality while some others do not. I have tried to control for 'near-the-household behaviour' in order to understand whether most of the eligible non-recipients are behaving the same way as the eligible ones who take-up the programme. They are not, which points out that conditionality may be binding and may be the main reason behind low take-up. The use of the latent variable of cost of participation is useful to understand this problematic. We can associate cost with particular groups or behaviours and understand the impact that conditionality may have on take up. The discussion of this methodology is carried forward in the following section.

3.5 Welfare participation costs as a latent variable

At the moment of estimating welfare participation, we may enquire to what extent unobserved causes are at stake in determining take-up of welfare. The connection between the presence of a latent variable and the analysis of the residuals of the estimation is that an inconsistent estimation can also be defined as a model that is not fully identified due to important omitted information. This omitted information is thought to be part of the residuals of the model. In the same way, measurement error can be defined as a problem of identification of the model, in which the information of a latent variable that causes the measurement is omitted from the model, causing at the same time inconsistent estimation (Weeks, 2001).

We may argue that the latent variable affects participants and non-participants not at random but heterogeneously (Zeitlin, 2010c). If we suspect a latent variable causes self-selection, we will not be able to hold the uncounfoundedness assumption in which the expected outcome for a person who did not receive the treatment is equal for both eligible and non-eligible households (Zeitlin, 2010b). On such occasions, by dealing with self-selection and the error term in an explicit way not only can we better understand the underlying causes of the bias but also we can choose the best method to deal with it. Not to mention that self-selection in cross-sectional analysis, such as the one used in this research, causes biased coefficients because the relationship between variables is estimated based on a biased sample. And it also limits the external validity of the analysis as results cannot be generalised to a wider population if the sample of participants is not representative to begin with (Cuddeback, *et al*, 2004).

If we are portraying measurement error as evidence of the existence of a latent variable, we cannot assume that the residuals are independently and randomly distributed across the population. Instead, the residuals are systematically related to other variables due to endogeneity[45]. The last specification is similar to the grounds for justifying the use of a sample

[45] Technically, selection bias and endogeneity are two different problems and disentangling them depends on the judgement of the researcher, that is, the 'appropriate' answer depends on how the researcher believes a latent variable affects participation in welfare. If we assume that we only observe participation of individuals who self-select into the programme, we have a sample selection bias. Conversely, if we believe the latent variable is relegated to an unobservable in the error term and that it affects other independent variables, then we have an endogeneity problem. In this analysis, I am assuming that selection bias and

selection correction method. According to Heckman (1979) a sample bias does not allow us to observe the full distribution of positive outcomes but a truncated one due to sample selection. We can test for the presence of such a bias if there is correlation between a first stage selection equation and a second stage outcome equation. If so, we can also argue that the selection process is determined by a latent variable.

In chapter 5 I argue that a latent variable of cost of participation also determines the distribution of the probability of receiving the cash transfer. Arguing the presence of a latent variable implies that no trivial assumptions have to be in place before undertaking such modeling. One of the most important assumptions of this model is that the correlation between latent variable and independent variables is not given by the empirical interaction of the data but by a functional form made explicit by the researcher (Krishnakumar and Nagar, 2008). This indicates that there is a joint distribution underlying the latent variables, which is grounded on a theoretical formulation of how eligible households and recipients behave, rather than on the empirical drive of the data. As discussed in the second section of this chapter, our sample selection correction model assumes an underlying conditional distribution between cash transfers and eligibility. Then, the functional form assumes that eligibility, which causes selection bias, is correlated with the latent variable of costs of welfare participation. Specifically, eligible households who underinvest in human capital face higher costs of welfare participation. This increases the risk of them being part of the exclusion error.

3.5.1 Formalisation of the analysis of errors.

There are at least three assessments of eligibility that concern us in this chapter: the analyst's assessment, the administrator's assessment and the claimant's assessment. From Duclos (1995) we understand that there is a y* that is the true level of entitlement of a household that will receive the benefits B if the household is eligible. However, the problems that we discussed before prevent us from seeing the true y* and we can only estimate y that is:

$$y^* = \beta x + \varepsilon_g + \varepsilon_h + \varepsilon_a$$

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endogeneity problem is present in the conditionality of the programme, whereas selection bias arises from the conditional distribution of cash transfers and eligibility.

Where ε_g is the error of the eligibility assessment made by the government, ε_h is the error of the eligibility assessment made by the household and ε_a is the error of the assessment made by the analysis. However, with the information we have, we can only estimate $\hat{y} = \beta x + (\varepsilon_g + \varepsilon_h + \varepsilon_a)$ where $\hat{y}$ is the net benefit of the cash transfer made by the government.

Truly unobservable latent variables can be expressed as a vector of observed regressors that imperfectly approximate the latent variable with error added to it. However, measurement error can also be simply derived from inaccurate measurement. Distinguishing between the two is not entirely straightforward. In this analysis, I assume that the measurement error of the selection equation is minimum and captured by the inverse Mills ratio or non-selection hazard in the first selection or measurement equation of the model above.

It turns out that a probit model can also be expressed as a latent variable y* that we cannot observe it but that is determined by:

$$y^* = \beta X + \varepsilon$$

In which y*=0 if the household does not participate in the programme and y*=1 if the household participates in the programme, for which the cost have to be lower than the benefits. We cannot observe the true eligibility (only available in administrative data) but we know that it will be positive for households which take up benefits and participate in the programme. If we consider y* as the net benefit from the programme, that is the cash transfer (standardized in *ln (B)*) minus costs (which are a subset of variables Z constructed from household characteristics)

$$\ln B - \alpha Z = \beta X + \varepsilon$$

And we know that if y is to be positive, the following conditions will be fulfilled:

$$y = \begin{Bmatrix} y^* \geq 1 & \varepsilon \geq -\beta X - \alpha Z + \ln B \\ y^* < 0 & \varepsilon < -\beta X - \alpha Z + \ln B \end{Bmatrix}$$

Then, we can also assume that the probability of participation also depends on the probability of having a positive net benefit from the conditional cash transfer:

$$\Pr(participation) = \Pr(y) = \Pr(y^* \geq 1) = \Pr(\varepsilon \geq -\beta X - \alpha Z + \ln B) = F(\beta x)$$

Pr (*participation*) is equal to *F (βx)* which is the cumulative distribution function of ε, where ε ~ N (0,1) when $y^* \geq 1$, and 0 otherwise.

Beyond measurement error, we are also interested in the extent to which a model can inform us on the value that potential recipients expect to obtain from participating in welfare programmes. This expected value is a good guess of the value of cash transfer after considering the conditions of eligibility, health and school attendance and other households characteristics. Moreover, given the fact that such characteristics are known to the household as well as to the researcher, they are more informative of the household decision making than, for instance, monthly entitlement or eligibility scores, which are determined by administrators but may be unknown to some households. The expected value then may also contribute us to overcome the limitations that Duclos (1995) highlighted. A key assumption would be that the expected value is independent of the assignment of the programme, but conditional on household's characteristics that determine eligibility, conditionality compliance, barriers to and taste for welfare benefits.

The estimation of the expected value of cash transfer is the expected value of the natural logarithm of the cash transfer:

$$ln(y) = E[\ln(y)\,|x] + u$$

A linear approximation of the model is better known as:

$$ln(y) = \beta x + u$$

Where x is a set household characteristics that are observed and determine eligibility, conditionality compliance, barriers to and taste for welfare benefits. Since the residual represents the departure of the observed value from the estimated value, it is important to understand the properties of the residual. The residual is thought to contain information about unobserved household and contextual characteristics. Such characteristics may elicit information about conditions associated with participation or take-up of the programme if the cash transfer is positive, which occurs if

$$ln(y) - \beta x > u$$

Therefore, the expected benefit is thought to be positive but sufficiently high to overcome the value of residuals so the total cash transfer remains positive. But we have mentioned that our data is censored as we do not observe the cash transfer when the expected value is not enough to overcome the cost of participation

$$ln(y) - \beta x \leq u$$

Given we have characterized participation as a latent variable, we have the following restrictions

$$y = \begin{cases} y^* > 0 & u < \ln(y) - \beta x \\ y^* \leq 0 & u \geq \ln(y) - \beta x \end{cases}$$

Hernandez et al (2007) understand that a model of take-up can also elicit information on the implicit claim costs of taking-up the programme, which are: 'the compensation variation required to offset stigma and other barriers to participation' (p. 15). The model can be thought of as a utility function but Hernandez et al (2007) and other research has found it more useful to work with the logarithm of the benefit entitlement, ln B, that is regressed on observed household characteristics Zα and a residual V that informs on the unobserved individual characteristics that jointly determine take-up of welfare. The model understands that take-up occurs if

$$\ln(B_i) > Z\alpha + V$$

$$\Pr(take - up \,|B_i, Z\alpha) = \Pr(V < lnB + Z\alpha) = F(\sigma^{-1}[\ln B - Z\alpha])$$

We obtain for households with positive take-up:

$$\Pr(D|Z) = \Pr(\varepsilon \geq -\alpha Z + \ln B)$$

$$\Pr(D|X, Z) = \Pr(\varepsilon \geq -\alpha Z - \beta X + \ln B)$$

for households with negative take-up:

$$\Pr(D|Z) = \Pr(\varepsilon < -\alpha Z + \ln B)$$

$$\Pr(D|X, Z) = \Pr(\varepsilon < -\alpha Z - \beta X + \ln B)$$

While variables Z_i model the eligibility, variables X_i model the conditionality of the programme, attending to school and health centres, and the cost of complying with those conditions. These

variables X_i are not considered as guaranteeing the access to the programme, and therefore work as an instrument for identifying the previous model.

An important assumption of this procedure is that the probability of getting the cash transfer D conditional on X is the same for different Z values. If the assumption holds (Tarozzi and Deaton, 2010), the expected value of the estimated measure would be 0

$$E(D|X,Z) = \int (X,Z)dF(X,Z) = 0$$

Hernandez, *et al*, (2007) use the distributions of positive and negative take-up mentioned above to estimate the costs of claim as:

$$\text{Cost} = \frac{e^{Z\alpha}\int_{-\infty}^{lnB+Z\alpha} e^{v}dF(V)}{\int_{-\infty}^{lnB+Z\alpha} dF(V)}$$

And based on theorem 2.6 and the point of truncation of a log-normal distribution (Aitchison and Brown, 1956, p. 87):

$$E\{X^j\} = \int_{\xi}^{\infty} \frac{x^j d\Lambda(x|\mu,\sigma^2)}{1-\Lambda(\xi|\mu,\sigma^2)}$$

$$= \frac{e^{j\mu+\frac{1}{2}j^2\sigma^2}}{1-\Lambda(\xi|\mu,\sigma^2)} \int_{\xi}^{\infty} d\Lambda(x|\mu+j\sigma^2,\sigma^2)$$

$$= e^{j\mu+\frac{1}{2}j^2\sigma^2} \frac{1-\Lambda(\xi|\mu+j\sigma^2,\sigma^2)}{1-\Lambda(\xi|\mu,\sigma^2)}$$

the cost for participants in Oportunidades is:

$$\text{Cost} = \exp\left(\text{Z}\alpha + \frac{\sigma^2}{2}\right) \Phi\frac{\ln \text{B} - \text{Z}\alpha - \sigma^2}{\sigma} \Big/ \Phi\frac{\ln \text{B} - \text{Z}\alpha}{\sigma}$$

While the costs of non-participants is:

$$\text{Cost} = \exp\left(\text{Z}\alpha + \frac{\sigma^2}{2}\right) 1-\Phi\frac{\ln \text{B} - \text{Z}\alpha - \sigma^2}{\sigma} \Big/ 1-\Phi\frac{\ln \text{B} - \text{Z}\alpha}{\sigma}$$

In chapter 5 I succinctly summarize this methodology and present the results of this analysis.

3.6 The assessment of targeting performance and its regional variations.

3.6.1 The link between methodologies of Chapters 4 and 6: the sample selection model is conditioned by the regional score.

Geography determines eligibility of Oportunidades to a great extent, and little attention has been paid to it in research. The official model used to allocate Oportunidades features eligibility scores differentiated in geographical terms to reflect the fact that household income levels vary greatly along the Mexican territory. The proxy means-test was tuned (using discriminant analysis) to identify households below a poverty line. But the effect of local prices and labour market dynamics determines income poverty to a great extent and this was present in the mind-set of the policy makers who designed the targeting mechanism. In order to account for this geographical variation of income, the means –test model divides the territory in 18 different regions with14 different scores for each region. Central Chiapas, the region that has proportionally more population in income poverty, receives no negative score, and the scores are negative as the income level of regions increases. The region that gets the highest negative score is the South Pacific Coast, with a -0.66 score for all households. In practical terms, a 0.69 household score in Chiapas warrants eligibility to Oportunidades, but the same household in the South Pacific Coast would have a score of 0.69 - 0.66=0.03, and loses its eligibility below the 0.69 threshold. A blatant consequence of differentiated scores is that we expect to see less eligible households in regions with lower scores.

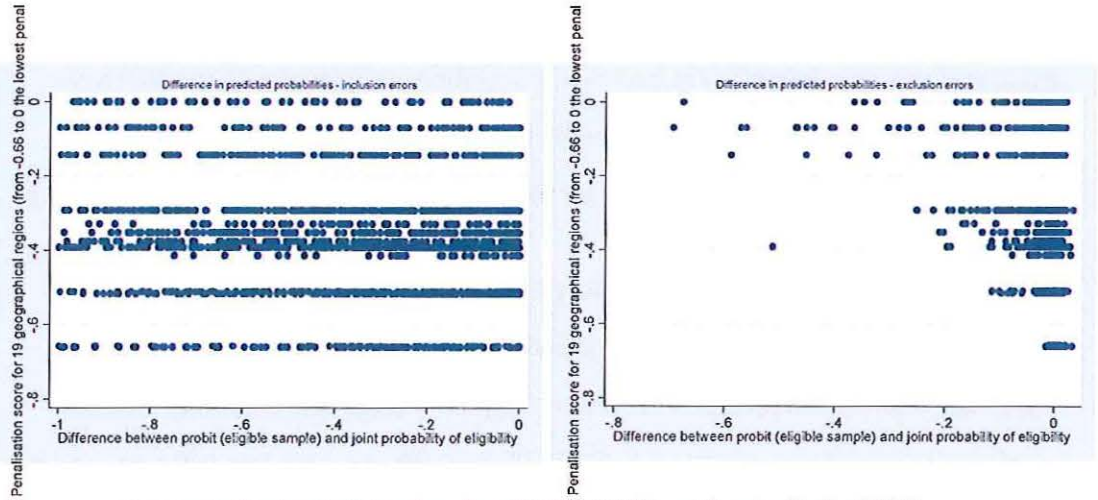
This has important implications for the sample selection correction model, as discussed in section 2 above. The first step of sample selection, that models eligibility, only includes sociodemographic characteristics and the instrumental variable $z_i = \bar{\lambda}_j$ as regressors. Consequently, the expected probability of being eligible and the inverse mills ratio are

correlated with the differentiated regional score. Figure 3.1 in section 3.2 above evidenced this correlation[46]. Yet, no other probabilities estimated with the different methods, or the residuals and latent variables obtained, vary with the regional differentiated score. This is reassuring for the estimation of the model at national level.

Nevertheless, I will recall the concept discussed in section 3.3 of differences between predicted probabilities with different methods. There is a regional variation in the difference between the predicted probability of jointly observing eligibility and cash receipt and the probit predicted probability. Figure 3.10 plots this last difference and the regional score. Differences are mostly negative, implying that the predicted joint probability is smaller than the probit one. When the difference is positive, they are also very small, meaning that it may not be statistically significant. In the left-hand-side panel we see the plot for ineligible households, in which we do not observe a particular relationship. Yet, in the right-hand-side panel we can see the plot for eligible non-recipients. We see that regions with less penalisation, such as Chiapas with a score of 0, the difference between the predicted probabilities can be very large. On the contrary, South Pacific Coast that has a regional score of -0.66, observes very small differences in probabilities. For eligible households, the probit and joint probabilities are closer in more penalized zones. As discussed in section 2, the probit model estimated probabilities closer to the sample selection for eligible households. Now, we see that the joint probability and probit probability are even closer in more affluent regions where the penalisation score is high. To put it differently, the predicted probabilities with different methods are closer as the probability of being eligible decreases, as occurs in the most heavily penalised regions.

FIGURE 3.10 DIFFERENCE BETWEEN PREDICTED PROBABILITIES OF PROBIT MODEL AND JOINT PROBABILITY OF SAMPLE SELECTION FOR INCLUSION AND EXCLUSION ERRORS

[46] As discussed, identification of the selection equation is only possible because of this exogenous regional variation in the eligibility scores.



Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

The marginal probability of cash receipt is not conditional on being eligible. In contrast, the joint probability of cash receipt is conditional on eligibility. As fewer households are eligible in more affluent areas, the probability of getting the cash transfer conditional on being eligible would be smaller in such areas. On the contrary, poorest areas would have more eligible households and hence larger probabilities of cash receipt conditional on being eligible. Moreover, figure 3.10 portrays ineligible households on the left hand side. As the marginal probability of cash receipt is high for ineligible recipients may be large but the joint probability conditional on being eligible is radically different. Hence the difference between probabilities resides on conditioning for eligibility. Meanwhile, the right-hand side portrays eligible households probabilities for which the marginal and the joint probability are better tuned. Moreover, the marginal probability is very similar to the joint probability in more affluent regions, indicating that conditioning on eligibility makes not much improvement in the probability of cash receipt. However, in less affluent areas, the marginal probability of cash receipt is greater than the one that condition on eligibility.

Another way of looking at this is that the probit model overestimates the potential of achieving perfect targeting. I interpret the joint probability of eligibility and cash receipt

as the probability of perfect targeting. At the margin, the probit model overestimates this probability. Yet, this probability of perfect targeting also varies as the eligibility score obtained from the means-test model varies. If a household has a low probability of being eligible then it is more difficult to rightly identify it as poor. Yet, ironically, in the graph we see that this also means a lower probability of making an exclusion error, due to the fact that there are fewer eligible households. In the poorest areas, such as Chiapas, we see a high probability of being eligible. Therefore, the probability of good targeting will be high, as will also be the probability of incurring exclusion errors. Therefore, we see a greater difference between the marginal probability and the joint probabilities in such areas.

These stylized facts should not make us jump to the conclusion that poorer regions would have better targeting efficiency. As is shown in Chapter 6, targeting efficiency does not only depend on the means-test model. Yet, the fact that the use of a finely tuned means-test model could empirically result in better measures of targeting efficiency but do not, tell us a great deal about the implementation of public policy at local level.

I should acknowledge the limitations of the national model to predict variations at regional or local level, where we expect more than 50 different ways of estimating welfare participation. In fact, I was only able to estimating the same model in one region, Puebla, and all of the marginal effects were statistically insignificant. Faced with hardware and time constraints, the fundamental limitation to estimate local models is that there is almost no exogenous variation in the eligibility score. This exogenous variation is huge at national level due to the regional differentiated score. Explaining welfare participation at local level is a potential area for further research.

3.6.2 Methods for assessing targeting performance: Targeting Differential and Concentration index.

There is a great variety of different methodologies to evaluate policy outcomes, and there is no less diversity in ways of assessing the targeting efficiency of public policies. As the initial puzzle of this research is to understand low participation of Oportunidades in urban areas, I judged it pertinent to frame the problem in terms of errors of targeting, which implies that low participation is a problem that needs to be solved. But many people would be interested about other issues of targeting. When Oportunidades was created, the main concern was whether the cash transfer would be allocated to the poorest quintiles of the population. But this last question would arguably be better approached with a different method for assessing targeting efficiency than the errors of targeting. I therefore want to explain the implications of the methodology chosen by contrasting it with other methodologies.

A seminal discussion of targeting efficiency was offered by Coady, Grosh and Hodinott (2004), in which they gathered information of at least 5 different targeting methods implemented in a sample of 67 welfare programmes in 30 different countries, and assessed with 4 different methods. Ravallion (2009) explores of 5 methods, and makes explicit the relationship between them for the case of *Di Bao* in China. He warns that all the measures he explores build upon the concept of targeting errors. Yet, some measures tend to weight one kind of targeting error more than others. In the empirical case of *Di Bao*, it turned out to be more insightful to use the targeting differential measure because it assigns similar weights to both targeting errors. Based on his explorations, I will now discuss two different methodologies for assessing targeting efficiency of Oportunidades: the concentration index and the targeting differential.

3.6.2.1 The targeting differential

We can think of the targeting differential as a summary of the accurate targeting of the programme minus the inaccurate one. The accurate targeting is the participation rate or coverage rate, which is the proportion of eligible households that get the cash transfer.

The inaccurate targeting is the proportion of non-eligible households benefited by the programme, also known as the leakage rate. But we can also think about it in terms of the errors of inclusion and exclusion that we have discussed. The inaccurate targeting is simple the error of inclusion. And the coverage rate is 1 minus the error of exclusion. So we have that the targeting differential TD=1-(exclusion errors + inclusion errors). This gives a similar weight to both errors of targeting in the summary measure.

In a scenario in which all and only eligible households get the cash transfer, we have a case of perfect targeting and TD = 1. In the unlikely scenario of all and only ineligible households get the cash transfer, we would have TD= -1. A positive TD that is closer to 1 indicates better targeting. From Table 2.8 we know that the proportion of eligible households excluded from the programme is 0.67, while the proportion of non-eligible households that get the cash receipt is 0.04, so TD= 1-0.71=0.29. A positive number tells us that there is accurate targeting, but there is still a lot room for improvement. In Chapter 6 I disaggregate the targeting differential by region and states in Mexico and understand three hypothesis that may help us to explain differences in targeting differentials. I also explore the association between targeting differentials and impact to poverty.

I do not test the hypothesis with other methods for assessing targeting efficiency. I argue that the targeting differential is the summary measure best suited to assess the problem of exclusion and inclusion errors in terms of eligibility. Yet, I know there are other methodologies that are more popular and may be also used to both test the hypothesis I have proposed and to explain poverty impact. In this section I would like to, first, present the concentration index methodology and, secondly, discuss how it could be used to approach the hypothesis proposed and the caveats of doing so.

3.6.2.2 The Concentration Index.

The Concentration Index (CI), as well as other measures of targeting, builds upon the idea of the Lorenz curve that is used to analyse inequality of income. The Lorenz curve aligns all individual incomes in the economy ordered from smallest to largest in the horizontal axis of a box. In the vertical axis the proportion of income in the economy is represented from 0% to 100%, and each individual shows the proportion of the total income in the economy they have. If all individuals earned an equal amount of money, each individual would also earn an equal proportion of income in the economy and the cumulative distribution would result in a 45 degree diagonal line crossing from the intersection to the top right corner of the box. The Lorenz curve is located below this line. The extent to which the distribution of income is unequal in an economy is given by the area that separates the diagonal line of perfect equal distribution from the observed cumulative distribution of income, and the Gini coefficient measures this area as an indicator of the inequality of income distribution in the economy.

The concept of the Lorenz curve has been modified to represent the distribution of public expenditure in an economy, giving as a result the concentration curve. Individuals are still aligned in increasing order of their income in the horizontal axis, but the vertical axis now represents the proportion of the total public expenditure in the economy each individual has. If all individuals were to get the same amount of public expenditure, its cumulative distribution would also follow the 45 degree diagonal line of perfect equal distribution. But conceptually, anti-poverty expenditure is supposed to be allocated to the lowest part of the income distribution. Then, the concentration curve would be ideally located above the equality line at the lowest part of the distribution and then converge to the top right-hand corner as more wealthy individuals are allocate the last proportion of the total public expenditure. The space that separates the perfect equality from the concentration curve is an indicator of unequal distribution. This area is measured by the CI, analogue to the Gini coefficient, which indicates the unequal distribution of public expenditure. The concentration index varies from -1 to 1, where CI=-1 is the most

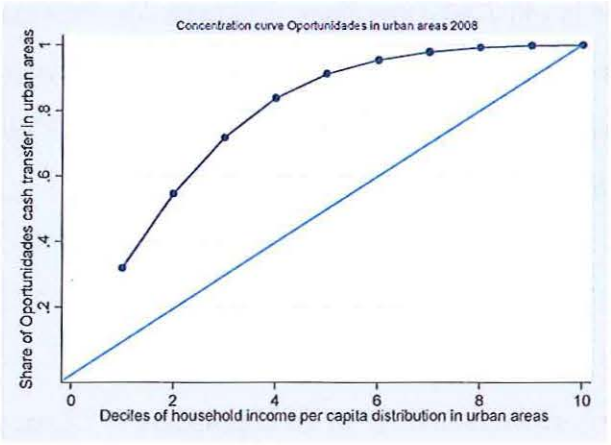
progressive situation in which all expenditure goes to the lowest income in the economy; CI=0 would imply an equal distribution of expenditure and CI=1 would mean that all expenditure is allocated to the highest income in the economy (O'Donnell *et al*, 2008). The formal expression of the Concentration index is:

$$C = 1 - 2 \int_0^1 C_e(p)\, dp$$

Where $C_e(p)$ is the concentration curve of the expenditure e [47]. Coady, Grosh and Hodinott (2002) indicate that the concentration curve is quite known in different international interventions and easier to interpret than other measures of targeting. However, Ravallion (2009) points some disadvantages: CI does not reflect the size of the expenditure and it is difficult to interpret as very different allocations of transfers can entail the same CI. CI=0 could reflect a concentration curve that coincides with the diagonal or a curve that crosses the diagonal and creates equal areas below and above the line that cancel out. The index then loses information on where the concentration curve is located (O'Donnell *et al*, 2008). Figure 3.11 below shows the share of Oportunidades expenditure by income decile in urban areas. The 45 degree diagonal is added for reference. The CI of Oportunidades cash transfers in urban areas is estimated in -0.5, which indicates that the programme is progressive at targeting the lowest income deciles if compare with some other neutral or even regressive social expenditure in Mexico.

FIGURE 3.11 CONCENTRATION CURVE FOR OPORTUNIDADES CASH TRANSFERS

[47] Ravallion (2009) formalizes the concentration index in a slightly different way, giving as a result that positive outcomes are progressive and negative outcomes are regressive. I use the notation of O'Donnell, *et al,* (2008) as a concentration index for Oportunidades used that notation in World Bank (2004).



Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

Despite the fact that Oportunidades concentrates benefits on the poor, it has a weak coverage of eligible and income poor people, as presented in the introduction and discussed in Chapter 6. Moreover, as Ravallion (2009) argues, CI does not provide information about the size of the transfer and the amount needed to reach all income poor and bring them above the poverty line. As mentioned in Chapter 6, Oportunidades only provides 1/3 of the resources needed to bring lift all poor people under the capability poverty line (in the first two deciles of the income distribution) up to the poverty line[48]. This may also limit the impact of the ptrogramme, which cannot be acknowledged precisely by only using the CI.

More fundamentally, the CI conveys information only on the distribution of expenditure in relation to the income distribution. If we use this tool to evaluate the targeting, we are implicitly setting our yardstick to be referenced in the income distribution. One could argue that the 'natural' target population of an anti-poverty programme is the one below the poverty line. In the introduction of this thesis I have set out our problem to be the allocation of cash transfer among households who are eligible for the programme. The

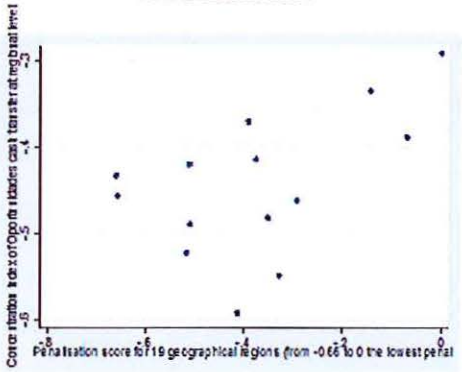
[48] The total gap to lift households to the capability poverty line is around 10 thousand million pesos and Oportunidades cash transfer amoun to 3.1 thousand million pesos, based on ENIGH (2008), Coneval (2010) and author's calculations.

target population of our problem is not income poor but eligible households. I therefore argue that it is more convenient to use a different methodology, one that explicitly considers the target group and the errors of targeting. The targeting differential considers explicitly the errors of targeting, and this is the method I suggest to be used for assessing targeting efficiency of Oportunidades.

I can also disaggregate the concentration index and see whether CI contributes to explaining poverty impact, which is our main challenge in Chapter 6. In order to avoid repetition, I do not elaborate here the assumptions of the hypothesis and the way in which they are tested. I would just like to exemplify that the CI can also provide information to test this hypothesis.

A first hypothesis is that the mean-test method provides the information needed to improve targeting efficiency. I assumed that regions with lower penalization would have better targeting efficiency because the means-test model was better fitted in those regions. The CI provides evidence precisely in the opposite direction: figure 3.12 shows how the CI score increases (meaning targeting deterioration) as the regional penalization decreases from -0.66 to 0.

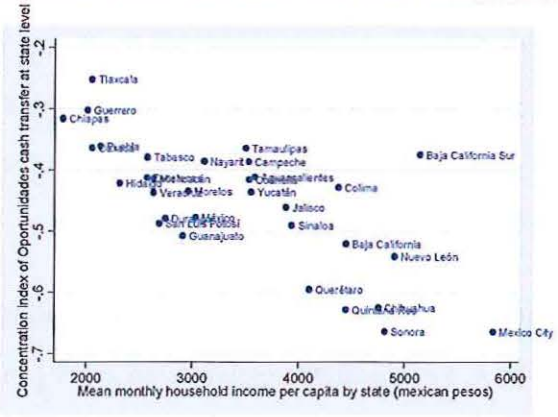
FIGURE 3.12 CONCENTRATION INDEX BY REGIONS OF OPORTUNIDADES AND REGIONAL PENALISATION



Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

A second hypothesis is that richest regions would have higher budgets available to cope with low levels of poverty, so I would expect improved targeting efficiency in those regions. This hypothesis is supported by the evidence provided by the CI. As we see in Figure 3.13 below, CI is higher for states with lower mean household income per capita. However, the hypothesis is not supported by using the TD, as explored in Chapter 6.

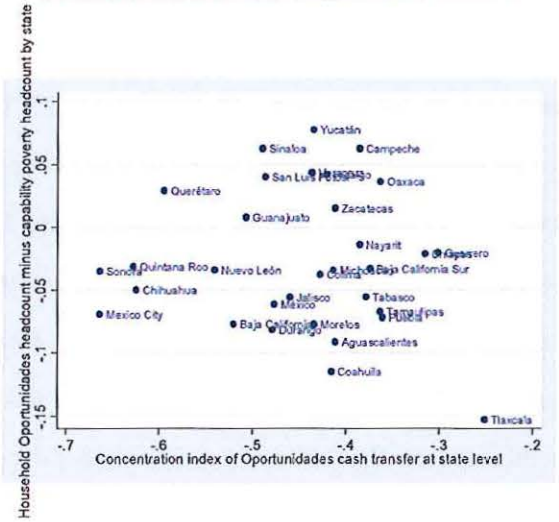
FIGURE 3.13 CONCENTRATION INDEX AND MEAN HOUSEHOLD INCOME PER CAPITA BY STATES



Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

A third hypothesis is that there is a mismatch between the Oportunidades model and the income poverty at local level. CI does not provide much information to deal with this hypothesis. Figure 3.14 plots the CI at state level in the vertical axis and in the horizontal axis the difference between the capability income poverty headcount and the Oportunidades headcount at state level. Although there seems to be a positive relation, the TD provides a clearer understanding between targeting efficiency and the mismatch in headcounts.

FIGURE 3.14 CONCENTRATION INDEX AND DIFFERENCE BETWEEN POVERTY AND OPORTUNIDADES HEADCOUNT BY STATES



Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

Neither the CI nor the TD acknowledge the size of the transfer, which is behind the fact that they cannot explain impact on poverty, as arguably the impact on poverty depends on the gap that separates the poor from the poverty line.

There is fair argument for focusing on the targeting of income poor household rather than the eligible ones. We could argue that an antipoverty programme should target the income poor population. We could use as reference the income poor population rather than the eligible one. In such a case, there would be a closer relation between the errors of targeting and the CI method. The CI methodology would then also be important in understanding targeting of income poor households at state level. We could also explore whether income poor households are also less likely to invest in human capital, which is an additional empirical question. These are certainly interesting question for further research.

3.7 Conclusions

In this chapter I have discussed the importance of changing the focus of analysis to the distribution of Oportunidades receipt conditional on being eligible. I have argued that the marginal distribution of cash transfers is driven by the largest group of households which is those that do not receive the cash transfers. The conditional distribution is also important to interpret our results in terms of four categories of targeting efficiency. In the second section of this chapter I presented the sample selection correction model which considers this conditional distribution in two steps: the first step models the means-test eligibility, and the second step uses that information to estimate the probability of receiving the cash transfer. The underlying assumption is that there is a sample bias in Oportunidades cash receipt induced by the condition of being eligible. The empirical validity of these assumptions was confirmed by the failure to reject the null hypothesis.

Yet, the sample selection method also offers the possibility of explicitly analyzing various categories of targeting efficiency, which also make explicit the errors of targeting: eligible non-receipt and ineligible receipt of cash transfers. The sample selection methodology allows us to see predicted probabilities of cash receipt according to the category of targeting efficiency in which they fall. Being aware of other methodologies also enhance our understanding of participation in Oportunidades. Section 3 analysed the differences between predicted probabilities from different methods. The differences were shown to be a useful tool to understand the errors of inclusion and exclusion and, therefore, targeting efficiency.

Methodologically, neighbourhood characteristics proved to be a valid instrument for identification of the model. And section 4 proposed the analysis of neighbourhood characteristics to understand the compliance with conditionality among the different categories of targeting efficiency. This method elicited information that was not explicitly present in the dataset but conditionality is an important factor behind eligible non-take-

up. Information on school attendance and preventive health check-ups at household and neighbourhood levels are used to answer the question of why some households decide to join the programme and comply with conditionality while some others do not.

Section 5 presented the latent variable methodology and how it is used to frame the cost of participation in Oportunidades. It also discussed the main assumptions behind it and the way in which they are justified. The main implications of this analysis is that eligible households who do not take-up the programme also underinvest in human capital and therefore face higher costs of welfare participation. This increases the risk of them being part of the exclusion error. Section 6 discussed how geographical means-test targeting is determinant in the analysis of take-up. Penalisation scores are empirically a determining factor in the sample selection model and the nuances of this have been presented. Regions have the potential for explaining targeting efficiency if only the means-test targeting was the only factor behind cash transfer allocation. Section 6 also discussed two different methodologies to assess targeting efficiency of public policies. Both build upon the concept of errors of targeting, but while the Concentration Index does not assign heavy weight to the exclusion error, the Targeting Differential is better suited for assessing the extent of exclusion error.

The objective of this chapter was to justify the methodologies used and exemplify the implications of such decision making for this research. This chapter has served as link between the theoretical part and the empirical contributions of this thesis. The following chapters present the assessment of participation in Oportunidades in urban areas, its costs and distributional consequences. Means-test is not the only factor for explaining cash transfer allocation and targeting efficiency, and the results are presented in the following chapters.

4 TARGETING EFFICIENCY AND THE COSTS OF HEALTH CONDITIONALITY OF OPORTUNIDADES IN URBAN AREAS

4.1 Introduction

Oportunidades is an antipoverty programme that provides a cash transfer to poor households in Mexico under the condition that children and teenagers attend school and the recipient of the cash transfer attends a monthly health talks. Being conditional on this behaviour, Conditional Cash Transfers (CCTs) are meant to give incentives for household to invest in health and education, as well as alleviating income needs. By means of enhancing health and education investments in the household, the programme aims to prevent the intergenerational transmission of poverty. On the one hand, Oportunidades recognizes the multiple dimensions of poverty and establishes a human development link between these dimensions. On the other hand, it gives priority to the income dimension of poverty since the weight of education, health, assets and other indicators in the eligibility score are determined according to their statistical relation to the income poverty threshold.

If multiple objectives are at the heart of the design of Oportunidades, there are also multiple targeting instruments in its design. Oportunidades defines poverty on an income basis but proxy indicators of income are used for means-testing. It hands out cash transfers but also attacks poverty in other dimensions by conditioning cash on children's school attendance and regular visits to health centre. It also entitles people to public health care and education. The aim of the dual discourse on poverty is to incentivize investment in human capital that is thought to result in higher future income. This complex definition of poverty has resulted, arguably, in difficulties when implementing targeting instruments and in low participation in urban areas.

Both the design of targeting and implementation of targeting play an important role in determining targeting efficiency. From the design point of view, policy objectives should coherently match the instruments used for selecting their target population. And fine-tuning of objectives and instruments has proven to be difficult for CCTs that

use Means – Test Targeting (MTT), since their benefits are not only expected to target income poverty but also demand conditional behaviour from recipients. From the implementation point of view, conditionality is in practical terms an additional targeting instrument. Additional barriers to access public services the programme is conditional also affect targeting outcomes and lay beyond the scope of design of the programme.

Chapter 3 above has justified the use of the sample selection correction method to analyse the distribution of Oportunidades conditional on being eligible; and the use of neighbourhood effects to elicit information on compliance with conditionality. Section 2 of this chapter discusses hypothesis that researchers have previously posed as an explanation of low participation in urban Oportunidades. It also discusses how this research differs from previous research that focused on the marginal characteristics and average treatment effects. I also justify the dataset used. Section 3 presents background information on the analysis of conditionality and descriptive information on the rates of compliance with such conditionality in Mexico 2008.

Section 4 presents the results of the two-step sample selection model. The results highlight that means-testing is not the only ground for allocating cash transfers, and it also analyses that compliance with conditionality is not the same for all eligible households. Section 5 presents the targeting efficiency of the model proposed and compares it with Azevedo and Robles (2009) proposal. Section 6 concludes and introduces some linkages with chapters 5 and 6.

4.2 Hypothesis for non-take-up and data specifications

Coady and Parker (2009) and Coady, Martinelli and Parker (2013) follow Heckman and Smith (2004) using administrative data from Oportunidades that allow them to acknowledge three administrative steps of participation in the programme. But this administrative data is not representative at national level. Azevedo, *et al* (2009) highlight that income distribution is similar in administrative and national

representative data, but that non-eligible households are under-represented in administrative data. Angelucci and Attanasio (2006) and Attanasio, *et al* (2008) coincide in mentioning that non-eligible households differ in their characteristics in treatment and control areas in the administrative data[48].

This research follows Heckman and Smith (2004) model for estimating participation in Oportunidades as does the work of Coady and Parker (2009) and Coady, Martinelli and Parker (2013) to model take-up rates[49]. However, it is distinguished from them in using a national representative survey, the National Income and Expenditure Household Survey (ENIGH for its acronym in Spanish) 2008, which does not under-represent non-eligible households and that can incorporate validity into the analysis. As we saw in Table 2.4, ENIGH 2008 estimates that 45.6% eligible households do not receive Oportunidades, but this percentage increases to 67.5% of eligible urban households.

There is another difference with research that has previously investigated Oportunidades take-up rates. Angelucci and Attanasio (2009) obtain participation rates for Oportunidades using the Average Treatment Effect (ATE) of Oportunidades. They identified that participation is highest for households with a higher poverty index, fewer assets and rooms per person, in receipt of other governmental programmes, with more children in school age that do not work, with lower education of head of household and that live closer to school. They are cautious in mentioning that this observation indicates correlation rather than causation effect. They specifically chose the ATE method because, due to low take-up rates, there was a high probability of self-selection of participants and, then, of sample selection bias[50].

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[48] Attanasio, *et al* (2008) follow the LATE framework and find comparable results to their non-parametric model of take-up. They in fact recognize that a more accurate model of take-up of Oportunidades has not been possible to estimate due to data constraints.

[49] Attanasio, *et al* (2008), Attanasio and Rubio-Codina (2010) and Azevedo, *et al* (2009) follow Todd and Wolpin (2006) for modelling the utility of going to work or to school of eligible children in Oportunidades.

[50] The ATE allowed them to relax the assumption of absence of covariance between independent variables and error term. This is possible thanks to the existence of an instrumental variable, which is

Most of the characteristics identified by Angelucci and Attanasio (2009) are part of the means-test or eligibility model, such as assets, rooms per person, number of children in school age, and low-educated of head of household (see table 2.2) But the method that they used relaxes assumptions that would prevent them from doing prospective analysis with them. According to Deaton (2009) and Heckman and Vytlacil (2007a), economic models of choices and structural determinants are better suited for policy forecasting. This is one of the reasons why I propose the analysis of the distribution of the cash transfer conditional on eligibility, assuming that eligibility makes households more likely to participate in Oportunidades. It differs from Angelucci and Attanasio (2009) that focuses on marginal characteristics and average treatment effects. The specification of such a model implies strong assumptions that were discussed to an extent in chapter 3. Such a method may arguably contribute to offer results with external applicability for policy forecast than a research based on ATE would not.

Coady, Parker and Martinelli (2013) explored Oportunidades low take-up in urban areas and found as a main factor the relatively scanty information that eligible urban households have about the programme. They conclude that lack of awareness about own eligibility prevented households from participating in Oportunidades in 2002. However, after more than 10 years of implementation in urban areas, it is less plausible that eligible households still lack of information about the programme.

It is also worth looking at the supply side of welfare participation. By looking at the supply-side factors, Gutierrez, Bertozzi and Gertler (2003) found that targeting is more difficult to carry out in localities with larger populations, larger poverty headcounts and deeper poverty. Skoufias and Davis (2001) conclude that MTT places

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the random allocation of cash transfers, that is present in the administrative dataset of Oportunidades, that is thought to be independent of the cash transfer allocation process. Oportunidades administrative data selected random 'treatment' and 'control' groups. This survey design then allows attributing differences in such groups to the treatment effect.

a burden-cost in eligible households, which deter people from taking-up the program. Orozco and Hubert (2005) highlight the cost of the procedures included in MTT[51].

The information on Oportunidades' budget constraints is contentious. On the one hand, Campos-Vazquez, *et al* (2013) mention that the expansion of the programme has been suspended and nowadays it is possible to incorporate new households only if old ones have left the programme. In such a case, there would be a budget constraint behind the low participation in Oportunidades. On the other hand, Attanasio, *et al* (2008) confirmed with Oportunidades officials that urban areas with initially high take-up rates experienced a drop in take-up rates after the initial figures. This would not support the hypothesis of budget constraints in urban areas, pointing towards the idea that the programme is just less attractive in urban than in rural areas.

An additional source of low take-up is the performance of targeting mechanism to select the poorest beneficiaries. In a different line of research, Azevedo and Robles (2009) argue that the means-test model used for targeting is no longer suited for selecting poor households. They focus on the methodology of the means-test, but this research discusses the mismatch of the targeting method and the identification of income poverty. It argues that while there are incentives at local level to fight income poverty, there are less incentives to focus on the ones who invest less in human capital. This is explored further in chapter 6.

The opportunity cost of attending school for children and teenagers is an additional hypothesis for low take-up rates. School-aged children and young people may have other sources of income, especially in urban areas. If so, conditionality might make Oportunidades less attractive in urban areas because the opportunity cost of the forgone income-earning activities associated with schooling, and even when mothers go to the health centre. This is assessed by Attanasio, *et al* (2008), Attanasio and Rubio-Codina (2009), and Azevedo, *et al* (2010).

[51] In particular, data collection, information management, household assessment mechanisms, institutional arrangements and monitoring and oversight mechanisms.

But it is not as straightforward to argue that conditionality is a barrier to take-up. Azevedo, *et al* (2009) found that income-earning activities and school attendance are not mutually exclusive in urban areas, which would make the conditionality hypothesis less plausible. De Brauw and Hoddinot (2011) recognize that it is difficult to find information that can test hypotheses related to conditionality. De Janvry and Sadoulet (2006) and Baird, *et al* (2011) also problematize the concept of conditionality. For them, conditionality should concentrate in inducing behavioural change on the households who are less likely to attend school. Then, cash transfers should not pay for what households were already going to do. It is not straightforward to measure such behavioural change (Baird, *et al*, 2011).

This research presents an approach to understand compliance with conditionality as behavioural change among recipients of Oportunidades. It is based on the group effects model of Manski (1993). He understands that the propensity of individuals to behave in a particular way may be related to the behaviour of their most proximate group. In this way, individual endogenous self-selection may be approached (instrumented) by disentangling the influence of its peers. The assumptions of this methodology have been discussed in section 3.4, but section 4.3 below adds background information on conditionality in public policy and summarizes descriptive statistics regarding school and health centre attendance in Mexico in 2008. Section 4.4 afterwards estimates the probability model of take-up that considers household characteristics and administrative barriers to welfare.

4.3 Compliance with conditionality as a determinant of participation

Targeting efficiency is not only determined by the means test, but also by the conditions the programme imposes on recipients of the cash transfer. Oportunidades may exclude those who do not comply with the conditions[52] and eligible recipients may also exclude themselves from the programme due to the conditionality. This is why the conditioning on school and health centre attendance is explored.

[52] Localities that are far away from a school or a health centre are excluded from the programme.

One of the strongest arguments in favour of conditionality is that it is thought to constrain the current behaviour of welfare recipients and induce a behavioural change. The most relevant argument that justifies it as part of Oportunidades is that it may incentivise investment in human capital that would not otherwise be priority among poor people (Fiszbein, *et al*, 2009). Evidence on the behavioural change among Oportunidades' recipients is contentious. Oportunidades' recipients more often make use of medical services than non–recipients (Ariana, 2012). But Cunha (2010) found no difference in the anthropometric outcomes of families faced with conditional cash transfers or unconditional food transfers in rural Mexico. De Brauw and Hoddinot (2011) and De Janvry and Sadoulet (2006) argue that Oportunidades does not achieve behavioural change in primary school given the high enrolment levels.

The introduction of conditionality in a cash transfer programme is also thought to improve targeting efficiency through promoting self-selection[53]. This line of argument asserts that the administrator of the programme has limited information on the true incomes of recipients. In order to allocate transfers to the poorest households, administrators may encourage self-selection of recipients by offering inferior goods at the expense of complicated procedures that are only to be taken-up by households in need (Currie and Gahvari, 2008). But the argument has some flaws. Conditionality implies costs that are seldom considered thoroughly (Aarar and Duclos, 2006) and if costs affect targeting efficiency, there wouldn't be such a strong argument for conditionality. Whether health and education conditionality of Oportunidades increase transaction costs are empirical questions that are to be addressed in this research. Currie and Gahvari (2008) argue there is little evidence to support conditionality on pure grounds of self-selection as most conditional cash transfers implement more than one method of targeting, including self-selection.

The question that concerns us is that of targeting efficiency: have the cash transfers been assigned to the target population? It is often assumed that the condition of being

[53] Conditionality is also justified on the grounds that it may increase political support for cash transfers among tax payers. I will not explore this or other arguments.

eligible characterizes income poor households who tend to underinvest in human capital. If we do not question this assumption, we end up with the somewhat naïve question that simply enquires whether the cash transfer was allocated to the eligible population. A more rigorous approach to conditional cash transfers has to acknowledge that the target population is the one who would not have otherwise achieve this conditionality. Please note that this counterfactual considers the theory of change of the programme: cash transfers to incentivize human capital investment of poor households who would have not otherwise invested in health and education. In such a rigorous scenario, the target population is not only the one defined in terms of the means-test but also those eligible who were less likely to invest in health and education.

It is in fact difficult to isolate the effect of conditionality in such a rigorous scenario. For instance, Currie and Gahvari (2008) explain that even though Medicaid has increased take up of public health insurance among eligible children, this expansion coincided with a reduction in the number of children eligible under private insurance. There has been a crowding out of private investment in health insurance as some of those who took up Medicaid would have otherwise been insured privately. Yet, it is not possible to disentangle whether the crowding out was due to Medicaid expansion or to individual characteristics that coincided with the introduction of the programme. Similarly, it is difficult to know whether households who received the cash transfers would have taken their children to school and to the health centre even in the absence of the cash transfer. Several surveys have information on compliance with conditionality, but as far as I am aware there is no information on this particular counterfactual, while enquiring cash recipients and non-recipients themselves is impractical. Individual characteristics could be used to approach the propensity of households to send their children to school and go to the doctor, but if we do not identify the mechanism through which these households make such a decision we won't be able to test the counterfactual in which some of them wouldn't have send their children to school without the cash transfer.

Several mechanisms have been proposed to explain take up of welfare taking individual household characteristics as a departure point. They consider access to information (Coady, Parker and Martinelli, 2013), the opportunity cost of school attendance (Azevedo, *et al*, 2009) and the eligibility model (Azevedo and Robles 2009), as the main factors that explain lack of take up. Yet, it has not been questioned how take-up varies according to the fulfilment of conditions. Indeed recipients of Oportunidades may attend school and health centres more often, yet we cannot observe whether they were more likely to do so to begin with. This self-selection of beneficiaries, as explained before, is difficult not only because of the lack of data but also because the tools for analysis may vary from the traditional ones.

As De Brauw and Hoddinott (2011) had found before, there are data constrains to testing the counterfactual of conditionality. Therefore, tools from other analysis will be used to build the counterfactual. In particular, the analysis of how take up varies according to groups will prove useful for this endeavour. As explained by Zeitlin (2011), the membership to a group may influence take-up and the theoretical and methodological underpinnings have been analysed by Manski (2001). He understands that the propensity of individuals to behave in a particular way may be assumed to be related to the behaviour of other members of their most proximate group. In this way, individual endogenous self-selection may be approached (instrumented) by disentangling the influence of its peers.

Again, evidence is contentious on whether groups of peers influence take-up. Bertrand, *et al* (2000) found that propensity to use welfare increases with membership of an ethnic group that has a high propensity to use welfare. Zeitlin (2011) argues that take-up of technology for agriculture varies according to the returns experienced by the members of agricultural groups. This research relies on the assumption that an individual's behaviour is similar to that of her neighbours. If I observe that a household sends children to school and has neighbours that also send children to school, I may be in the position to infer that they would have sent their children to

school regardless the cash transfer. If we see that individual's and neighbours' behaviours are different, we may infer that conditionality of a cash transfer may matter more for such groups.

There is information in the ENIGH 2008 about children and teenagers that attend school, as well as the usage of health services, whether public or private, at neighbourhood level. I will use this information to disentangle the way in which conditionality affects take-up. Eligibility and take-up rates are also present at a neighbourhood level. Methodologically, neighbourhood characteristics proved to be a valid instrument for identification of the model, as it is discussed in section 3.2. I argue that they also contribute to a better understanding of the role conditionality plays in Oportunidades.

In order to understand the impact of the programme in changing the behaviour of households, we should answer the question of why some households decide to join the programme, that is, to send their children to school and attend the health centre in exchange for a cash transfer. The most immediate way of doing this would be to have information on changes in school enrolment and programme participation, but this is not available in the dataset being used. In order to isolate the impact of the programme, I will consider the behaviour of each household's neighbours. I assume that an eligible household is likely to behave as its neighbours do. I will also assume that if she doesn't, it is due to the incentives of the programme.

For instance, if an eligible household gets the cash transfer and decides to send its children to school, this is assumed to be due to the cash transfer if we can observe that the household lives in a neighbourhood in which most children do not attend school. In calculating average behaviour in the neighbourhood, I consider the primary sampling units (unidades primarias de muestreo) as the neighbourhood of each household. I am trying to control for 'near-the-household behaviour' in order to understand whether most of the eligible households are behaving the same way as the

eligible ones who take-up the programme. If they are, conditionality is not binding. If they are not, conditionality may be the main reason behind take-up.

The sample selection correction model used in section 3 below includes a set of neighbourhood characteristics $\beta_j \bar{x}_j$ that provide information on the endogenous effect of conditionality at the moment of claiming the cash transfer. These are the proportion of children and teenagers attending school in the neighbourhood and the proportion of households that had at least one member's weight's measured in the last 12 months.

These variables have been included in the analysis under the hypothesis that they portray the endogenous group effects that, according to Manski (1993), screen household's behaviour. Endogenous characteristics are the ones for which individuals adapt their behaviour towards an outcome of interest that then shapes that particular neighbourhood characteristic. The theory of change behind Oportunidades is a good example. If eligible households decide to go to the monthly health check-ups in order to get the cash transfer, we will observe that neighbourhoods in which they live have higher rates of health centres attendance. We could label this as an endogenous effect induced by cash transfers.

Drawing on Manski (1993), it is the endogenous effect which has interest for public policy as it implies the decision to change behaviour in order to pursue an outcome. If Oportunidades eligible households decide to send their children to school in order to get the cash transfer, and so do their neighbours, we would be in the presence of an endogenous effect of school attendance. In this endogenous effect, the cash transfer incentivizes human capital investment.

4.3.1 Descriptive statistics on school conditionality

It is important now to summarize descriptive statistics about the compliance of conditionality in Mexico in 2008. Firstly, I would like to draw on the categories of targeting that shape our analysis, that is eligibility and cash receipt of urban and rural households, and understand the rates of compliance for each of those categories.

Cash recipient are those households who received a strictly positive income from Oportunidades in the past months. As no administrative information is included in this survey, it is not possible to know whether those who in the end received the cash transfer where the same who claimed the benefit and were enrolled in the programme. The errors arising from these administrative processes are not modelled due to the lack of information and are assumed to be randomly distributed, although further research may explore this puzzle on its own right.

Table 4.1 recovers some information of tables 3.4 and 3.5 in section 3 above. It summarises the mean proportion of households that have either children or teenagers of school age[54] and that send them to school, as well as the mean proportion of households who do the same in their neighbourhood. Although levels of school attendance are slightly lower in rural areas, disaggregation by urban and rural areas does not change the analysis and conclusion drawn. In order to avoid repetition I do not present a similar table for rural areas. The information is disaggregated by the four categories of targeting.

TABLE 4.1 AVERAGE PROPORTION OF HOUSEHOLDS AND NEIGHBOURS THAT COMPLY WITH EDUCATION CONDITIONALITY BY HOUSEHOLD ELIGIBILITY AND CASH RECEIPT

Households	Mean proportion of school-age children that attend school (std. error)	Mean proportion of school-age children that attend school in the neighbourhood	Mean proportion of teenagers that attend school (std. error)	Mean proportion of teenagers that attend school in the neighbourhood (std. error)
Eligible cash-recipients	0.89	0.88	0.66* (0.02)	0.64
Ineligible cash recipients	0.94* (0.006)	0.89	0.74* (0.02)	0.7
Eligible non-recipients	0.83* (0.01)	0.87	0.54* (0.02)	0.66
Non-eligible non-recipients	0.9	0.9	0.8	0.79* (0.02)

Sample size: 28,463,012households
Source: Own calculations based on ENIGH (2008) and Oportunidades (2010).
* Statistically different from the rest of mean estimates at a 0.05 level.

[54] In Mexico, the starting age of compulsory education is 4 years old, although for Oportunidades cash transfer is conditional on education until third grade, when children are around 9 years old. Compulsory education lasts for 11 years to cover pre-primary, primary and lower secondary education, until pupils are around 15 years old. Oportunidades offers support to students that continue higher secondary education for three additional years, when pupils are around 18 years old. The limit age to which Oportunidades offers support is 21 years old. UNESCO (2013) and Oportunidades (2010).

School attendance among children is higher than among teenagers, reaching up to 95% for ineligible recipients. The fact that almost all households' children attend school has been used as evidence to suggest that behavioural change may not be any longer induced in primary school. But it is worth noticing that school attendance rates decline for eligible households. And if we compare own school attendance and neighbourhood school attendance, we get a more complete picture. Recipients seem to attend school more frequently than their neighbours. But this is not true for eligible non-recipients. Teenage school attendance among eligible recipients does not differ from their neighbourhoods rates. And ineligible recipients' teenagers also attend school more frequently than their neighbours.

However, eligible non-recipients show statistically significant lower proportions of children and teenage school attendance if compared to other households and if compared to their own neighbours. Compliance with conditionality does differs across these four groups of targeting and we can hypothesize that conditionality might represent a burden for eligible non-recipients. This evidence is not strong enough yet to establish a causal effect in which conditionality leads to exclusion of households, but discussing it in a broader context would contribute to making the argument stronger[55].

Eligible households, in general, have more children than non-eligible ones. This is confirmed intuitively given the fact that the eligibility model considers children as one of its proxy variables. This may explain why it is more difficult for eligible households to comply with primary school attendance if they have more children to begin with. But interestingly, households that get the cash transfer have significantly more people aged between 12 and 18 years old, which is the common school age of secondary education that Oportunidades supports. Entitlement to cash transfers

[55] An additional hypothesis behind this evidence is that current ineligible households were eligible years before the survey of 2008. I do not have panel data to analyse the evolution of households supported by Oportunidades to find out whether ineligible households in 2008 were eligible households supported before. But according to Cortes, *et al*, (2007) and Campos-Vazquez, *et al* , (2013), only a very small proportion of households change their eligible condition over the time they have received Oportunidades.

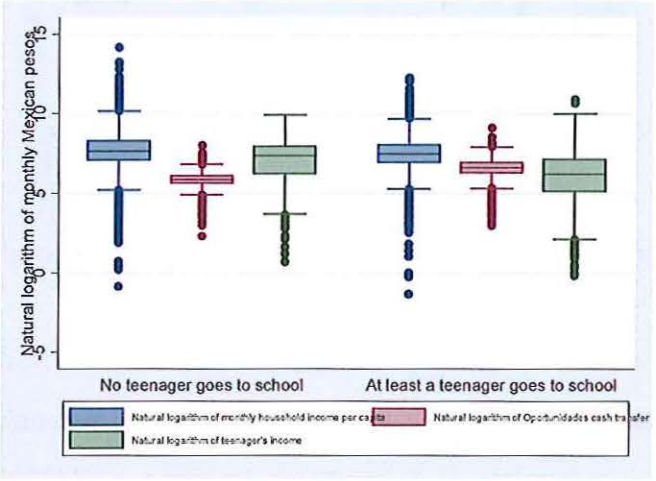
doubles for teenagers that continue from primary to lower secondary education, and may even treble if students continue to higher secondary education. Cash incentives might be attracting teenagers whether eligible or not, to claim the benefits. This would explain the different demographic composition of Oportunidades recipients at teenage, but the underinvestment of eligible excluded households remains unexplained.

Figure 4.1 summarises the distributions of the natural logarithm of monthly income, cash transfer and income received by teenagers, by teenage school attendance. For visualisation purposes, households in which teenagers receive no income have been dropped from it. In fact, only 27% of households with teenagers receive income from them, which means that the majority of households should be graphed with a value of 0 in the box plot. Yet, this drives the median value towards 0 and does not show the median earning of the teenagers that actually earn some money. This 'censored' median value is useful as I want to know the income that most of those 27% teenagers expect to earn. The purpose is to compare this value with the value of the cash transfer. Yet, the reader must bear in mind that the distribution of teenagers would change if we consider all teenagers, and not only those who earn some income.

In the blue boxes of the left, income median value does not differ for both groups although monthly income may reach higher values for households with no teenager attending school. The median Oportunidades cash transfer is higher for households in which teenagers attend school and it also reaches its highest values in that group. Yet, the median income brought by teenagers is lower in households in which at least one teenager attends school. Teenage income reaches its highest value in school attendance households, but its median value is lower than the median cash transfer. In contrast, the median value of the teenage income in households with no school attendance is higher than the median value of the conditional cash transfers in any group. This is an indication of the set of incentives that excluded eligible households face: if Oportunidades is claimed, some income may be forgone. The income from the cash transfer may not be as high as the income brought home by teenagers, and this

income will be even lower if at least one teenager attends school. It is important to mention that, generally, teenagers combine Oportunidades cash receipt with other types of earning activities, which is also confirmed by Azevedo, *et al*, (2009).

FIGURE 4.1 DISTRIBUTIONS OF MONTHLY HOUSEHOLD INCOME, CASH TRANSFER AND INCOME BROUGHT BY TEENAGERS, BY TEENAGE SCHOOL ATTENDANCE

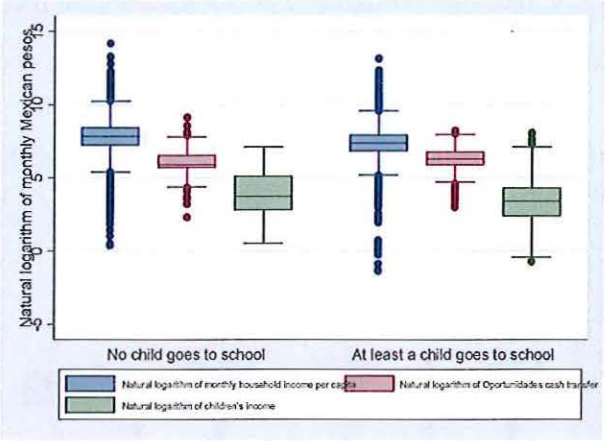


Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

If the cash transfer works partially as an incentive for teenagers, it does not have the same effect for children as the cash transfer is significantly lower. Although levels of teenage income are slightly lower in rural areas, the analysis and conclusion drawn are not different from the overall picture; hence I do not disaggregate this figure, and the following one, by urban and rural areas.

Figure 4.2 presents the distribution of the natural logarithm of household income, Oportunidades cash transfer and children's income. We see that children's income would hardly reach levels of Oportunidades cash transfer, and cash transfer would not reach levels of household income. Incentives other than cash transfer may be at stake for households to send children to school.

FIGURE 4.2 DISTRIBUTIONS OF MONTHLY HOUSEHOLD INCOME, CASH TRANSFER AND INCOME BROUGHT BY CHILDREN, BY CHILDREN SCHOOL ATTENDANCE



Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

School attendance is the condition that has captured the attention of recent research about Oportunidades. The evidence presented above leads us to conclusions similar to the ones presented by Azevedo, *et al* (2009) and Attanasio, *et al* (2008) [56]. The cash transfer is not enough to cover the opportunity cost of continuing secondary education for eligible non-recipients. However, I have highlighted the importance of conditionality at primary school age, and the importance of conditionality for non-eligible recipients at secondary school age.

4.3.2 Descriptive statistics of health conditionality

Oportunidades rules of operation establish that recipients of the cash transfers should register in the nearest health centre, attend regularly programmed health check-ups, and attend monthly health meetings with other Oportunidades recipients (Oportunidades, 2008). But less research has been carried out on compliance with the health conditionality. Conveniently, ENIGH 2008 includes some information on usage of health services: monetary and non-monetary health related expenses, type of

[56] Attanasio *et al* (2008) assume that work and school attendance are excluding conditions for adolescents between 12 and 18 years old, whereas Azevedo *et al* (2009) have evidence of work and school attendance not being excluding conditions and that Oportunidades recipients combine both activities.

institution they frequent or are affiliated to, preventive health care and reasons for refraining from health assistance when needed[57].

The importance of health conditionality is strongly justified by table 4.2 below, that reproduces the proportion of households and neighbours who had their weight measured in a visit to the doctor over the past 12 months of the survey. This is taken as a proxy measure for preventive health care, and Oportunidades conditionality is geared towards promoting preventive health practices according to the rules of the programme. The last column highlights that preventive health practices have a statistically similar rate in neighbourhoods regardless the category of targeting we are looking at. But individual preventive health practice is higher for cash recipients than for non-recipients, and is also higher than the average of their neighbours. Less frequency in preventive health practices is particularly worrying among eligible non-recipients, who show lower frequencies than other categories and lower than their neighbours.

TABLE 4.2 AVERAGE PROPORTION OF HOUSEHOLDS AND NEIGHBOURS THAT HAD PREVENTIVE HEALTH CHECK-UPS BY HOUSEHOLD ELIGIBILITY AND CASH RECEIPT

Households	Proportion in which at least one member had her weight measured in the previous year (std. error)	Proportion of neighbours that had one member had her weight measured in the previous year
Eligible cash-recipients	0.96	0.93
Ineligible cash recipients	0.97	0.93
Eligible non-recipients	0.87* (0.01)	0.91
Non-eligible non-recipients	0.93* (0.002)	0.93

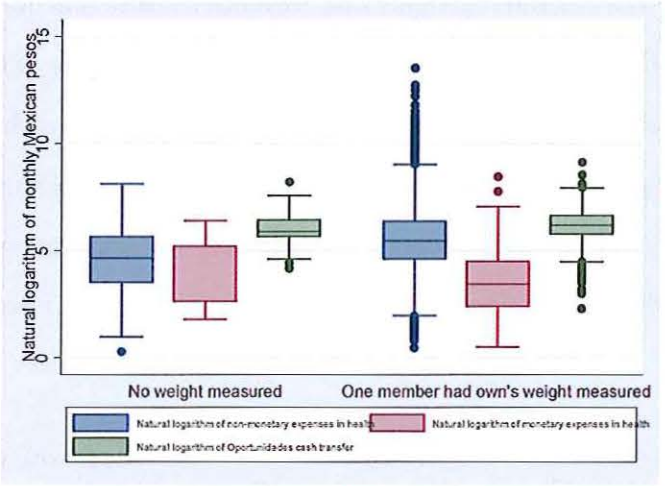
Sample size: 28,463,012households
Source: Own calculations based on ENIGH (2008) and Oportunidades (2010).
* Statistically different from the rest of mean estimates at a 0.05 level.

Figure 4.3 below show us that households who had no preventive health practice in the past year (weight not measured of any member) have both monetary and non-monetary health related expenses of similar median value. But those households with preventive health practice had higher median non-monetary health expenses than

[57] It also includes information on time to get to the health centre but the information had many missing values and I have decided not to use it for this analysis.

monetary ones. Non-monetary health expenses are incurred when health services are used for free. A monetary value is assigned to those services received. The provider is usually a public institution, although some charity and solidarity services are also included in the non-monetary category. Still, the usage of health services reaches higher values, both monetary and non-monetary, for those with preventive health practice over the year previous to the 2008 survey.

FIGURE 4.3 DISTRIBUTIONS OF NON-MONETARY AND MONETARY HEALTH EXPENSES AND CASH TRANSFERS BY PREVENTIVE HEALTH PRACTICE



Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

An additional way of assessing the importance of conditionality as a factor that determines participation can be assessed by understanding the extent to which it is observed among eligible non-recipients. We have discussed that they observe the lowest levels of investment in human capital. Yet, some of those households still invest in human capital and do not receive the programme. Table 4.3 provides a summary of school and health check-up attendance of eligible households excluded from Oportunidades. I have divided the table in between households that have no school-age offspring that represent 7.7% of the eligible non recipients, and those who do. Each category adds to 100%. Most of them comply with health conditions even though they do not receive the cash transfer. Moreover, 75% of households send at least one child to school and 57% send at least one teenager to school. It is puzzling that they do not claim benefits despite of showing school and health check-ups

attendance. I also bear in mind that excluded households have an average of 2 school-aged children per household which coincides with 2 average school-aged children of eligible recipients. They tend not send all of them to school, but at least one, similarly to eligible recipients.

If households do not send children or teenagers to school, the cash transfer reduces significantly, but they are still entitled to a part of it. Among those who have no school-age offspring, attendance to health check-ups is significantly lower 33% than among those who do (between 7 and 12%). These are probably the most vulnerable group as they are eligible to benefits in cash and health care that they are not claiming.

TABLE 4.3 SCHOOL AND HEALTH CHECK-UPS ATTENDANCE AMONG ELIGIBLE NON RECIPIENTS

Eligible non-recipients	Proportion in which at least one member had her weight measured in the previous year	Weight not measured
No children or teenagers	0.67	0.33
Households with children		
No child attend school	0.18	0.01
At least one child attends school	0.75	0.06
Total	0.93	0.07
Households with teenagers		
No teenager attends school	0.32	0.06
At least one teenager attends school	0.57	0.06
Total	0.89	0.12

Sample size: 2,701,778 excluded households
Source: Own calculations based on ENIGH (2008) and Oportunidades (2010).
* Statistically different from the rest of mean estimates at a 0.05 level.

In conclusion, conditionality compliance occurs among eligible excluded households, as 67% of households observe at least one form of conditional behaviour. I could think that conditionality does not seem to be a determinant of take-up. But the programme considers behavioural change in conditionality. That is, the ability of changing behaviour towards human capital investment. As is discussed below, behavioural change does matter in explaining take-up and recipients consistently show behaviour more prone to human-capital investment than non-recipients.

The ENIGH 2008 also has information on the reasons why people did not receive health assistance last time they had a medical emergency. In 2008 7% of households who suffered an illness in the previous year refrained from medical assistance. Table 4.4 presents some of the reasons of those who refrain from medical assistance in the last year. Household could mention several reasons at the same time and then percentages represent frequency rather than proportion of responses. Most of them (33.6%) refrained from medical assistance because it was either too costly or they didn't have enough money to pay for it; 21.76% did it because they found administrative barriers, that is the health centre was closed, too far away or it did not have medicines. A fourth of those households (26.12%) did not want to go to the doctor because they thought it was not necessary, and 7.73% did it because they were mistreated in the health centre, didn't trust the doctor or the doctor did not speak their language.

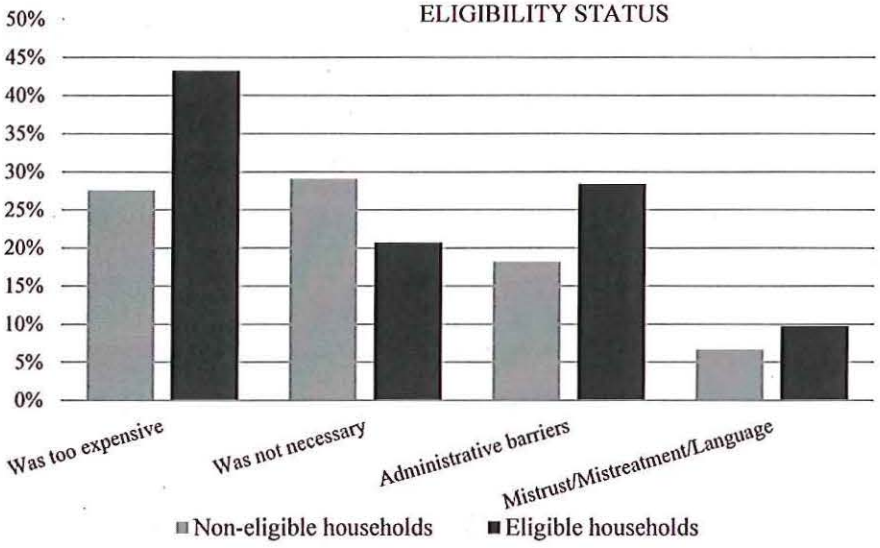
Figure 4.4 shows figures from table 4.4 disaggregated by eligibility. For non-eligible households, the most frequent reason mentioned was that they thought it was not necessary to go to the doctor, closely followed by households who faced an administrative barrier. In contrast, for eligible households, the monetary cost of going to the doctor was more frequently mentioned as the reason to refrain from going to the doctor, followed by administrative barriers. Mistrust, mistreatment or misunderstanding of language was more frequent among eligible than among non-eligible households.

TABLE 4.4 HOUSEHOLDS WHO REFRAIN FROM MEDICAL ASSISTANCE

	Total	Non eligible	Eligible
Didn't receive medical assistance last year because:	1,991,275	1,291,520	699,755
Was too expensive	33.1%	27.59%	43.26%
Was not necessary	26.12%	29.08%	20.66%
Administrative barriers	21.76%	18.18%	28.38%
Mistrust/Mistreatment/Different language	7.73%	6.65%	9.71%

Sample size: 28,463,012households
Source: calculations based on ENIGH 2008

FIGURE 4.4 HOUSEHOLDS THAT REFRAIN FROM MEDICAL TREATMENT BY ELIGIBILITY STATUS



Source: Own calculations based on Oportunidades (2010) and ENIGH 2008.

4.3.3 Evidence of behavioural change

As mentioned in section 2.7, the aims of the programme are both to increase the capability set and the socioeconomic opportunities of the target population. The capability set is understood as investment in health and education, whereas the socioeconomic opportunities are understood as income poverty for practical purposes. Both aims would seem coherent and compatible at first sight, but with the benefit of hindsight I argue that, in terms of efficiency, they may be at odds. I explain this contradiction below.

It is assumed that eligible population are not only income poor but they also lack from access to health, education and assets. It is also assumed that, to some extent, it is income poverty what prevents them from investing in human capital. Arguably, eligible households would benefit from investing on health and education, and they would benefit the most from a cash transfer that pays for the opportunity cost of such investment. Oportunidades is built on this assumption, as the cash transfer is not offered with the aims of redistribution, but conditional on human capital investment.

This conditionality is problematic in terms of efficiency, as the eligibility score does not necessarily identify those who are willing to comply with the conditions of the programme if barriers other than income poverty prevent them from doing so. We could think of eligible households as facing three scenarios. A first group of eligible households would not be willing to comply with conditions at the price of the cash transfer and may not claim the programme. Those eligible households that did not invest in human capital did not change their behaviour. Some other eligible households would comply with conditions and obtain the cash transfer. Among them, a second group would have complied with the conditions and invested in human capital regardless the allocation of cash transfer. I argue such eligible households did not change their behaviour either as a result of the cash transfer. A third group of eligible households would have complied with conditions of human capital investment only due to the presence of the cash transfer. Such group is the one that changed behaviour as a result of the cash transfer. I argue so as those eligible households would have not invested in human capital had they not received cash in exchange.

If the aim of the programme is to incentivise human capital investment, the programme could pursue the most efficient allocation of cash transfers if it targeted those households that would have shown behavioural change. It would be more efficient to target those who would have not invested in human capital otherwise, rather than targeting those who would have invested in human capital anyway. Unfortunately, the mean-test targeting model is not designed to identify those characteristics of eligible households.

The concept of behavioural change may refer to change in behaviour over time, assuming that by observing changes from one period to the next we could assess behavioural change. Yet, there are caveats in measuring behavioural change across time. If we measure health centre or school attendance before and after the intervention, we would observe changes over time for those who invested in human

capital. However, it would be difficult to disentangle those who would have done it regardless Oportunidades' cash transfer from those who did it only due to cash receipt. The most efficient allocation is to assign the cash transfer to the latter group. But even if we measure outcomes over time, we cannot discern between households with similarly observed outcomes but different reasons for such behaviour. In fact, even if you gather information on such underlying causes for human capital investment, you may obtain biased information as some household would confirm the importance of the cash transfer even though it had not been crucial for their investment to happen (Baird, *et al*, 2011). We would need further information to disentangle those who are already prone to human capital investment before the intervention from those that needed cash incentives to attain behavioural change. It is this particular definition of behavioural change that is difficult to capture, even with nuance information of behaviour over time.

The observation of outcomes in different points in time offers only partial information on our concept of behavioural change. This is why it is necessarily to disclose household's behavioural change by other means of information. I argue that the group effects model of Manski (1993) can provide with an heuristic understanding of behavioural change.

Manski (1993) poses the assumption that individuals tend to behave in the same way as their most proximate group. This assumption is used here to elicit information on behavioural change. In this sense, we could assume that individuals who invest at a similar level of human capital investment would cluster together. But if a household shows a different level of human capital investment than mean household in its cluster, I argue that household shows behavioural change. Yet if a household show similar level of human capital investment than the mean household in its cluster, I argue such household was likely to invest in human capital levels regardless the cash receipt.

Such information on behavioural change, although questionable, is important. It may contribute to discern those who would not have invested in human capital had they

not received a cash transfer from those who would have done so. It may also contribute to elucidate whether the cash transfer has caused investment in a change in behaviour towards investing in human capital.

In this research I define those peers as the households that inhabit the same primary sampling unit. I have labelled such group as the neighbourhood, because the primary sampling unit is defined in the ENIGH 2008 as houses located in the same block or up to 250 households if the block contains more households. The assumption of homogeneity in the primary unit of sampling has been discussed in section 2.7.

It is true that this measure is not exempt of problems. One scenario is that legitimate behavioural change occurs among eligible individuals in the neighbourhood, but if it happens at the same time and with similar strength for all of them. This behavioural change would not be identified as such by the measure used here. Moreover, in Manski (1993) framework, identifying behaviour different than peers does not necessarily imply a causation link. Other reasons may be behind this differentiated behaviour, which have nothing to do with the causal link of the treatment and the outcome.

If I observe a change of behaviour occurs in a neighbour of eligible households, I argue that it is a legitimate behavioural change as it occurs among the most deprived households. Similarly, if I observe a change of behaviour among ineligible households, doubt can be cast upon such behavioural change, as ineligible households arguably have high levels of human capital already. In the end, the analysis below shows that eligible households tend to underinvest in human capital compared to their neighbours, which was expected from the general theory of change upon which Oportunidades is built.

The relative importance of the different sources of conditionality is determined both empirically and by the logic of the programme. As discussed above, the variables that proxy for compliance with conditionality show higher rates for health than for

education. 92.5% of households had a member's weight measured in the past year while only 89.6% of children in school age attended school. However, differences between health and education conditionality have at least two additional sources.

School conditionality comprises 85% of daily school during the school year; health conditionality comprises attending a monthly health talk and regular visits of members of the household to the doctor following a previously agreed appointment schedule (Gonzalez-Flores, *et al*, 2012).

The first difference is demographic. In order to be able to comply with school conditionality, the household must comprise of children or teenagers aged 3 to 18 years old. 31.5% of households in Mexico do not have a member in school age. Looking at the total of those households, only 11.7% are eligible, 8.94% receive the cash transfer, and most of them (86%) had a preventive health check up in the last year, although this is true for only 79% of eligible households without school-age member, compared to 94% of eligible households with school-age members.

A second important difference in the administrative processing of conditionality exists. According to De Janvry and Sadoulet (2006), lack of compliance with school conditionality does not necessarily determine immediate termination of cash transfers. However, lack of attendance to one of the health appointments or monthly health talks may cause immediate termination of the cash transfer. This would point towards a relative higher importance of the health conditionality, if it weren't for the fact that recipients are not officially notified of this differential treatment but receive the information that both conditions are equally biding. Therefore, although the behaviour of recipients is expected to be equal for both conditions, we expect to see a higher compliance rates for health than for school conditionality.

Still, an important part of conditionality is to incentivize change of behaviour in favour of human capital investment. As mentioned before, the delivery of cash transfers is justified on the fact that income poor households tend to under invest in

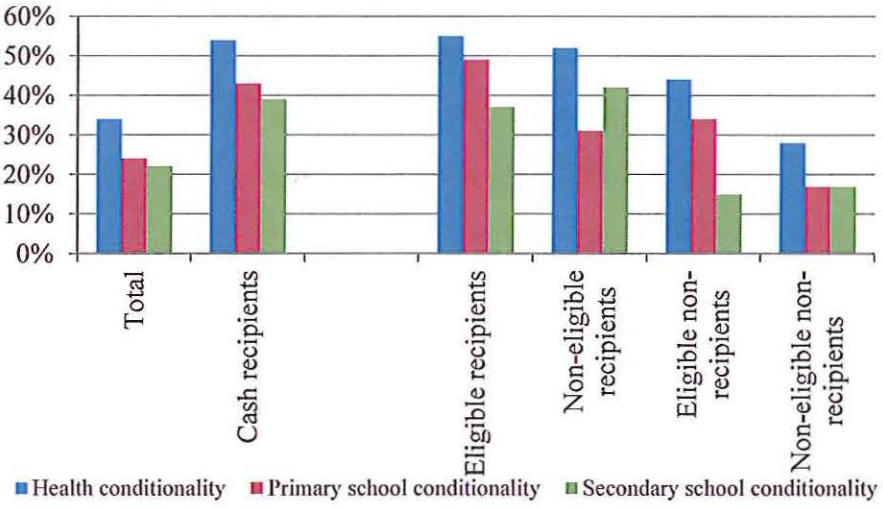
health and education. Therefore, along with actual levels of health and school attendance, we should also focus on the extent to which the behaviour of households, especially poor households, has changed in favour of activities that enhance their human capital. As Baird, *et al* (2011) discusses, it is difficult to measure such a change of behaviour in a simple survey question and researchers have to find different ways of eliciting such information from respondents. Zeitlin (2011) understands that we can appeal to the assumption that households would observe behaviour similar to their neighbourhood. I have assumed the peer group to be the neighbourhood or block in which households live. Therefore, I have defined that households may have changed their behaviour if they show higher health and school attendance than the mean attendance of households in their neighbourhood.

In this scenario, I have estimated the proportion of households that show health attendance higher than its neighbours is 34%. But it reaches 54% for cash recipients and 50% for eligible households, compared to 30% among non-eligible households. This may speak of the importance of the change of behaviour towards investment in health, although there are several caveats to this measure. The first one is that one could object to the neighbourhood as the primary peer-group reference for households. As mentioned before, neighbourhoods are considered as the primary unit of sampling in the survey and are explicitly selected based on their homogeneity. Yet, other relevant peer references could be acknowledged, such as other neighbours, ethnicity, kinship, gender or age of head of household, and so on, none of them have been considered in this research.

I have also estimated the proportion of households with children or teenagers of school age that show higher rates of school attendance than their neighbours. These are 24% for households with children and 22% for households with teenagers. Figures are lower than health attendance and they have a different composition. 43% of cash recipient households showed a higher rate of child school attendance than their neighbours, and this reaches as high as 49% for eligible cash recipients, while 34%

eligible non-recipients show this behaviour and 31% of non-eligible recipients. Interestingly, 39% of cash recipient households with teenagers attending school showed higher rates of secondary school attendance than their neighbours, but theirs reached 42% among non-eligible cash recipients, and it is lower for eligible recipients (37%) and even lower for eligible non-recipients (15%). Figure 4.5 attempts to summarize this information.

FIGURE 4.5 PROPORTION OF HOUSEHOLDS WITH HEALTH AND SCHOOL ATTENDANCE HIGHER THAN THEIR NEIGHBOURS, BY CATEGORIES OF TARGETING



Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

Blue bars show the proportion of higher-than-neighbours health attendance, while the red colour shows primary school attendance and the green colour displays teenage school attendance. Blue bars are higher for every group, highlighting again the importance of behavioural change in health conditionality as it is the easiest to achieve. Primary school attendance tends to be higher than secondary school attendance, and higher red bars in figure 4.5 indicate that it is easy to observe behavioural change in primary education, i. e. higher child school attendance than own neighbours. Yet, for non-eligible recipients, behavioural change in secondary school education seems easier to achieve. For them, it is more likely to observe teenage school attendance higher than their neighbours, as indicates a green bar

higher than a red bar for this group. This is interesting given the fact that teenage school attendance is the lowest of conditions achieved.

On the one hand, I hypothesise that a more frequent change of behaviour for that particular group indicates that they obtain larger benefits from attending secondary education more than their neighbours. This could obey several factors. Firstly, the cash transfer for each student in secondary education is larger than in primary education. But this cannot explain a larger behavioural change as it does not occur among the eligible recipients. It then may be more likely that non-eligible households achieve higher level of education if they are better off than eligible households. Alternatively, they can also show this particular behaviour if they live in mostly-eligible neighbours or if they follow the behaviour of a better-educated peer-group that is not precisely their neighbours. Lastly, we have to remember that non-eligible households tend to have less number of members and it may be easier to invest in their education.

On the other hand, this speaks about the difficulties in incentivizing change of behaviour for investing in higher levels of education. Eligible recipients do change their behaviour with respect to secondary education, but less frequently than with respect to primary school and health attendance. Eligible non-recipients change their behaviour with respect to secondary school attendance less frequently than the rest of the population, and the cash transfer does not seem to be enough incentive to break this behaviour. This also hints towards the existence of unobserved costs and barriers that prevent eligible non-recipients to change behaviour.

As a final note I observe less frequent behavioural change for non-eligible non-recipients. This group includes households without school-age members and higher educational levels. Therefore we may observe lower behavioural change in school attendance among them. Those costs may be important at determining targeting efficiency, as it is discussed in section 6.4 below. Regarding health conditionality, non-eligible non-recipients in fact do not show higher attendance to preventive health

check-ups than the rest of the groups. In fact, they show a similar pattern of health care spending as non-eligible recipients, which is higher than eligible population. Yet, they have the lowest levels of health check-ups. Without neglecting the limitations of our measurement, comparisons in this chapter lead us to conclude that Oportunidades may have generated a behavioural change in terms of health conditionality.

Although previous research has contentious evidence about the importance of conditionality in CCTs, I have described succinctly an innovative method of approaching information on the behavioural change incentivized by Oportunidades. I do so by looking at the neighbourhood rates of compliance with conditionality and comparing them to the individual rates. Descriptive statistics support the hypothesis of conditionality as a factor that determines participation in Oportunidades. This section has also highlighted the importance of focusing on the four categories of targeting in order to understand differences in behavioural change for human capital investment, as eligible households are investing less in human capital, and eligible excluded households are investing less than their own neighbours, which is particularly worrying. The following section presents the result of the sample selection correction model and the way in which eligibility, conditionality and other neighbourhood factors contribute to explaining participation in Oportunidades.

4.4 Sample selection correction model of participation in Oportunidades

The aim of this section is to discuss the intuition behind the model of participation and present the results of the model. Methodological assumptions and its justification were discussed in sections 1 to 4 of chapter 3. I have argued that we should approach the distribution of cash transfers as it is conditional on eligibility of the model. I argue that the sample of participation in Oportunidades is not random because the eligibility in the rules of the programme causes a bias in it. That is, we cannot see 'offers to participate' in Oportunidades for non-eligible households. Some may argue that in fact we do observe them, as there are non-eligible recipients of the programme. I

would argue that even those ineligible recipients are near to the eligibility threshold[58], so there must be incentives to apply the means-test rule most of the time a cash transfer is allocated.

In sections 3.2 and 3.3 I explained that once the mean inverse mills ratio in neighbourhood was included as an instrument, I was able to reject the hypothesis of sample selection. I also explained that the econometric test for sample bias is simple but contentious[59]. Nevertheless, I justified the use of the sample selection method because it also considers explicitly the non-take-up of eligible households and allows us to frame our analysis using the four categories of targeting efficiency; and we also gain insight on the errors of targeting by comparing the sample selection and other methodologies.

Assuming that households who claim are eligible and those who do not claim are not eligible, that is if perfect targeting efficiency is assumed, sample selection of recipients should be present on the grounds of the characteristics that make them eligible[60]. So the absence of sample selection bias also indicates that MTT is limited at explaining cash transfers allocation and processes other than eligibility matter. Therefore, I think of the second step of the model as the allocation step, in which I include factors that are related to cash receipt but that are not correlated with eligibility. Such factors include conditionality but also barriers to conditionality and other neighbourhood effects.

Coady and Parker (2009) and Coady, Martinelli and Parker (2013) also follow Heckman and Smith's (2004) model in using an administrative dataset and

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[58] Among non-eligible claimants, the 50% with highest eligibility score and closer to the eligibility threshold (i. e. the most deprived 50 per cent considering eligibility as the definition of deprivation), have a mean eligibility score of 0.4 when the threshold is 0.69.

[59] On the one hand, if the ρ coefficient of the inverse mills ratio is different from 0, we are in the presence of sample bias, as there is a 'different-from-0' correlation between the non-selection hazard and the residuals of the model. On the other hand, I had we may observe a 'different-from-0' correlation if any of the two steps is incorrectly identified. I obtained a 'different-from-0' correlation before including the neighbourhood effects that contributed to improving the identification of the model.

[60] Sample selection bias could happen on grounds different from eligibility, such as income poverty, awareness, migration status; I have not tested for eligibility on these other grounds.

considering three steps: an awareness step, an application step and an acceptance or enrolment step[61]. In table 4.5 I summarize their results, including the participation rate of eligible and non-eligible households. Coady, and Parker (2009) account for targeting efficiency using the targeting differential that has been discussed in section 6 of chapter 3[62], which basically consists of the difference between the participation rate (i.e. eligible take-up) and the proportion of non-eligible claims.

TABLE 4.5 COADY AND PARKER (2009) ELIGIBILITY AND PARTICIPATION RATE IN URBAN OPORTUNIDADES

	Aware	Apply	Enrolment
Participation rate of eligible households (PRE)	72.2%	60.4%	50.4%
Participation rate of non-eligible households (PRNE)	50.9%	31.3%	19.8%
Targeting differential (TD = PRE-PRNE)	.213	.291	.306

Source: Coady and Parker (2009).

Coady and Parker (2009) estimated that among the eligible people in 2002, 72.2% know about Oportunidades but 11.8% are deterred from applying and only 60.4% apply. A further 10% of eligible people who knew and applied are rejected by the means-test selection process, which gives a participation rate (or take-up rate) of 50.4% at national level. According to Coady and Parker's (2009), the means test seems to be rejecting a similar percentage of eligible people (10%) as non-eligible people (11.5%). For them, reducing the proportion of excluded eligible people would imply increasing awareness of the programme among ineligible people and this could increase the leakage of resources in ineligible claims.

There is one caveat in this analysis. The administrative data they use (ENCELURB for Encuesta de Evaluacion de los Hogares Urbanos 2002) was gathered among communities that were treated with the programme or that could serve as control for the programme. ENCELURB is not representative of the whole population. I proposed in section 4.2 to use the ENIGH 2008 in order to obtain better estimation of

[61] There is an argument for including eligibility as a step on its own since awareness of the programme is partly determined by a positive self-assessment that potential applicants make of their own eligibility.
[62] Coady and Parker (2009) also have an indicator of vertical efficiency. They obtained the marginal utility assuming a welfare function in terms of household consumption, in which low-income households have higher weight and in which transfers do not represent any cost for cash recipients.

participations rates at national level. In table 4.6 I lay out the results presented already in tables 2.8 and 2.9 in section 2. I also include the targeting differential in the last row. This measure is discussed more extensively in chapter 6.

TABLE 4.6 ELIGIBILITY AND PARTICIPATION RATE IN URBAN OPORTUNIDADES

	Urban
Participation rate of eligible households (PRE or coverage rate)	33%
Participation rate of non-eligible households (PRNE or inclusion error)	4%
Targeting differential (TD = PRE-PRNE or coverage – inclusion error)	.29

Source: own calculations based on Oportunidades (2010) and ENIGH 2008.

Only 4% of non-eligible households in Mexico get Oportunidades, in contrast to 19.8% estimated by Coady and Parker (2009). Meanwhile, they estimated 50.4% in Coady and Parker (2009), but table 4.5 shows a lower participation rate of 33%. Oportunidades in urban areas seems to do better at vertical efficiency (PRNE) but worse at horizontal efficiency (PRE) in my estimations than in Coady *et al* estimation. However, national representativeness comes at the cost of parsimonious information as ENIGH does not include questions on awareness of application for Oportunidades.

This model has two steps: 1) eligibility and 2) receiving the cash transfer. I use a sample selection correction model as to correct for selection bias among eligible households. The probability of participation in the programme depends on the probability of being eligible and on the probability of receiving the cash transfer conditional on being eligible. The first step of the model considers if households are selected according to the eligibility (means-test) model. The second step is defined in practical terms as factors that explain cash receipt beyond the means test. In this second step I have included individual characteristics, usage of health services and neighbourhood effects of conditionality. I have also included information on whether households answer that they would have easy access to people who could help them to improve their community, as a proxy for social networks. I also included information on internal migration of household members (with reference to five years before the survey), the natural logarithm of the value of entitlement to Oportunidades for each household according to the rules of the programme, the natural logarithm of

the value of non-monetary health expenses and the natural logarithm of the income brought by teenagers into the household. The mean values for rural and urban areas for each of the independent variables included in the model are presented in table 4.7. A full description of the minimum, maximum, median, variance and skewness values for each of this characteristics is provided in Appendix C.

TABLE 4.7 DESCRIPTIVE STATISTICS OF THE DEPENDENT AND INDEPENDENT VARIABLES OF THE MODEL

Variables	Mean	Urban mean	Rural mean
Dependent variable			
Oportunidades cash receipt (dichotomic)	0.17	0.07	0.53
Instrumental variables			
Predicted probability of cash receipt in neighbourhood	0.17	0.07	0.53
Residuals of the predicted probability of cash receipt at neighbourhood level	-0.1	-0.12	-0.04
Mean inverse mills ratio in neighbourhood (only included in the first step)	6.38	7.63	1.92
Inverse mills ratio at household level	5.08	6.06	1.56
Eligibility first-step			
Number of people per room	1.87	1.7	2.46
Demographic dependency	0.73	0.67	0.93
Female head of household	0.24	0.26	0.19
Number of children aged 0-11	0.96	0.89	1.22
Age of head of household	47.72	47.46	48.65
No social security benefits	0.48	0.41	0.74
No education of head of household	0.09	0.07	0.19
Incomplete elementary education	0.21	0.17	0.37
Shared or no toilet	0.13	0.09	0.26
Toilet without running water	0.27	0.2	0.51
Dwelling with dirt floor	0.07	0.03	0.19
Household without gas or electrical stove	0.11	0.05	0.35
Household without refrigerator	0.18	0.12	0.41
Household without washing machine	0.49	0.42	0.73
Household without car or truck	0.58	0.54	0.72
Cash receipt second-step			
Endogenous effects (conditionality)			
Teenagers attend school	0.75	0.78	0.65
Teenagers in neighbourhood attend school	0.75	0.78	0.65
School-age children attend school	0.9	0.9	0.88
School-age children in neighbourhood attend school	0.9	0.9	0.88
Had a preventive health check-up in the last 12 months	0.93	0.93	0.92
Proportion of neighbours who had preventive health check-up	0.93	0.93	0.92
Indigenous language spoken in household	0.1	0.06	0.23
Natural logarithm of the value of entitlement	6.04	6.8	3.34
Natural logarithm of income brought by teenagers into the household	6.34	6.37	6.26

Source: ENIGH (2008)

We see that for the group effects tested, the neighbourhood has a mean value very

similar to the mean value at individual level, which is expected at an aggregated level, except for teenage and children school attendance. This is due to the fact that 33% of households in urban areas do not have members of school age (3 to 18 years old) and this drives down the mean value in aggregate terms.

As we mentioned before, the probability of participation is estimated based in a model that considers the joint probability of being eligible and on the probability of getting the cash transfer conditional on being eligible. We have :

$$\Pr(eligibility = 1|X) * \Pr(receipt = 1|\, eligibility = 1, X)$$

In which Pr (eligible=1) is the selection equation. As discussed in section 2 of chapter 3, the identification of the model relies on the assumption of normality of the probit and on two exclusion restrictions or instrument variables the use of which has also been justified.

The methodology followed is summarized in the following steps:

1) A two-step model at neighbourhood level is estimated to obtain instrument variables for the individual level model:
    a. In the first step, individual eligibility is regressed on the individual variables defined by the means-test model and the average neighbourhood rates for those same variables and the distance between individual and its neighbourhood rate. From this first step, the inverse mills ratio or non-selection hazard for each household is used to compute the average inverse mills ratio in each block of each household. This will be the exclusion term in the eligibility regression or first step regression of the model proposed. The inverse mills ratio increases for households who live in neighbourhoods with few recipients, and it is informative of:
        i. eligible households that are less likely to have neighbours who know the procedure.

ii. eligible households in regions with lower probability of being eligible. These are regions that have a coefficient that penalises eligibility scores of households in those regions. As discussed in chapter 6, the region that concentrates the poorest households in Mexico has no penalisation and from there, penalisation increases as regions increases in affluence.

iii. It has a Pearson correlation coefficient of 0.688 with the household mills ratio.

b. In the second step, the model regress the proportion of recipients in the neighbourhood on the average characteristics of the neighbourhood and the individual inverse mills ratio. The prediction and residuals of this regression are also used in the second step of the individual level model. This is the control function approach proposed by Rivers and Vuong (1988) and also followed by Zeitlin (2011). This is an additional approach to deal with the endogeneity of, for example, eligibility and cash receipt. The residuals come from an instrumented estimation of an endogenous variable. According to Imbens and Wooldridge (2007) , cited by Zeitlin (2011), the inclusion of these residuals 'control' for the endogeneity of the endogenous variable. However, the residuals are not statistically significant in the individual regression, probably because they provide a similar effect to what is already being controlled by the household inverse mills ratio. The proportion of eligible households in the neighbourhood proved to be an statistically significant control variable for endogeneity and it is also correlated to the residual of this first neighbourhood level model.

2) An individual two step model that:

a. Runs a probit sample selection model of eligibility regressed on the means test characteristics and that includes the average mills ratio of the neighbourhood. The latter is known as the exclusion term that is excluded from the second step of the model.

b. The exclusion restriction also allows the inverse mills ratio to act as a correction sample selection in the second step. Sample selection is present in step 1a.

c. The second step regresses the probability of getting the cash transfer conditional on households being eligible and on their inverse mills ratio, but also on neighbourhood effects of correlated, contextual and endogenous effect of their neighbours that contribute to understand factors beyond eligibility that determine cash receipt.

d. Once the exclusion restriction, neighbourhood contextual and endogenous effects have been included, I cannot reject the null hypothesis that the ρ coefficient is 0. Then sample selection is questioned when the model is fully identified.

e. The model at national level cannot explain take-up at state level.

    i. Technically, the variation of the penalisation scores is necessary for the first step of the model to be estimated. When we limit the model at regional level, there is little variation in the eligibility equation and it cannot be estimated.

    ii. Without any variation, the inverse mills ratio cannot serve as a control for 'unobserved' characteristics that determine both eligibility and cash receipt. The model cannot be fully identified

    iii. It was only possible to estimate the model for the state of Puebla, which in terms of means-test contains five regions with different penalisation scores. It was also possible to estimate a similar the model, which has no information on social networks, for the northern states. Again, variation in eligibility scores of different areas allowed the convergence of the software calculations.

    iv. Although several authors recommend generating standard errors with bootstrap methods, this was not possible to do due to hardware constraints.

As mentioned at the beginning of section 4.3.3, neighbours' eligibility is assumed to be correlated with individual eligibility to the extent to which Manski's (1999) assumption of peer behaviour holds. This assumption indicates that individuals tend to show a similar behaviour than their peers. If their peers are the group living near to them, that is their neighbours, eligible households could be assumed to show similar behaviour than their neighbours. Given this correlation, neighbours' eligibility could serve as an instrument for individual eligibility if I can sustain that it does not explain individual participation rates. This is also confirmed on table 3.3, as it does not contribute to explaining allocation of cash receipt on a simple linear regression model. Still, the exclusion restriction should not only be based on a statistical test but should be sustained by theoretical reasoning. I have been encouraged to spell out the exclusion restriction in practical terms. In such case, I need to argue for neighbours' eligibility to be correlated to individual eligibility but to be independent from individual allocation of cash transfer.

Previous models used the monthly entitlement to cash transfer as an instrument for eligibility, as it was argued that demographic characteristics and entitlement cannot be deliberately changed by households in a small period of time and would not determine cash receipt. However, the argument for this exclusion restriction becomes weaker as the period of implementation becomes larger. Over several years, households may adapt their demographic characteristics (number of children or elderly in the household) in order to increase their entitlement.

I argue the mean inverse mills ratio in the neighbourhood is a good instrument for individual eligibility. And I argue the exclusion restriction as follows. In 2007, due to budget constraints the number of recipients was set to 5 million households. The process of 'incorporation' started, in which eligible households would only be enrolled once an equal number of households have left the programme. In this way, the number of recipients would be maintained at 5 million. Based on Manski (1999), the mean inverse mills ratio in the neighbourhood would still be correlated to

individual's eligibility, but individual cash receipt of households incorporated between 2007 and 2008 would be determined by the spaces available in the community[63]. Indeed, this exclusion restriction would only be valid for those who were not receiving cash transfers before 2007. Yet, it makes it plausible that, for 2008, mean inverse mills ratio of neighbours' eligibility may capture variation on household eligibility that is not correlated to cash receipt. Other factors that may determine cash receipt are conditionality, budget constraints or stigma.

According to Zeitlin (2011), neighbours' decisions have to be properly instrumented in order to identify the variation in neighbourhood's take-up that is independent of neighbourhoods' eligibility score. The average inverse mills ratio in the neighbourhood would be an invalid instrument for individual's eligibility if it is not causing cash receipt. In such case, the instrument z may also determine the outcome y, in which case it would not be independent of the residuals of both steps (u, v). In this case, the instrument would not be valid as the expected value of the u conditional on the instrument is 0. Intuition also supports my assumption of no partial effect of the instrument on cash receipt. Neighbourhood's eligibility does not determine cash receipt as:

a) Table 3.3 presents plausible instruments. For each of them an OLS regression model is run on the natural logarithm of cash receipt. The average inverse mills ratio of eligibility for the neighbourhood fails to significantly explain cash transfer allocation in this regression analysis.

b) If households want to adapt their eligibility towards their neighbours' eligibility in order to get the cash transfer, they would need information on the demographic characteristics and living conditions of those neighbours. This is plausible to know in rural areas. Yet, in urban areas, it is more difficult that this information is available

[63] While Gonzalez *et al* (2009) and Campos-Vazquez *et al* (2012) found that those who leave the programme are close or above the eligibility threshold and that this attrition problem does not show a particular bias against certain group.

among neighbours, given the complexity, variety and concentration of population in urban neighbourhoods.

c) Although distance proximity is related to the formation of friendship and networks (Preciado, *et al*, 2008), the neighbourhood may not be a salient reference group for individual behaviour, and this may be argued in urban areas with larger concentration of people. As Daraganova, *et al* (2011) explain, the effect of distance in network affiliation is unknown *apriori* and further research is needed to understand the mechanisms in which distance determines behaviour.

d) Moreover, as Manski (1993) explains, although there might be a correlation between peers' characteristics (in this case neighbours), causality cannot be implied from this correlation. In such case, a common cause behind both, such as individual eligibility, would relate instrument and outcomes but would not bias the correlation between the residual of the estimation and the instrument.

c) Since 2007, a budget constraint prevents eligible households from being enrolled on the programme if no other household have stop claiming the cash benefit.

Unfortunately, u is not fully observed so we cannot corroborate whether we observe correlation or not, but the justification above leads me to assume that the correlation between the average inverse mills ratio of eligibility in the neighbourhood and the residual from the cash receipt estimation is 0.

The model that identifies neighbours' decision making includes as dependent variable the mean cash receipt in the neighbourhood $\theta\bar{y}_j$, which is explained by independent variables that include:

a) the mean value of each characteristic of the eligibility score in the neighbourhood,
b) the distance between each individual characteristic and the mean value of such characteristic considered above, and
c) the inverse mills ratio of eligibility to correct for sample selection bias of eligibility

The resulting residuals and inverse mills ratio from the latter estimation are then averaged at neighbourhood level. Such neighbourhood averages are the values used in the final models estimated in sections 4.4 and 5.3.1 of this research.

Results and the syntax to obtain them can be shared upon request. Table 4.8 presents the marginal effects of the sample selection correction model.

TABLE 4.8 SAMPLE SELECTION CORRECTION FOR URBAN AND RURAL CASH TRANSFERS CONDITIONAL ON ELIGIBILITY

Independent variable	Marginal effects (dy/dx) change on dependent variable P (Receipt | eligible)					
	National		Rural		Urban	
Predicted probability of cash transfer in the neighbourhood	0.62	***	0.88	***	0.48	***
Residuals of eligibility at neighbourhood level	-0.97	***	-1.25	***	-0.80	***
Mean inverse mills ratio in neighbourhood	-0.49	***	-0.29	***	-0.39	***
Inverse mills ratio for households	-0.09		-0.11		-0.16	
Eligibility first-step						
Number of people per room	0.01	***	0.02	***	0.01	*
Demographic dependency	0.004		-0.01		0.01	
Female head of household	0.004		0.02		-0.01	
Number of children aged 0-11	-0.02	**	-0.02	*	-0.02	**
Age of head of household	0.003	***	0.00	***	0.002	***
No social security benefits	0.02		0.05		-0.02	
No education of head of household	0.09	***	0.11	***	0.07	***
Incomplete elementary education	0.06	***	0.09	***	0.04	**
Shared or no toilet	0.09	***	0.14	***	0.05	*
Toilet without running water	0.08	***	0.14	***	0.03	
Dwelling with dirt floor	0.000		-0.03		0.03	*
Household without gas or electrical stove	0.001		0.00		0.01	
Household without refrigerator	-0.01		-0.01		-0.01	
Household without washing machine	0.01		0.00		0.02	
Household without car or truck	0.04	**	0.03		0.06	*
Rural effect	0.10	***				
Cash receipt second-step						
Endogenous effects (conditionality)						
Teenagers attend school	0.12	***	0.10	***	0.12	***
Teenagers in neighbourhood attend school	-0.03	*	-0.05	*	-0.02	
School-age children attend school	0.07	***	0.05	***	0.09	***
School-age children in neighbourhood attend school	-0.10	**	-0.09	**	-0.11	**
Had a preventive health check-up in the last 12 months	0.17	***	0.37	***	0.11	***
Proportion of neighbours who had preventive health check-up	-0.22	***	-0.35	***	-0.10	
Indigenous language spoken in household	0.05	***	0.08	***	0.03	*
Natural logarithm of the value of entitlement	0.03	***	0.05	***	0.02	**
Natural logarithm of income brought by teenagers into the household	0.00		0.00		0.00	*
Sample size	64200		16063		48137	
Dummies for state level effects	Yes		Yes		Yes	
Rho	-0.09		-0.11		-0.16	
Sigma	1		1		1	
Predicted participation	0.18		0.58		0.10	

Source: Own calculations based on ENIGH (2008)

*** significance at 0.001 level

** significance at 0.05 level

* significance at 0.01 level

The first step of the model, which aims to estimate the probability of being eligible, is not reported in the table above. Only the marginal effects of the second step of cash transfer allocation are reported in table 4.8. The second column in the table reports the marginal effects at national level and the third and fourth columns present the marginal effects for rural and urban areas. Overall, the effects of the eligibility indicators are larger for rural areas than for urban ones, reflecting the fact that there are more eligible recipients in rural than in urban areas. But the marginal effects of the education conditionality are larger in urban areas.

The inverse mills ratio in the neighbourhood, which is the instrumental variable for the first equation, is excluded in the second step but its coefficients in the table. All of them are significant and powerful too. Other instrumental variables included in the second step are the predicted proportion of cash transfer allocation for the neighbourhood and its residuals, which have powerful and consistent effect that is larger in rural areas.

From the variables of the means-test model, only number of people per room, children aged 0-11, age and education of head of household, toilet without running water and lack of car or truck are statistically significant in the final model. The marginal effect of those variables is very small in urban areas. In table 2.2 above we saw that female-headed households have a negative weight in the eligibility score. This is because the statistical relationship between household income and female head was positive when the means-test was estimated in rural areas[64]. But in the table above we see that the coefficient for female-headed households is not different from 0 in any of the models.The 4 most important variables that determine both eligibility and cash receipt

[64] Female headed households in rural areas tended to have more income earners (generally working age individuals employed) and a lower dependency ratio.

are the ones related to education of head of household, shared or no toilet and having a car. Again, the effect is higher for rural areas.

Interestingly, the number of children has a negative marginal effect in cash receipt of between 1 and 3% reduction in the probability of receiving the cash transfer. This may be due to the fact that households with more children find it more difficult to send them to school and do not receive the cash transfer despite of having a higher eligibility score and entitlement. This also confirms the conclusions in section 4.3, in which we discussed that eligible households have more children than non-eligible households. And eligible non-recipients send at least one but not all of their children to school, hence their low levels of child-school attendance. The most powerful effect is the rural effect. As I discuss in chapter 6, this is due to the fact that the MTT assigns higher weight to rural areas as it considers there is higher income poverty among them

The second step in the model is conditional on the sample selection that results from the first step, which reduces the unweighted sample of households from 64200 to 12731. When we look at the second-step marginal effects, a few things are worth noticing.

The effect of conditionality can be interpreted by looking to both household investments in human capital and neighbourhood variables to understand the extent of behavioural change. Regarding attendance to health check-ups, those who have preventive health check-up at household level may have a 17% higher probability of receiving the cash transfer, which goes from as low as 11% in urban areas to up to 37% in rural ones. This indicates that recipients comply with health conditionality. The neighbourhood coefficient does have a negative effect at national level and in rural areas, but is not different from 0 in urban ones. This indicates that eligible urban cash recipients do not behave particularly differently from non-recipients in their neighbourhoods in terms of health check-ups. This also confirms our results in the last column of table 4.2 where I mentioned that neighbourhoods are not different from

each other in terms of rates of health check-ups. Then, in rural areas, eligible recipients do have more frequent check-ups than their neighbours (presumably eligible non-recipients). Put it differently, in urban areas, eligible non-recipients comply with health conditionality and those who do not and reported to have faced monetary constraints to attend health centres, are less likely to receive cash transfers. I then hypothesise that cost may prevent behavioural change among eligible urban households.

The marginal effects of teenage school attendance are positive for urban and rural areas. But the marginal effects of neighbourhood proportions of teenage school attendance are negative in rural areas, which indicate that the lower the school attendance in the neighbourhood, the higher the probability of receiving the cash transfer. Put it differently, recipients actually attend school in higher proportions than their neighbours, which I consider an indication of behavioural change. In urban areas[65], eligible recipients who attend secondary school do not have behaviour that is statistically different from their neighbours. This may point towards thinking that conditionality at secondary school is not encouraging teenagers to attend school more frequently that they would have done anyway.

Interestingly, the effect of child school attendance is positive and very consistent for urban and rural areas. Yet, the effect of the neighbourhoods' child school attendance is negative and significant only at national level and in urban areas. This indicates that child school related behavioural change may be occurring precisely in urban areas. This conclusion is in tune with what we had seen in table 4.1, in which on average eligible non-recipients have consistently lower school attendance than the neighbourhoods in which they live. Behavioural change then matters to explain

[65] When the regression only includes households who either have children or teenagers, the coefficient and marginal effect of neighbours attending primary school is negative and statistically significant. This would point toward the importance of primary school conditionality if we interpret that eligible households who receive the cash transfers tend to have eligible neighbours who do not send their children to school. Yet, a regression only on households with school age members drops the unweighted sample from 12671 to 4252, so I decided to present the results for whole sample of eligible households.

participation in Oportunidades. Behavioural change in child-school attendance is more important at explaining take-up in urban areas.

Conditionality matters in increasing the probability of receiving cash transfers once you are eligible, and the highest effect is for teenage school attendance, which is understandable as they also have the highest entitlement and the lowest mean rate of compliance, as we see from last column in table 4.8. However, the measured behavioural change is not different from 0. This speaks to whether cash recipients are investing more in human capital because of the cash transfer. There is evidence that eligible households have a statistically different behaviour from their neighbours in terms of health in rural areas, and in terms of child-school attendance in urban ones. For teenage-school attendance behavioural change, the evidence is only significant at national level, but not at rural or urban areas. Health related behavioural change is associated with cash receipt in rural areas, although it does not make a difference in urban ones.

This also points towards the difficulty of screening those who would attend to school regardless the conditions from those who would not. I argue that eligibility is a good screening mechanism as eligible households show less marginal investment in health and education, as we saw from tables 4.7 and 4.8 above.

It is worth now comparing these results with the results of the linear regression for the full sample included in table A1 of appendix A. A linear regression on the whole population at national level, and urban and rural areas confirms the statistical significance of positive marginal effect of conditionality and negative marginal effects of neighbourhood effects on the probability of getting the cash transfer. Yet, the linear regression using the OLS method yields lower marginal effects than the sample selection model. In table A2 I also report the probit regression marginal effects. I see that the negative sign of the neighbourhood effect is uncertain for child school attendance. Other research have argued that it is precisely child school attendance where you see the less benefits of the cash transfer as households would have sent

children to school anyway (De Janvry and Sadoulet, 2006; Attanasio, *et al*, 2008). However, I argue that by focusing on the eligible households and acknowledging that they are less likely to invest in education to begin with, there is evidence of behavioural change among eligible households. Put it differently, eligible households who do not claim the benefits also tend to underinvest in school attendance and are not attracted to send children to school more frequently. This speaks of the behavioural change of those eligible who by taking up the benefit, invest more than those who do not take it up. The overall conclusion is that conditionality and behavioural change explain participation in Oportunidades, but behavioural change cannot be taken for granted among eligible households who are the less likely to change their behaviour in favour of complying with the conditions of the programme.

Speaking an indigenous language could prevent people from understanding the procedure to get Oportunidades, but it does not. In fact, being indigenous population increases the probability of receiving the cash transfer by 4% at national level, but it reaches 8% in rural areas and 4% in urban ones.

Eligible households are also more likely to receive the cash transfer if they have a larger entitlement, although the marginal effect of entitlement is very small. The natural logarithm of the income earned by teenagers has a positive but not statistically significant effect in the sample selection model[66]. Yet, if we have a look at table A1, they do have statistically significant marginal effect in determining cash transfer receipt but the coefficient is very small, almost negligible.

After discussing these results, I argue that participation in Oportunidades depends not only on the probability of being eligible but also on the capability of complying with conditionality, of speaking indigenous language, of residing in the same administrative state over the last 5 years, on the entitlement according to the rules of the programme and on the usage of public health services. Eligible households are less likely to receive the cash transfers if they do not comply with conditionality.

[66] Azevedo *et al* (2009) had found a significant effect regarding this variable.

However, conditionality does not seem to improve human capital investment among eligible households who receive the cash transfer, as their investment in human capital is not statistically different from their other eligible neighbours.

4.5 Targeting efficiency of the sample selection correction model

Following Azevedo and Robles (2009), I compare the errors of targeting for each of the two steps of the model in table 4.8. I summarise this in the targeting differential, discussed in section 6 of chapter 3. I also include the Azevedo and Robles (2009) proposals for an up-to-date-means-test and multidimensional model.

Table 4.9 presents the distribution of urban households along each quintile of the income distribution and the programme's respective coverage for each of the quintiles. The comparison considers the lowest 16.6% of the distribution for each of the models as there is an allocation of budget to 16.6% of Mexican households. The distribution of Oportunidades cash transfers in 2008, using the current MTT model, is progressive as half of the resources (51%) are allocated to the first quintile of the income distribution. Azevedo and Robles (2009) updated the coefficients of the MTT model using 2006 ENIGH. This updated model, evaluated with 2008 data, would have a better performance than the current MTT model by selecting 64.7% of the poorest households. The remaining rows show the first step and second step of the model, disaggregated in rural and urban areas. We see that the first step is more progressive than the second step. Moreover, the model is more progressive in rural areas than in urban ones. Still, between 60% and 75% of the cash transfers are predicted to be allocated to the two lowest quintiles of the income distribution.

TABLE 4.9: DISTRIBUTION OF AND TAKE-UP OF ELIGIBLE URBAN HOUSEHOLDS - DIFFERENT MODELS

Model	Household income per capita quintiles					Total
	20%	40%	60%	80%	100%	
Distribution						
Oportunidades 2008	51%	28%	14%	6%	1%	100
MTT updated (only urban)	64.7%	21.5%	9%	3.6%	1.2%	100
Rural areas						
First step selection	66%	22%	8%	3%	1%	100
Second step allocation	59%	25%	10%	4%	1%	

Urban areas						
First step selection	46%	33%	15%	5%	1%	
Second step allocation	43%	34%	15%	7%	1%	
	46%	29%	16%	8%	2%	100

Source: Own calculations based on ENIGH 2008 and Azevedo and Robles (2009)

Yet, we must bear in mind that income distribution is useful for evaluating vertical efficiency of the model. However, targeting differential is better at summarizing errors of exclusion or non-take-up and errors of inclusion or non-eligible take-up. The first column of table 4.10 presents errors of exclusion for urban households; the second column presents errors of inclusion; the third column presents the targeting differential. Again, the comparison is done considering the model selects the lowest 27.5% of households of the distribution. The first row of table 4.10 below shows (as in table 4.6) that the current targeting differential is 0.29. The updated model of Azevedo and Robles (2009), in the second row of table 4.10 would have fewer exclusion errors but slightly higher inclusion errors than the current model. Still, it would give a better targeting differential. From our sample selection model we see that, if the cash transfer was only allocated on the basis of means-test, the exclusion and inclusion errors would decrease significantly. Yet, in the second step we see that both errors increase considerably. Comparing the different models, we can see that none of the models achieves perfect targeting. Yet, all of them increase the inclusion errors. In the end, targeting differential is better for any other model than the current allocation.

TABLE 4.10 TARGETING ERRORS AND DIFFERENTIALS OF MODELS OF TAKE-UP

Model	Errors of exclusion	Errors of inclusion	Urban targeting differential
Oportunidades 2008	45.6%	7%	0.47
MTT updated-only urban (A & R 2009)	51.1%	9.2%	0.397
First step selection	14.8%	0.5%	0.85
Sample selection model	39.9%	5.5%	0.55

Source: Own calculation based on ENIGH 2008 and Azevedo and Robles (2009)

The main conclusion from the above analysis is that the current MTT selects the poorest population with a high exclusion error that can be improved. However, this

may come at the cost of increasing inclusion errors (Coady, Martinelli and Parker, 2013).

Coady, Martinelli and Parker (2013) found that targeting inefficiency is caused by administrative features of the programme, whereas Angelucci and Attanasio (2009) found that targeting inefficiency is due to the lack of incentives for participants. But in this chapter I have argued the importance of the sample selection model to highlight the contribution of conditionality to explaining targeting efficiency of urban Oportunidades.

4.6 Conclusion

One of the working hypotheses was that means-test targeting determines participation in Oportunidades. And our sample selection correction model explores the distribution of cash transfers conditional on being eligible. However, I cannot confirm the existence of sample bias due to the means-test model and this indicates that the allocation of cash transfers cannot be fully explained solely by reference to means-testing. In the presence of perfect targeting, no other variable should influence take up, and benefits would be assigned on variation of individual characteristics. But this does not occur. In fact, among those who are eligible, there is a lower probability of receiving the cash transfer if the proportion of eligible households in the neighbourhood is higher.

This would suggest that the means-test model and the cash transfer receipt happen independently, supported by the fact that the residuals from these two steps are not correlated. Azevedo and Robles (2009) argue that this is due to the erosion in the relationship between the weights of the means-test and the selection procedure. But in this chapter I have demonstrated that, conditional on eligibility, households have higher probabilities to claim benefits on the basis of their compliance with conditionality. Affiliation to health insurance is the factor that contributes more than the means-testing to improving the probability of cash receipt. But the contextual

effect of peers affiliated to health insurance was negatively related to the probability of cash receipt, which implies the cost of only having health insurance without having Oportunidades. Other household characteristics increase the probability of cash receipt, such as access to public health services, expected benefits of the programme, indigenous language spoken, access to solidarity networks and residence in the same state over the last 5 years.

Conditionality is set to incentivise the use of health and education services, and impact evaluations of the programme show evidence of positive outcomes of this investment in both, the health and education dimensions. However, the counterfactual of whether conditional cash transfers change the behaviour of Oportunidades recipients has seldom been tested. The analysis above shows that recipients do increase their human capital investment, but these investments are not statistically different from the ones of their eligible neighbours. After conditioning the sample on eligibility, compliance makes no difference to the take-up of health and education among eligible households. The question remains why they do not invest more than their neighbours or than ineligible households.

This questions the theory of change upon which Oportunidades is based and that poses income constraints as the main limitation of investment in human capital. The link between disadvantaged groups and underinvestment in health and education is widely known although the exact mechanisms that influence such remain unknown.

With regards to income, it is known that lack of health is correlated with low income earning and may subsequently contribute to poor children's health and education, and low human capital when they proceed to adulthood. This corroborates the idea of poverty being transmitted intergenerationally, and the idea that once an income constraint is alleviated by a cash transfer, the cycle could be broken straightforward.

Yet, the effects of income on health have not been clarified. As Jack (2011) explains, in most research on adults, income may have such a minimal effect in health that it does not induce further health investment from households.

This is particularly true in environments where high mortality rates decrease the benefits of additional investments in human capital. In particular, this was true for the lack of success in effort to eradicate malaria in Latin America (Bleakley, 2006 *cit* by Jack, 2011).

On the same tune, disadvantaged socioeconomic background may be correlated with low school and health outcomes. Yet, the mechanisms under which this occurs are varied. Although health status may be related to higher wages, but the wage level was fairly similar between those who had a highly nutritious supplement and those who did not in Guatemala (Hoddinott, *et al*, 2008). Geographic association is correlated with socioeconomic disadvantage but the mechanisms vary from geographical concentration of health care facilities, to psychosocial mechanisms or to historical discrimination of disadvantaged groups. Having such a varied mechanisms to explain the correlation between space and health and educational outcomes makes it necessary to elaborate beyond the merely spatial correlation (Cutler, *et al*, 2011).

If increasing income and socioeconomic status cannot fully explain higher investments in health and education, it is then doubtful how a conditional cash transfer may on its own trigger such investment. Further research on the social mobility of Oportunidades' participants highlights that participants tend to find job opportunities similar to their parents despite the fact of being better educated (Yaschine, 2012). In such scenario of lack of social mobility, incentives in the labour market may be needed to encourage higher educational levels. More fundamentally, the current scheme would not be the only factor influencing households' decision making on human capital investment. Further research is needed in the precise mechanisms that underlie this interaction between human capital and socio-economic opportunities.

As would be expected, eligible households who do not comply with conditionality are less likely to receive the cash transfer; and they also show lower levels of investment in human capital. Conditionality may prevent them from receiving Oportundiades. If we are to consider rigorously the theory of change of the programme, cash transfers should incentivize human capital investment among those who would have not otherwise invested in health and education. A question can be raised about the effect of conditionality among eligible households, as it may discourage participation of some eligible households while not incentivising a significant investment in health and education.

The relationship between conditionality and non-take-up of benefits leads me to enquire about whether take-up may beget costs that are difficult to observe. Chapter 5 investigates these questions by exploring the costs of participation in Oportunidades as a latent variable. I explore the association of those costs with the four different categories of targeting, and with conditionality compliance, in order to understand the mechanism which leads to participation in Oportunidades.

These conclusions make us reflect about the notion of what good targeting is. The programme gives welfare benefits but it also seeks to induce behavioural change through conditionality. But if households are not taking up the programme, how should targeting change in order to induce self-selection of those households willing to change their behaviour and invest in human capital? I have argued that, conceptually, a model such as the means-test is better suited to identify households that underinvest in human capital over an income measure. Despite of being tuned to income poverty, it contains information on investment on human capital, and this is useful to identify recipients of a human capital programme such as Oportunidades.

Still, the means-test cannot explain cash receipt due to the conditionality on human capital investment, to some extent. This also points towards the difficulty of screening those who would attend school regardless the conditions from those who would not. I argue that eligibility is a good screening mechanism but it should be regarded in the

terms of human capital investment. If there are incentives to target income poor households instead, targeting may not be working towards selecting low-human-capital households but income-poor-households. As discussed in chapter 6, these populations are not the same at all.

Targeting efficiency then may be discouraged by conditionality. Paradoxically, while the means-test contributes to identifying households who are less likely to invest in human capital, there is no evidence that conditionality increases those investments beyond the levels achieved by other eligible households living in the same neighbourhood. Although conditionality compliance guarantees cash transfer, it may be that those who receive the cash transfers were more likely to attend school and health centres in the first place. In the meantime, conditionality does not encourage higher investments in human capital from the ones that are less likely to do them. Those eligible households are excluded from the programme, contributing to targeting inefficiency.

In the introductory chapter 2, I proposed that this research would look at the demand and supply side factors behind take-up of urban Oportunidades. I now argue that conditionality is not enough to incentivise behavioural change among eligible recipients while at the same time discourages eligible non-recipients who find it more difficult to comply with conditionality of the programme. Given the structure of benefits, individuals still shape their participation in the policy. I argue that it is the interaction of three spheres of analysis —individual characteristics, structural features and policy design and implementation in a specific context— that, in the end, determine take-up of urban Oportunidades.

The model presented above may explain a small proportion of the variance of take-up conditional on eligibility. This is somewhat discouraging. Yet, the cautious words of experts in this field acknowledge that a definitive hypothesis test of take-up of welfare benefits is difficult to carry out due to information constraints. Limitations on methodologies also exist. As discussed before, the methodology proposed is argued to

improve the analysis of participation by focusing at the distribution of cash transfers conditional on eligibility. On the one hand, this take-up model considers explicitly the errors of exclusion or eligible non-recipients. The marginal effects have direct interpretation to that problem, and it is particularly enlightening when analysing the hypothesis of conditionality. It can help us to target households that underinvest in human capital. It is worth highlighting that fine-tuning of means-test would improve the coverage rate and, arguably, the impact of the programme. Chapter 6 further discusses the relationship between impact and targeting efficiency.

5 WHAT CAN THE EXPECTED VALUE OF CASH TRANSFER TELL US ABOUT THE COST OF *OPORTUNIDADES*?

5.1 Introduction and rationale behind the latent variable

Welfare programmes almost never reach 100% of the total target population. Sources of targeting inefficiency are individual household choices or official selection criteria. In chapter 4 I have argued that participation of eligible urban households in Oportunidades is systematically determined by conditionality. However, it is important to consider there may be causes behind welfare participation that are difficult to observe by researchers and policy makers. At the moment of estimating welfare participation, we may enquire to what extent unobserved causes are at stake in determining take-up of welfare. We may have reasons to think that sample selection in welfare benefits is a manifestation of an unobservable latent variable that does not allow us to see the counterfactual outcome of those households that are selected into treatment.

In the same way, we may suspect that selection bias is caused by factors (such as the cost of claiming, the potential receipt of the transfer or the random allocation of treatment) that determine participation. If this selection bias affects participants and non-participants in equal random terms, we could think that the important omitted information occurs at random. Then, the error term of the equation would be normally distributed. In such a case, the omitted information would not bias our estimation even if we cannot observe it, measure it or include it in the estimation (Wooldridge, 2009, p. 254).

Yet, we may also argue that the latent variable affects participants and non-participants not at random but heterogeneously (Zeitlin, 2010c). If we suspect a latent variable causes sample selection, we will not be able to hold the uncounfoundedness assumption in which the expected outcome for a person who did not receive the

treatment is equal for both eligible and non-eligible households (Zeitlin, 2010b). On such occasions, by dealing with self-selection and the error term in an explicit way not only can we better understand the underlying causes of the bias but also we can choose the best method to deal with it. Not to mention that self-selection in cross-sectional analysis, such as the one used in this research, causes biased coefficients because the relationship between variables is estimated based on a biased sample. And it also limits the external validity of the analysis as results cannot be generalised to a wider population if the sample of participants is not representative to begin with (Cuddeback, *et al*, 2004).

Can we measure what we cannot observe? Not only is this a rhetoric question but it also helps me to introduce the methodology used for analysing the error in modelling take up of conditional cash transfers. Truly unobservable latent variables can be expressed as a vector of observed regressors that imperfectly approximate the latent variable with error added to it. However, measurement error can also be simply derived from inaccurate measurement. Distinguishing between the two is not entirely straightforward but in simplified terms, we would expect that measurement error is uncorrelated to the true variable if it doesn't occur systematically for certain values of the true variable. We would also expect that pure measurement error is uncorrelated with the error of the equation (u) (Wooldridge, 2009, p. 319). However, if we are portraying measurement error as evidence of the existence of a latent variable, we cannot assume that the error is independent and randomly occurs but that it is systematically related to other variables and the error term of the equation (u), which makes it endogenous[67]. The last specification is similar to the grounds for justifying the use of a sample selection correction method. According to Heckman (1979) a sample bias does not allow us to observe the full distribution of positive outcomes but

[67] Technically, selection bias and endogeneity are two different problems and disentangling them depends on the judgement of the researcher, that is, the 'appropriate' answer depends on how the researcher believes a latent variable affects participation in welfare. If we assume that we only observe participation of individuals who self-select into the programme, we have a sample selection bias. Conversely, if we believe the latent variable is relegated to an unobservable in the error term and that it affects other independent variables, then we have an endogeneity problem. In this analysis, I am assuming that selection bias and endogeneity problem is present in the conditionality of the programme, whereas selection bias arises from the conditional distribution of cash transfers and eligibility.

a truncated one due to sample selection. We can test for the presence of such a bias if there is correlation between a first stage selection equation and a second stage outcome equation. If so, we can also argue that the selection process is determined by a latent variable.

Are we ready to accept the inclusion of a latent variable which we cannot observe or measure accurately? An accurate specification of our problem (or of our econometric model) has to theoretically consider the concepts that may have an influence on the dependent variable and then find observed explanatory variables or proxies that best approach those concepts. In the case of a latent unobservable variable, its conceptualization and specification may be nebulous and we may like to keep our model simple. It is certainly better to control for latent variables than to do nothing because we cannot find suitable observed variables or proxies to represent a latent variable, and test the existence of that variable with some other techniques. Moreover, a probit model, in addition to helping us to determine a 0 – 1 outcome in terms of probability, can also be thought of as a latent variable model and as a measurement error model. As a measurement error, the discrete outcome 0-1 and its error term is thought to represent an imperfect measure of a continuous latent variable (Weeks, 2001).

Methods that postulate a latent variable can be of use in analysing the scope that households and officials have to shape the take-up and / or allocation of welfare benefits. I have analysed observable characteristics that determine welfare take up, but in this chapter I argue that a latent variable of cost of participation also determines the distribution of the probability of receiving the cash transfer. This type of analysis has been carried out with information on means-tested benefits in the United Kingdom and the United States, but its use is less extensive in developing countries, probably due to data limitations.

With regards to the Conditional Cash Transfer Oportunidades in Mexico, this type of analysis has not been carried out extensively despite the fact that it speaks directly to

its design. The Oportunidades means-test was built using discriminant analysis, which is a data reduction technique that results in a linear single score that is then used to allocate the benefits. Although discriminant analysis is not a latent variable method, it shares with the latent variable method the idea that selected demographic characteristics provide information on an unobserved household income. Income directly reported by households is not a convenient source of information to allocate benefits as it is prone to measurement error. But more stable demographic characteristics chosen to best fit the discriminant analysis arguably offer a more reliable source of information of income levels below a threshold or poverty line.

Arguing the presence of a latent variable implies that non trivial assumptions have to be in place before undertaking such modelling. One of the most important assumptions of this model is that the correlation between latent variable and independent variables is not given by the empirical interaction of the data but by a functional form made explicit by the researcher (Krishnakumar and Nagar, 2008). This indicates that there is a joint distribution underlying the latent variables, which is grounded on a theoretical formulation of how the world behaves, rather than on the empirical drive of the data.

Another important assumption is that the relation between endogenous latent variables can be estimated because there is an exogenous variable that serves as an Instrumental Variable (IV) for identifying the first equation of latent variables, and from then on we can identify the rest of the model (Wooldridge, 2009). As in the IV methodology, the error term of the equation plays a key role in the identification process: one of the conditions for tracking independence in the instrumental variable is that it does not exhibit co-variation with error term of the estimation. In fact, the IV method is the leading method for estimating simultaneous equations models (Wooldridge, 2009), but structural equations also face identification conditions —discussed further in the technical appendix— and require a weaving of assumptions that can only support careful interpretations.

A third important assumption that households' decision making on whether or not to claim benefits is independent from their decision to participate in the labour market. That is, households do not trade off work effort and income for welfare income. This is a common assumption in the literature, especially for developing countries (Coady, Martinelli and Parker, 2013; Attanasio, *et al*, 2008; Ravallion, 2009; and section 2.2.6, 3.2 and 3.3 of this research). However, there are structural and instrumental variable models that challenge this assumption and enquire if there is a joint decision to participate in welfare benefits and labour market (Moffitt, 1983; Ravallion, 2009) For the case of Oportunidades, Skoufias and Di Maro (2006) and Vaz (2009) present confronting evidence of rural households responses to Oportunidades. Attanasio, *et al* (2008) argue that behavioural responses for Oportunidades tend to be low or non-existent.

Targeting inefficiency cannot be explained only on the basis of erroneous measurement. Ravallion (2009) shows even though the coverage of the target population improved in the *Di Bao* means-tested income transfer in China, a significant share of benefits is still allocated to ineligible population, as there is a trade-off between Type-1 and Type-2 error. Moreover, Duclos (1995), based on Besley and Coate (1992), argues that there are two issues at stake: firstly the targeting errors of inclusion and exclusion, and secondly, errors that go beyond the inaccurate application of targeting rules. This latter type of error occurs when administrators are aware of the incentive effects that cash transfers may have for income generating ability: individuals may choose to work less in order to qualify for the benefit. The important link with the implementation of the programme is that administrators may affect individual choices through the way in which conditionality and the supply of services are offered. In this case we are concerned with the costs of welfare participation.

As described in section 2.7, the latent variable of costs is measured by the utility forgone of alternative activities not pursued due to the claim of the benefit. I follow

Hernandez *et al* (2006) as they focus on participant preferences which are thought to reveal such utility forgone in the context of multiple economic activities that could be pursued. They conceptualised costs as the net value of benefits relative to income. This definition of costs of participation is labelled as the compensating variation approach (p. 10), because claim of benefits will be observed only if benefits compensate the costs of claim relative to household income. In this sense, lack of participation arises when costs are larger than benefits; whereas we observe a cash receipt when opportunity costs of participation is smaller than benefits.

Put it differently, if cost are large enough not be compensated by the benefits, the net benefit is negative, and the claim of the benefit will not be observed. If so, lack of participation provides information about the costs of participation. This definition of cost of participation is the one adopted on this research. It can factor in multiple sources of costs, and they are considered in relation to household income and entitlement. The sources of costs could be transaction costs (attaining information, eligibility self-assessment, time, transportation costs, among other), psychosocial costs (stigma and personal shame of participation) and the costs of compliance with conditions (attending to school and health centres, employment opportunities forgone). Unfortunately, I do not have information to disentangle between different sources of costs. Moreover, this definition also considers a monetary value of costs relative to the net welfare benefit for each particular household. This monetary assessment also proved to be informative for further assessing the impact of Oportunidades on income poverty, which is carried out on chapter 6. Last but not least, this definition of costs is also best tuned for considering individual determinants in addition to the administrative barriers and budgetary constraints to participation.

Other approaches could well attempt to directly quantify the costs incurred by participants. Manchester and Mumford (2012) argue that such information would entail measurement error and misreporting, and this effort would not preclude the

need for imputing non-participants' costs. This is why Hernandez *et al*, (2007) is the prefer methodology for eliciting cost of welfare participation.

Then, why are we still analysing the residual of the model? The analysis of the error of the model is interesting because it speaks directly to the errors of inclusion and exclusion that are relevant for the assessment of policy performance. The second part of this chapter summarizes the latent variable methodology that was discussed in section 5 of chapter 3 and which is used in this chapter. The third section presents a discussion of the non-parametric nature of this analysis. The fourth section presents the cost of participation by targeting categories and vulnerable groups. The fifth section concludes by presenting the implications of this analysis.

The analysis of error in the model doesn't have to be solely of parametric nature and careful discussion of its sources contributes to its understanding. This is the aim of the following section 2 of this chapter.

5.2 The nature of measurement error in the model

As Pudney (2001) rightly warns, the analysis of error can start by a simple and careful revision of consistency in the dataset and a disclosure on how the information was treated. In fact, I acknowledge that I do not have an administrative dataset that informs me about the eligibility of recipients, nor about the eligibility households may think they have. These are sources of error in my measurement that may lead to discrepancies and that were conceptually laid out in section 3.5.1. In this section I will describe the main discrepancies in the data, although I consider they are unlikely to bias the analysis significantly.

Dornan (2006) uses two datasets to analyse the take-up of minimum income in the UK and compare qualitatively the standard errors of his estimates. Azevedo, *et al* (2009) also uses two different techniques and two different datasets to model the take-up of Oportunidades among youngsters and compares the error in both of them. In

this section I would like to first acknowledge one of the main sources of error in my estimation which is the imputation of values in the eligibility model. Secondly, I would like to make explicit the relationship between the indicators included in the sample selection correction model (table 4.8) and the latent variable of costs of participation.

The total sample of households in the survey is 60,161 that are representative of 26,706,397 households in Mexico in 2008. After the eligibility analysis, the sample increases to 64,200 households which represents a total of 28,463,012 households where the extra 4039 households are secondary households living in the same housing arrangement as the primary household and having different income sources, including cash receipt from Oportunidades. The model considers an unweighted sample of 48137 households, of which 44180 are censored from the first stage mainly because they are not eligible, leaving 12731 uncensored unweighted households.

Indicators that determine the eligibility model are captured in the ENIGH 2008 survey at four different levels. The first one comprises urban and rural areas and regions of the means-test, which are indicators gathered at municipality and locality levels. The second level attains the whole dwelling and the indicators are rooms in the dwelling, type of floor and of bathroom of the dwelling. The third level relates to specific households, which is defined as people who share the kitchen. At household level the survey captures possession of car, gas or electric stove, refrigerator and washing machine, access to social security, demographic dependency and number of members of the household. Last but not least, a fourth level concerns achievements of certain specific members of the household (gender, age and schooling of head of household, number of children in household). In general, there is a very low proportion of missing data for indicators of individual achievements, and there is almost no missing information for indicators at municipality, locality, dwelling and household level.

However, the means-test requires a mixture of those four levels of analysis to create its indicators. And the dwelling indicators are particularly difficult as they have been captured in the survey only for the principal household but they are missing for subsequent households living in the same dwelling. Had I kept them as missing information, I basically lose all information on secondary households which receive Oportunidades cash transfer. They represent 1.26% My preferred option was to assume that they have the same information on the indicators at dwelling level as the principal household. However, I adopted the decision taken by Oportunidades officials[68] which is imputing the mean value in the survey for those three indicators in each of the secondary households of the survey. The mean values evaluated for ENIGH 2008 and the indicators inputted only for secondary households are below. They were also reported in table 2.2 above[69].

- Number of rooms in dwelling: 1

- Demographic dependency ratio: 0.83

- Presence of dirt floor: 0.07

- Presence of toilet without running water: 0.21

This is somewhat problematic as, for instance, presence of dirt floor or toilet without running water are normally a 0 – 1 indicator, whereas now a fraction has been imputed for all secondary households. This imputation would be problematic if missing data is affecting particularly households that prone to be eligible or receive the cash transfer. However, I justify following this procedure for the sake of being consistent with what Oportunidades officials have done before[70]. These imputations

[68] Sintaxis of the assessment of eligibility for ENIGH 2004 available upon request.
[69] The replacement in the dependency ratio is done when no person in the household is older than 15 years-old or younger than 65 years-old.
[70] The analyst does not have access to that information. Only the household may have access to that information, and the administrator might also be keen in checking up household's characteristic in situ for its own records. I received the syntaxis for selecting eligible households that was developed by Oportunidades officials to perform analysis of ENIGH survey data rather than administrative

were done for 3.6% of the eligible and 1.6% of non-eligible households who are also secondary households. However, given the fact that we are imputing a value lower than what could possibly be attained (that is 1) in presence of dirt floor and toilet without running water, we could consider that our measure it is at least underestimating eligibility, rather than overestimating it.

In chapter 4 and in section 3 of chapter 3 we discussed that the independence of the first equation and the second equation could be approached by the correlation of the inverse mills ratio of the first equation and the error of the equation. The error term has played a crucial part in understanding the validity of our assumption of sample selection. But Krishnakumar and Ballon (2008) argue that it is important to make explicit the relationships between the latent variable and the independent variables of the model. This is due to the fact that it is our model which leads this relationship, instead of the empirical data (Krishnakumar and Nagar, 2008).

Figure 5.1 makes the relationship between indicators of this latent variable model explicit. While the boxes indicate the measured indicators, the ovals indicate the concepts to which these boxes are related in the functional form. In grey we see the indicators that were not statistically significant in the model. These concepts were drawn from the theoretical approach presented in chapter 4, but the arrows that unite concepts with indicators have been drawn from the results of exploratory factor analysis, which indicated the relationship between indicators in latent variables or concepts.

The first part of the sample selection correction model of chapter 4 is described in the left-hand side of the diagram. We see that all variables converge in the eligibility model which in turn is measured by the eligibility score. The eligibility score is related to the cash transfer and also to the income measure. But other eligibility indicators have also relationship with other concepts in the box. The second part of

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information (which is the one used officially to determine eligibility). In this syntaxis, missing values are considered as potential non deprivation of households, as explained before.

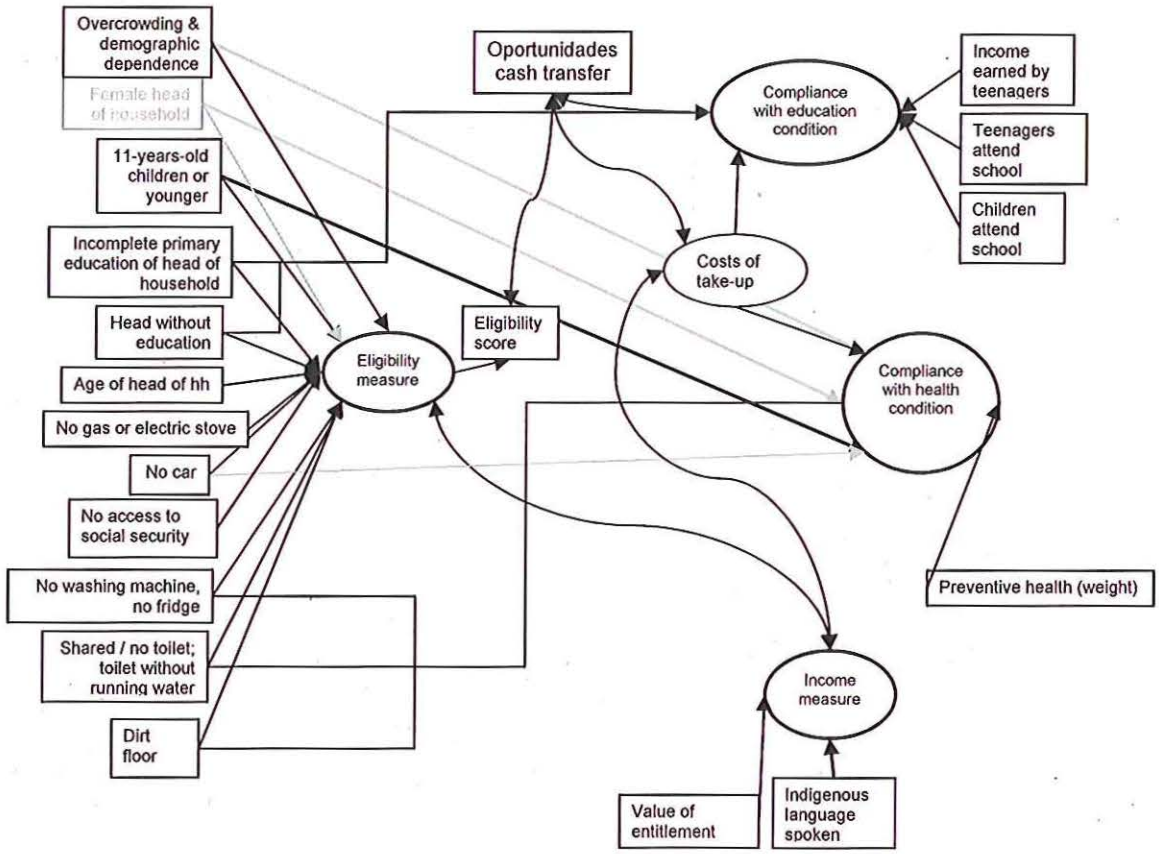
the two-step model would be the structural part of participation in welfare. It is located on the right-hand side of the diagram. On that side we find the latent concepts of compliance with education conditionality, with health conditionality, access to solidarity networks, and household income. The concept of costs of participation is related to the compliance with conditionality and represents information that plays a role in the decision making of recipients and administrators.

The rationale behind the latent variable is the following. It is argued that costs of participation arise from conditionality and affect particularly eligible households that do not receive the cash transfer. On the one hand, households with a higher number of children and teenagers find it costly to fully comply with conditionality of school attendance, and the opportunity cost of teenagers' education is even higher in urban areas. Paradoxically, households with higher numbers of youngsters also attain a higher eligibility score and entitlement to Oportunidades. Still, costs may overcome the benefits and some households do not claim the cash transfer despite of being eligible.

Administration of public services may increase the costs recipients incur. Conditionality is one of the main barriers, but there are others such as barriers to use health centres due to its monetary cost and due to stigmatizing treatment[71].

[71] It is also assumed that households' participation is independent from their decision to participate in the labour market. This is a common assumption in the literature, especially for developing countries (Coady, Martinelli and Parker, 2013 and Ravallion, 2009). However, there are structural models that challenge this assumption and enquire if there is a joint decision to participate in welfare benefits and labour market (Moffitt, 1983)

FIGURE 5.1 PATH DIAGRAM OF THE SPECIFIED MODEL



Source: Author's calculation based on ENIGH (2008)

5.3 The expected value and the cost of participation

5.3.1 The expected value of participation

In order to understand non-take-up, we have to hypothesise that there is an expected value of the transfer of which people are aware. Depending on their eligibility, on school and health attendance, on barriers to welfare experience and on individual taste for welfare, individual households would have a different expected value of such a transfer. In this occasion, even though the probability of cash receipt is near to 0 for non-takers, there is a expected benefit that was not high enough to compensate for the cost of claiming. As a result, we do not observe the claim.

The expected value of a welfare measure is widely used to approximate the distribution of welfare with incomplete information. When doing a critical review of Elbers, *et al* (2002) methodology for poverty mapping, Tarozzi and Deaton (2009)

provided me with the link between the expected value as a wider methodology for assessing welfare. They confirm that most welfare measures (poverty and inequality indexes among others) rely on the mean estimate of welfare and, more importantly, on the distribution of such welfare (Tarozzi and Deaton, 2009, pp. 774). Two assumptions have to be in place, the measurement of predictors and conditional independence.

The second assumption of conditional independence is of particular importance for this research. According to Tarozzi and Deaton (2009), this assumption is the basis of, among others, programme evaluations. The assumption simply establishes that the conditional distribution of income in a particular area or household characteristics is the same in a larger region or population that shares such characteristics. Phrased as it is in programme evaluation, it establishes that treatment is independent of potential outcomes (Tarozzi and Deaton, 2007), as it is only conditional on the characteristics under which the conditional distribution is evaluated.

Randomized Control Trials rely heavily on this assumption to carry out impact evaluation. Given the fact that treatment is allocated randomly, the researcher may assume that the fact of being treated is independent of variables observed and of other unobserved characteristics. Moreover, it is also assumed that the outcome of the treatment is homogenous among the treated, as the only differences between persons treated or not is the random assignment of the treatment. Having these assumptions in place, the precise contribution of the treatment on the outcome of the treated can be estimated, conditional on the variation of observed characteristics.

The expected value of welfare can then inform us about the costs of claiming benefits. This expected value will be conditional on being eligible, complying with programme conditions and other households characteristics that may inform us of barriers to and distaste for welfare benefits. Moreover, given the fact that such characteristics are known to the household as well as to the researcher, they are more informative of the household decision making than, for instance, monthly entitlement or eligibility

scores, which are determined by administrators but may be unknown to some households.

The expected value of welfare benefit will also allow us to determine the unobserved costs. The main motivation behind this methodology is to overcome the limitations of information[72]. Due to the fact that our data is censored for non-recipients, the expected value can be an approximation to project what households would have guessed their expected benefit to be had they participated in the programme. Again, a key assumption would be that the expected value is independent of the assignment of the programme, but conditional on household's characteristics that determine eligibility, conditionality compliance, barriers to and taste for welfare benefits. In fact, the expected value is also the basis of Hernandez, *et al* (2007) method for estimating claim costs, as I will discuss below.

The estimation of the expected value of cash transfer occurs as follows. Firstly, household cash transfer is modelled as

$$ln(y) = E[\ln(y)\,|x] + u$$

A linear approximation of the model is better known as:

$$ln(y) = \beta x + u$$

Where x is a set household characteristics that are observed and determine eligibility, conditionality compliance, barriers to and taste for welfare benefits. Since the residual represents the departure of the observed value from the estimated value, it is important to understand the properties of the residual. The residual is thought to contain information about unobserved household and contextual characteristics. Such

[72] In the case of poverty mapping, it allows us to estimate household poverty from income surveys and project such estimation into census data. Steps and information followed in this research has also been used previously for constructing maps of income (Lopez-Calva, et al, 2008), poverty and inequality (Szekely, et al, 2007) and child nutrition (Rascon-Ramirez, 2012) in Mexico.

characteristics may elicit information about conditions associated with participation or take-up of the programme if the cash transfer is positive, which occurs if

$$ln(y) - \beta x > u$$

Therefore, the expected benefit is thought to be positive but sufficiently high to overcome the value of residuals so the total cash transfer remains positive. But we have mentioned that our data is censored as we do not observe the cash transfer when the expected value is not enough to overcome the cost of participation

$$ln(y) - \beta x \leq u$$

Given we have characterized participation as a latent variable, we have the following restrictions

$$y = \begin{matrix} \{y^* > 0 & u < \ln(y) - \beta x \} \\ \{y^* \leq 0 & u \geq \ln(y) - \beta x \} \end{matrix}$$

This simple form of expected value is to be made conditional on being eligible, to make it consistent to the methodology used in chapter 4. The model of conditional probabilities of Heckman and Smith (2003) can also be used to estimate the expected value of the cash transfer conditional on being eligible.

$$E(y\,|x) \int E(x)\, dF(x) = 0$$

$$E[\ln(y)] = (\beta x + u) * \Pr(x)$$

In chapter 4, I used a Heckman model to regress the probability of getting the cash transfer on household characteristics. The dependent variable was a dichotomic one. Assuming that it has an underlying normal distribution, the second step of the Heckman model is fitted as a probit model. In this occasion, in order to estimate the expected value I will regress the natural logarithm of the actual cash transfer on the same independent variables reported in the model of chapter 4, table 4.7.

I will draw on the sample correction model as it will allow me to explicitly understand the expected value as a function of the distribution of eligibility, the selection equation. The underlying probability$dF(x)$, the probability of being eligible, is again assumed to follow a normal distribution function. It is also used to obtain the inverse mills ratio.

$$E[X|x] = \frac{f(x)}{dF(x)}$$

Where $f(x)$ is the probability density function and $dF(x)$ is the standard normal cumulative function. The Inverse Mills Ratio is not only a device for instrumenting unobserved sample selection but it builds upon a larger property of the normal distribution known as the truncated normal distribution. In general, this property allows estimating the probability distribution of a normal variable at a certain specified value. The probability of observing that particular value of X is given by the probability density function over its cumulative distribution function, just as in the formula of the IMR above. In fact, this property is fundamental for the estimation of costs resulting from the second step of the equation. I discuss it further in section 5.3.2 below.

This model of expected cash transfer uses the same explanatory variables used for the model in chapter 4 and described in table 4.7. It is also convenient to draw on similar instrumental variables as the ones described in sections 3.2 and 4.4 for the Heckman-probit model. Hence, table 5.1 presents a brief description of the dependent and instrumental variables used for this model, which differ from table 4.7.

TABLE 5.1 DESCRIPTIVE STATISTICS OF DEPENDENT AND INSTRUMENTAL VARIABLES OF THE MODEL

Variables	Mean (rural)	Mean (urban)
Dependent variable		
Natural logarithm of Oportunidades cash transfer	3.3	0.42
Instrumental variables		
Expected value of cash transfer in neighbourhood	3	-0.3
Residuals of the expected value of cash transfer at neighbourhood level	-0.27	-0.72
Mean inverse mills ratio at household level	1.55	6.05
Mean inverse mills ratio in neighbourhood (only included in the first step)	1.91	7.62

Source: ENIGH (2008)

In table 5.2 I present the marginal effects obtained during the estimation of the expected value. The second column presents the coefficients for the national sample, and the third and fourth column present coefficients for rural and urban areas respectively.

TABLE 5.2 EXPECTED VALUE OF CASH RECEIPT CONDITIONAL ON ELIGIBILITY (HECKMAN SAMPLE SELECTION)

	Dependent variable is natural logarithm of cash transfer					
	Marginal effects					
Independent variable	National		Rural		Urban	
Predicted probability of cash receipt in neighbourhood	0.67	***	0.65	***	0.78	***
Residuals of predicted prob. of cash receipt in neighbourhood	-0.98	***	-0.87	***	-1.17	***
Mean inverse mills ratio in neighbourhood	-0.49	***	-0.29	***	-0.39	***
Inverse mills ratio for households	-0.14	*	-0.21		-0.17	
Eligibility first-step						
Number of people per room	0.09	***	0.09	***	0.08	***
Demographic dependency	0.04		-0.01		0.09	
Female head of household	0.00		0.06		-0.11	
Households with children aged 0-11	-0.14	***	-0.11	**	-0.15	***
Age of head of household	0.02	***	0.02	***	0.02	***
No social security benefits	0.16	*	0.23	*	0.03	
No education of head of household	0.44	***	0.47	***	0.44	***
Incomplete elementary education	0.33	***	0.34	***	0.33	***
Shared or no toilet	0.53	***	0.69	***	0.37	**
Toilet without running water	0.51	***	0.68	***	0.25	
Dwelling with dirt floor	-0.02		-0.14	*	0.25	*
Household without gas or electrical stove	0.04		0.02		0.14	
Household without refrigerator	-0.06		-0.08		-0.05	
Household without washing machine	0.05		0.03		0.15	
Household without car or truck	0.18	*	0.11		0.43	***
Rural effect	0.69	***				
Cash receipt second-step						
Endogenous effects (conditionality)						
Teenagers attend school	0.97	***	0.72	***	1.26	***
Teenagers in neighbourhood attend school	-0.28	**	-0.30	**	-0.24	*
School-age children attend school	0.42	***	0.24	**	0.70	***
School-age children in neighbourhood attend school	-0.74	***	-0.56	**	-0.92	***
Had a preventive health check-up in the last 12 months	1.54	***	1.72	***	1.15	***
Proportion of neighbors who had preventive health check-up	-1.30	***	-1.44	***	-0.62	
Indigenous language spoken in household	0.24	***	0.28	***	0.24	**
Natural logarithm of the value of entitlement	0.20	***	0.24	***	0.14	***
Natural logarithm of income brought by teenagers into the household	0.02	*	0.02		0.04	
rho	-0.06	0.08	-0.09		-0.08	
sigma	2.35		2.39		2.24	
lambda	-0.14		-0.21		-0.17	
Sample size	64200		16063		48137	
Dummies for state level effects	Yes		Yes		Yes	
Mean natural logarithm of expected benefit	1.60		3.50		0.97	

Source: Own calculations based on ENIGH (2008)

*** significance at 0.001 level

** significance at 0.05 level
* significance at 0.01 level

In general, the model of expected value confirms the significance and marginal effects of the take-up model in table 4.8. The marginal effects of the variables that determine eligibility are very stable in terms of statistical significance, and they have stronger effects than in the probit model. A few changes may be appreciated in the lack of significance of not having a car in the column for national and rural results, or the significance of not having a stove in urban areas. In general, the most important determinant of eligibility is the rural effect.

The changes that interest me the most are the ones regarding conditionality. The marginal effects are stronger in the model of expected value, more importantly, and the differences between urban and rural areas are more substantive. Health conditionality and its neighbourhood effect are stronger in rural areas. Moreover, behavioural change regarding school attendance has stronger effect in urban areas. The effect of teenage school attendance is stronger than the one for children. The effects of neighbours' compliance with education conditions is negative. Interestingly, the negative effect of neighbours' child school attendance is very large in urban areas, larger than school attendance itself. This is important since some research has suggested that allocation of benefits towards child school attendance is not efficient given the already high levels of school attendance in the country (De Janvry and Sadoulet, 2006, Attanasio, *et al*, 2008). Yet, the table shows the importance of children attending more than their neighbours as it determines the expected cash transfer, which is arguably evidence that supports the existence of behavioural change among Oportunidades' children.

Beyond confirming results from previous chapters and adding some caveats with regards to health conditionality, this model offers the coefficients rho (ρ), sigma (σ) and beta-lambda (β_λ). As discussed in section 3.2, rho indicates the correlation between residuals of the first and the second equation. Sigma is the standard deviation

of the residuals of the model, that is, the difference unexplained by the predicted value. Beta-lambda is the coefficient by which the inverse mills ratio from the selection equation enters in the second-step equation. And given the fact that the inverse mills ratio is a function of the residuals of the first equation, these three coefficients are related to each other as follows:

$$\rho = \frac{\beta_\lambda}{\sigma}$$

I can think of rho the correlation between residuals but also as the ratio that tells me the effect of the inverse mills ratio on sigma. This also place rho as a measurement of the fit of the selection equation.

The model in chapter 4 assumed a normal distribution of residuals. Yet, the variation in the linear version of the Heckman selection equation used in this chapter may be greater than the one in chapter 4. This makes it more plausible to have a significant rho. Hence, it is not surprising that we are more certain that the rho coefficient at national level in table 5.2 is significantly different than 0.

Nevertheless, the coefficient rho is not statistically significant. Given the comparison on the different exclusion restriction in section 3.2, I have opted for the best available instrumental variable. The extent to which the inverse mills ratio contributes to explaining the unobserved variation (residuals) of the equation is not different from 0 at aggregate level. As some unobserved variation remains unexplained at local level, I think of those as the costs of participation, as discussed in section 5.3.2.

In table 5.3 below I present the mean values that arise from this analysis for each of the four categories of targeting. The first column present the mean natural logarithm of cash transfer, which is similar for all recipients regardless of whether they are eligible or not and it is equivalent to approximately 500 Mexican pesos (22 GBP). The second column presents the mean unconditional prediction of cash transfer. We see that, based on households characteristics included in the model, the highest

prediction is for eligible recipients and then for non-eligible recipients, which is expected as they actually get the cash transfer. But non-recipients are also predicted some cash transfer. In order to get the expected value, the model then considers the probability of being eligible, which is higher for eligible than for non-eligible households as we see in the third column.

The fourth column reports, finally, the mean expected value of the cash transfer, which derives from multiplying figures summarized in the second and third columns for each household. In this table, as the values presented are mean values, the multiplication of the cells only gives an approximation to the mean value in the fourth column. For eligible recipients, I do not observe much change from the unconditional prediction (4.6) to the expected value (4.3) as they have an almost 100% probability of being eligible (93% on average). Similarly, eligible non-recipients also have a high expected value. However, for ineligible households the expected value (0.4) is lower than the unconditional value (3.6) as they have low probability of getting the cash transfer (11%). Similarly, non-eligible non-recipients have the lowest expected values of cash transfers.

TABLE 5.3 MEAN EXPECTED VALUE OF CASH TRANSFER BY HOUSEHOLD ELIGIBILITY AND CASH RECEIPT

Households	Mean ln of cash transfer	Mean linear prediction (βx)	Mean probability of being eligible	Mean expected value
Eligible recipients	6.2	4.6	0.93	4.3
Ineligible recipients	6.2	3.6	0.11	0.4
Eligible non-recipients	0	2	0.88	1.8
Non-eligible non-recipients	0	0.93	0.02	0.03

Source: Own calculations based on ENIGH (2008) and Oportunidades (2010). Figures are statistically different from each other at a 0.05 level.

As we can see from table 5.1, rural areas have a larger amount of cash transfers allocated and also a higher eligibility score, as I will discuss further in chapter 6. Hence, I anticipate that rural households have a larger expected value of the cash transfer. Table 5.4 disaggregates the information in table 5.3 into rural and urban areas, only for three categories which include recipients and eligible non-recipients.

TABLE 5.4 MEAN EXPECTED VALUE OF CASH TRANSFER BY ELIGIBILITY, CASH RECEIPT, AND RURAL AND URBAN AREAS

Households	Mean ln of cash transfer	Mean linear prediction (βx)	Mean probability of being eligible	Mean expected value
Rural areas				
Eligible recipients	6.2	4.8	0.92	4.5
Ineligible recipients	6.2	4	0.08	0.35
Eligible non-recipients	0	2.7	0.85	2.3
Urban areas				
Eligible recipients	6.2	3.5	0.96	3.4
Ineligible recipients	6.2	3.2	0.13	0.44
Eligible non-recipients	0	1.3	0.92	1.2

Source: Own calculations based on ENIGH (2008) and Oportunidades (2010). Figures are statistically different from each other at a 0.05 level.

The mean linear prediction is higher in rural areas than in urban ones for all categories of targeting. Yet, the probability of being eligible is higher in urban areas, given the fact that there are less households eligible in urban than in rural areas. Still, the mean expected value from the programme is higher for eligible households in rural areas. Yet, the expected value for non-eligible recipients is slightly higher in urban areas than in rural ones, mostly due to the higher probability of being eligible they have. And if I compare this expected value with the one of eligible non-recipients in urban areas, the difference is very small. Hence, exclusion errors could be explained as a result of the low expected value that some eligible households have. The following table 5.5 explores the distribution of the expected value by conditionality.

TABLE 5.5 SCHOOL AND HEALTH CHECK-UPS ATTENDANCE AMONG ELIGIBLE NON RECIPIENTS

Eligible non-recipients	Expected value of cash transfer for households whose member had her weight measured in the previous year	Weight not measured	Actual entitlement according to the rules
No children or teenagers	2	0.6	1.7
Households with children			
No child attends school	1.6	0.6	3.5
At least one child attends school	1.9	1.1	5.8
Households with teenagers			
No teenager attends school	1.9	0.8	4.3
At least one teenager attends school	2.6	1.9	7.1
Eligible recipients	4.3	3.2	4.9
Ineligible recipients	0.4	0.3	5.6

Source: Own calculations based on ENIGH (2008) and Oportunidades (2010).

We see that the expected value for most of the excluded households goes from 0.6 to 2.6. This is the natural logarithm of the cash transfer, which would correspond to 2 to 14 Mexican pesos (approximately £0.5). Monthly entitlements are very sensitive to conditionality compliance. And they reach the highest value when excluded households go to the health centre and send children (1.9) and teenagers to school (2.6). The last two rows of the table present the expected value and actual entitlement of recipients of Oportunidades. We can firstly note that the expected value is very similar to the actual entitlement. The model is then a good guess of what recipients expect to get from the programme. However, the model also identifies that excluded households have a much lower expected value of the cash transfer than their actual entitlement. It is important to mention that households may know the schedule of payments if they attend school, but they cannot know whether they are eligible as the MTT has not been disclosed to prevent that households change their behaviour in order to become eligible for the cash transfer. The comparison above let us know that excluded households find little expected benefit from participating in the programme. This expected benefit is lower than the actual entitlement and much lower than the entitlement of excluded households who already send children and teenagers to school.

5.3.2 Costs as compensation for barriers to participation

The expected value informs us that eligible households who are not taking-up the programme would actually expect to receive cash transfer given their characteristics. The expected value is also the basis for estimating the welfare costs in the methodology of Hernandez, Pudney and Hancock (2006) published also as Hernandez, *et al* (2007). They propose that, when modelling take-up of the Income Support among British pensioners, we can understand that the difference between the observed take-up and the expected benefit of claimants can inform us about the problem of take-up. For successful claimants, benefits of welfare are higher than costs, which are unobserved. I have drawn on the method used by Hernandez, *et* al (2007) to understand the cost of participating in Oportunidades. They understand the costs of welfare participation as the expected value of the observed cash receipt, conditional

on the whether or not the household claims the benefit they are eligible for. The implicit claim costs of taking-up the programme are 'the compensation variation required to offsetting stigma and other barriers to participation' (p. 15). The model can be thought of as a utility function but Hernandez, *et al* (2007) and other research has found it more useful to work with the logarithm of the benefit entitlement, ln B, that is regressed on observed household characteristics $Z\alpha$ and a residual V that informs on the unobserved individual characteristics that jointly determine take-up of welfare. The model understands that take-up occurs if

$$\ln(B_i) > Z\alpha + V$$

$$\Pr(take - up \,|B_i, Z\alpha) = \Pr(V < lnB + Z\alpha) = F(\sigma^{-1}[\ln B - Z\alpha])$$

Otherwise, non-take-up is observed. Hernandez, Pudney and Hancock (2006) understand the costs of welfare participation as the expected value of the observed cash receipt, conditional on the whether the household claims or not the benefit they are eligible for.

$$Cost_{claimants} = E[cas\square transfer \,|cas\square receipt] = E[\exp(Z\alpha + V)\,|V < lnB + Z\alpha]$$

$$= \frac{e^{Z\alpha}\int_{-\infty}^{lnB+Z\alpha} e^{v} dF(V)}{\int_{-\infty}^{lnB+Z\alpha} dF(V)}$$

Based on a log-normal distribution (Aitchison and Brown, 1956, p. 87), and by assuming the function to be of probit distribution, the cost for participants in Oportunidades is:

$$= \exp\left(Z\alpha + \frac{\sigma^2}{2}\right) \Phi \frac{\ln B - Z\alpha - \sigma^2}{\sigma} \Big/ \Phi \frac{\ln B - Z\alpha}{\sigma}$$

(Hernandez, Pudney and Hancock, 2006, p. 16)

As we can see, the numerator of the cost function is the expected value of the cash transfer, which I have reported in table 5.3. The denominator is the probability of receiving the cash transfer, which was obtained in chapter 4 after the model reported in table 4.8.

For eligible non-recipients, Hernandez, *et al* (2007) propose that the distribution function is 1 minus the probabilities of getting the cash transfer, as the unobserved costs V are larger than the expected value:

$$C_{non-recipients} = E[cash\ transfer\ |cash\ receipt]$$
$$= E[\exp(Z\alpha + V)\,|V \geq lnB + Z\alpha]$$

$$= \exp\left(Z\alpha + \frac{\sigma^2}{2}\right) 1 - \Phi \frac{\ln B - Z\alpha - \sigma^2}{\sigma} \Big/ 1 - \Phi \frac{\ln B - Z\alpha}{\sigma}$$

Hernandez, *et al* (2007) assumed that costs followed a logit distribution, but my estimation differs in assuming a normal distribution of the residuals from the second step equation. Then, I have built upon the general truncated normal distribution to elicit the information of the costs of participation. This property allows us to estimate the probability of observing Y conditional on X characteristics and B entitlement which are larger than V or the residual of the estimation of Y. Since my model builds upon a normally distributed probability of participation, I can estimate the central moments of the distribution. The denominator follows a normal distribution on which our model is based. I am interested in the expected value of the cash transfer (E(Zα+V)) conditional on observing the cash receipt, which is only possible to observe is the estimation is larger than the residual (V< Ln B + Zα). This is given by:

$$E[cash\ transfer\ |\ cash\ receipt] = E[\ Z\alpha + V\,|\,V < lnB + Z\alpha]$$
$$= Z\alpha + (\sigma(\varphi\left(\frac{\ln B - Z\alpha - \sigma}{\sigma}\right)) / (1 - \Phi\left(\frac{\ln B - Z\alpha}{\sigma}\right)))$$

Similarly, when cash receipt is not observed, I understand there is an expected value of cash transfer (E(Zα+V)) conditional on the residual being larger than this expected value (V > Ln B + Zα). This is given by:

$$C_{non-recipients} = E[cash\ transfer\ |cash\ receipt]$$
$$= E[\exp(Z\alpha + V)\,|V \geq lnB + Z\alpha]$$

$$= \mathrm{Z}\alpha + (\sigma(-\varphi\left(\frac{\ln \mathrm{B} - \mathrm{Z}\alpha - \sigma}{\sigma}\right)) / (\Phi\left(\frac{\ln \mathrm{B} - \mathrm{Z}\alpha}{\sigma}\right)))$$

In summary, the expected value is given by the probability density function of Y over the its cumulative distribution function, just as in the formula of the IMR above. This property is fundamental for the estimation of costs resulting from the second step of the equation. As the latent variable is not directly observed, we only obtain information of its distribution from the mean estimation and its variance. As the mean and variance represent a monetary value, we can elicit the monetary value of the latent variance only with respect to this particular estimation. Hence, the monetary values expressed here can only be compared within this analysis. This means that these costs are experienced by any given participant only with reference to the monetary benefits they would expect in this estimation[73].

This definition of cost of participation could include transaction costs (attaining information, eligibility self-assessment, time, transportation costs, among other), psychosocial costs (stigma and personal shame of participation) and cost of compliance with conditions (attending to school and health centres, employment opportunities forgone). It has been possible to elicit this information because of the property of the mean truncated normal distribution, upon which the Inverse Mills Ratio is also estimated. Unfortunately, the information available cannot be used to quantify the costs incurred by participants directly, and hence the different sources of costs cannot be disentangled. As we have discussed the origins of this methodology and how it is related to the analysis

[73] I am grateful to Andrew Zeitlin who raised this issue upfront.

in previous chapters, I present some results and analysis of the cost of participating in Oportunidades.

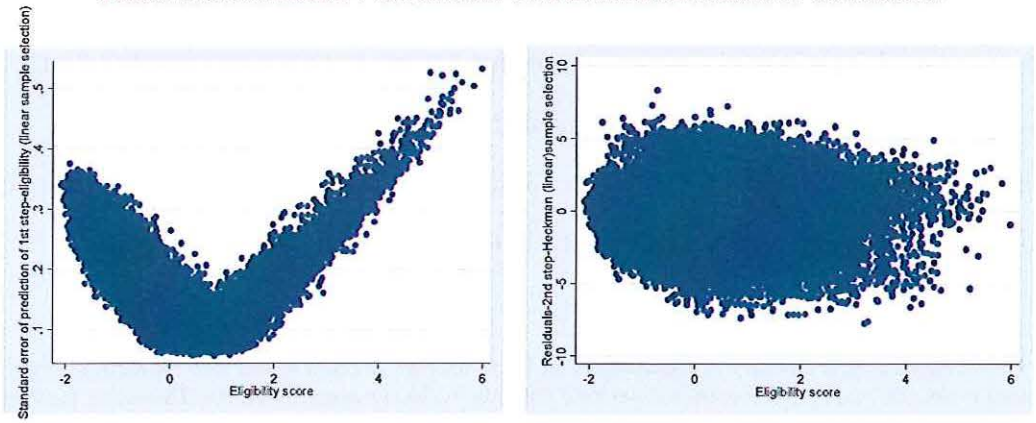
5.4 Cost of participation by targeting categories and vulnerable groups

My objective is to understand:

1. The costs faced by the four different categories of targeting.
2. The costs of barriers to participate in urban Oportunidades people face.
3. The ways in which the targeting of the programme can be improved.

I will firstly explore the distribution of the error term. Figure 5.2 plots the standardized errors of the model against the eligibility score. The first equation (the means testing model) is on the left hand side. We see that the higher the eligibility score, the higher the standardized error. This clearly indicates that eligibility score is not enough to explain the probability of getting the cash transfer and the need for a model that further explores what the variables included in the eligibility score cannot explain. On the right side of figure 5.2 I plot the standard error of the second equation against the eligibility score for eligible households in urban areas. We see that the errors are more evenly distributed along the spectrum of eligibility, which indicate that the eligibility score is no longer needed to explain the result of the equation after including other factors (including the variables that predict eligibility) on their own.

FIGURE 5.2 ELIGIBILITY SCORE AND STANDARDIZED ERROR OF THE MODEL



Source: Author's calculation based on ENIGH (2008)

The array of hypothesis that we can test following Hernandez, *et al* (2007) methodology is limited. This cost could include the compliance with conditionality of the programme, the forgone leisure time[74] and stigmatizing treatment. I am aware other research disentangles different sources of costs (Manchester and Mumford, 2012). This type of nuanced analysis is not possible to carry out with the data available. Yet, I would like to describe how this concept of cost is shaped by our model in order to understand its limitations and implications.

If we think of the residuals as the outcome that cannot be explained by the model, we will be certain that the cases that deviate from the model are the ones who will have the greatest residuals. Since the dependent variable is cash receipt, households who get the cash transfers have the largest predicted probability and the lowest residuals. Similarly, households who do not get cash transfer have the lowest predicted probability and the largest residuals. Yet, we also have to remember that our sample selection regression considers firstly only the observed sample of eligible households to estimate cash receipt conditional on being eligible. Then, it also predicts the probability of getting the cash transfer for non-eligible households who get it based on the model obtained for eligible households. The underlying idea is that the unobserved sample selection is being controlled by the inverse mills ratio. The model then determines four categories of analysis: eligible cash recipients, non-eligible cash recipients, eligible non-recipients and non-eligible non-recipient households.

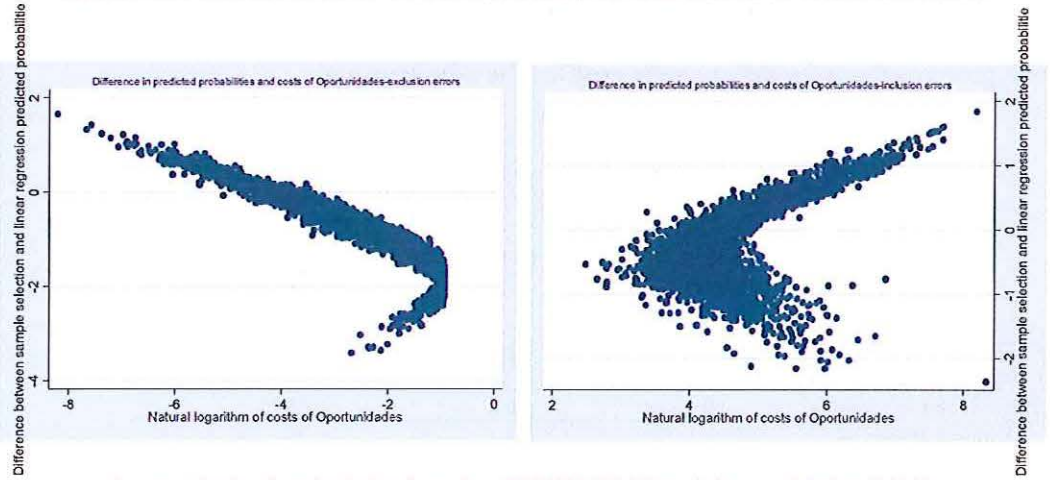
The shape of errors is determined by the methodological decisions and the functional form assumed in the model discussed in section 3.3 and exemplified in table 5.2. I would like to explore now the cost are related to the probabilities predicted with different methods. In section 3 of chapter 3 I discussed that a simple linear regression could also be used to predict participation in Oportunidades. The probabilities

[74] I assumed that the possibility of allocating more hours to work in order to increase the income from wages is not affected by participation in welfare but only by labour market features. Therefore, the time occupied in participating in the programme could have only been used as leisure time, and not as work time. This is a strong assumption that much of the research makes. Moffitt (1983) offers a discussion on how this assumption can be tested.

obtained by different methods are very similar for eligible recipient households. But the difference increases for households constitute into errors of targeting. In particular, it has been revealing to use the difference between the linear regression and the linear probability of the sample selection method to understand targeting efficiency. It shows that the larger the difference, the higher the risk of incurring in a targeting error.

Figure 5.3 presents the difference between the linear predicted probabilities using the sample selection correction and using the linear model, on the vertical axis. The difference between predicted probabilities can be positive or negative, within a range of 2 and -2, but it makes a huge difference for the costs of participation. On the horizontal axis it plots the natural logarithm of the costs of participation. The left-hand-side panel includes only the cases that fall into error of exclusion, while the right-hand-side panel presents the errors of inclusion.

FIGURE 5.3 DIFFERENCE BETWEEN LINEAR PREDICTED PROBABILITY OF SAMPLE SELECTION MODEL AND OF THE LINEAR MODEL AND COSTS OF PARTICIPATION



Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

We see that costs are negative for households in error of exclusion, and positive for recipients. For excluded households on the left side, we also see that the difference between predictions of models is mostly negative, meaning that the probability of the linear regression is higher than the linear predicted probability of the sample selection model. In such situation, costs are closer to 0 and, as the probability of the sample selection increases, so the costs expand towards the negative axis. This means that if

the probability of receiving the cash transfer increases, the lower the costs for that household to get them. I must bear in mind that there are a few excluded households that have a positive cost instead of a negative one. Those households have a positive expected value conditional on the residuals being larger than the prediction. Put it differently, the model is not good to predict non-take-up for those households.

In the meantime, costs for ineligible recipients are plotted in the right side. If the linear probability is lower than the sample selection probability, households tend to have relatively lower costs of participation. However, if the linear probability is larger than the sample selection, those households tend to have larger costs of participation.

Table 5.6 presents the mean value of the predicted probabilities, residuals and costs of welfare participation for four different categories of analysis or groups of households. The second and third columns state the average probability and residuals for each group. The fourth column states the cost and although the costs result from probabilities and residuals, this fourth column does not result from the previous two as it presents the weighted average cost for the whole group.

TABLE 5.6 MEAN PREDICTED PROBABILITIES, EXPECTED VALUES AND COSTS OF PARTICIPATION BY HOUSEHOLD ELIGIBILITY AND CASH RECEIPT

Households	Mean predicted probability of cash receipt	Mean expected value	Mean cost of welfare following Hernandez *et al* (2007)
Eligible cash-recipients	0.74	4.3	5.1
Ineligible cash recipients	.58	0.4	4.5
Eligible non-recipients	0.32	1.7	-2.3
Non-eligible non-recipients	0.14	0.2	-1.4

Source: Own calculations based on ENIGH (2008) and Oportunidades (2010). Figures are statistically different from each other at a 0.05 level.

We see that the highest predicted probability, expected benefit and costs are for eligible households who get the cash transfer. Non-eligible non-recipients have the lowest predicted values. Two categories that are of particular interest are ineligible recipients and eligible non-recipients. Ineligible recipients have a predicted probability lower than the eligible recipients but higher than the rest of the households.

Eligible non-recipients have a significantly lower predicted probability than recipients, they have large costs but not as high as non-eligible households.

Please note that mean expected values and mean costs of welfare in table 5.6 are expressed in terms of natural logarithm. This is, mean costs are always positive for recipients and negative for non-recipients, and higher for eligible than for non-eligible households.

For eligible households that get the cash transfer, large expected values residuals are divided by near-to-one probabilities, resulting in costs that are slightly lower than the expected value. The group that has the largest positive cost of welfare are ineligible households. This is due to the relatively high expected value of cash receipt and slightly lower probability of getting the cash transfer. Eligible households that do not get cash transfers have negative expected values and low probability of cash receipt, giving large negative costs as a result. The following table disaggregates the costs by urban and rural areas.

TABLE 5.7 MEAN PREDICTED PROBABILITIES, EXPECTED VALUES AND COSTS OF PARTICIPATION BY HOUSEHOLD ELIGIBILITY AND CASH RECEIPT

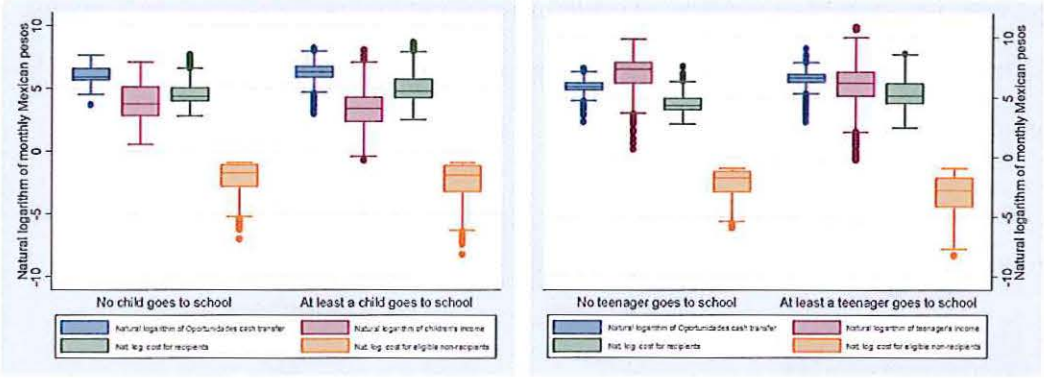
Households	Mean cost of welfare in rural areas	Mean cost of welfare in urban areas
Eligible cash-recipients	5.2*	4.5
Ineligible cash recipients	4.7*	4.4
Eligible non-recipients	-2.8*	-1.7*
Non-eligible non-recipients	-2.2*	-1.4*

Source: Own calculations based on ENIGH (2008) and Oportunidades (2010).
* Figures are statistically different from each other at a 0.05 level.

We see that costs are larger in rural areas. For recipients, this means a larger positive cost, but non-recipients face larger negative cost. In urban areas, non-recipients have large negative costs of participation. This may be due to the fact that more cash transfers are allocated in rural than in urban settings. This is also interesting as exclusion error in urban areas are higher, where excluded households face relatively lower costs of non-participation if compared to their rural counterparts.

In theory, conditionality may be included in the programme as a device through which policy makers increase the cost of participation in order to induce self-selection of claimants. It is argued that if the costs of conditionality are high, only the poorest households would have incentives to comply with them as they need the cash transfer more urgently. Yet, there are at least two caveats to this mechanism. Cost may be too high for households to claim the benefit. Moreover, some households would invest in human capital and comply with conditionality regardless the existence of cash transfers and may claim the benefits without having to change their behaviour. Figure 5.4 summarizes the distribution of such costs for children and teenagers that attend to school and compares it with the distribution of cash transfers and the income earned by children and teenagers.

FIGURE 5.4 DISTRIBUTION OF COST OF PARTICIPATION, CASH TRANSFERS AND INCOME BROUGHT BY SCHOOL-AGE PEOPLE IN THE HOUSEHOLD, BY EDUCATION CONDITIONALITY



Source: Author's calculation based on ENIGH (2008)

When looking at the condition of sending children in school age to primary school, we see that the median cost for recipients is very similar regardless whether households send none or at least one school-age children to school. Moreover, recipients who do not send any teenager to school bear just slightly lower cost than households who do. For recipients, median costs are relatively smaller than the median cash transfer[75]. However, median costs are larger than median income brought

[75] There are caveats in this affirmation. Firstly, households who comply with conditionality for all children can reach the lowest as well as the highest costs, having more variation in that group. And the greatest mean cost are for households who achieve conditionality partially, this is they send only some of their children or teenagers to school while some others do not go. In the graph, they are represented in the category in which at least one school-age person goes to school. Moreover, some of the costs that

home by children, whereas median costs are smaller than median income brought by teenager. For eligible non-recipients, the median cost is similar whether they send children to school or not, but median costs are slightly further from if they send teenagers to school. Table 5.8 presents the mean value of the costs for different household categories and for conditionality. Cash recipients (both eligible and non-eligible) have positive costs and non-cash recipients have negative ones. I have omitted the category of non-eligible non-recipients as I consider it of less interest for this research. Since the average cash transfer has the value of 6 in natural logarithm ($e^6 \approx$ 403 MXN), costs tend to be always higher than the cash transfer. Moreover, for eligible non-recipients, costs are negative and large if the comply with conditions, whereas are smaller and closer to 0 if they do not comply with conditions.

TABLE 5.8 AVERAGE COSTS THAT RESULT FROM THE PARTICIPATION MODEL, FOR HOUSEHOLDS THAT COMPLY WITH CONDITIONALITY BY HOUSEHOLD ELIGIBILITY AND CASH RECEIPT

	Mean value of the natural logarithm of cost for households who…					
	Send children to school	Do not send children to school	Send teenagers to school	Do not send teenagers to school	Had members' weight measured	Do not have their weight measured
Eligible cash-recipients	5.2	4.8	5.7	4.6	5	4.5
Ineligible cash recipients	4.6	4.6	4.8	4.2	4.6	4.6
Eligible non-recipients	-2.3	-2.2	-3	-2	-2.4	-1.7

Source: Own calculations based on ENIGH (2008) and Oportunidades (2010).

Eligible households have lower costs if they do not comply with any of the conditions. For eligible non-recipients, this means that costs get closer to 0. Interestingly, compliance with teenage education begets the highest costs among eligible households, and also among non-eligible recipients. However, non-eligible recipients have very similar costs for child school and health conditions. This may speak of the relative importance that has secondary school over primary schooling for non-eligible households, who have already high levels of investment in education.

are represented in the 'No conditionality' category correspond to households that have no children or no teenager, who also have high costs as they receive Oportunidades less frequently. Still, costs in general are lower for those who do not comply with education conditionality than for those who do comply.

If I now concentrate in eligible non-recipients, I can see that the gap between compliance and non-compliance of secondary school attendance is much wider, followed by the gap in health conditionality. I interpret this as the difficulties that households may have in changing their behaviour. In fact, moving from child school attendance to the absence of it seems less difficult than the behavioural change in secondary and health check-ups attendance. This difficulty in changing the behaviour may be an explanation for lack of take-up of the programme.

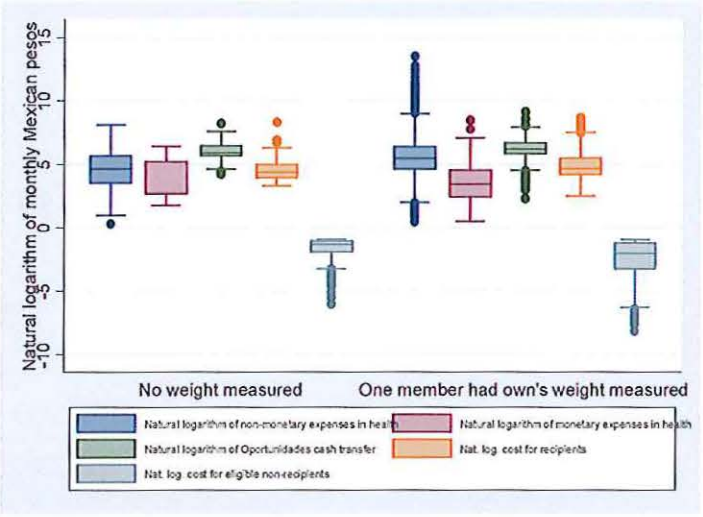
Similarly, a wide gap can be observed among recipients that attend health check-ups and those who do not, indicating the higher costs of this type of condition. The costs for ineligible recipients are lower than the mean value of the cash transfer and even lower if they comply with conditionality. As discussed in section 4.3, ineligible households tend to have higher investments in human capital than eligible households, to begin with. Costs for them are then lower than the benefits of claiming the cash transfer. This could lead us to think that ineligible recipients would still invest in human capital had the transfer not been in place. Alternatively, ineligible recipients could be the ones who have benefited from the programme for a longer time. Disentangling between these hypotheses would require several observations of the same households over time which is not available for this research.

An explanation to the high costs of secondary education can be found in the income earned by teenagers. In figure 5.4 we saw that the median income earned by teenagers was slightly higher than the median cash transfer, especially if teenagers do not attend school. There may be a cost involve in the income forgone by attending school, although the costs are lower for eligible recipients than for non-eligible recipients. Similarly, the relatively lower cost of primary school attendance may be due to the fact that the amount of income earn by children is even lower than the cash transfer they would receive.

In section 4.3 I presented some evidence of behavioural change related to health conditionality, affirming that Oportunidades seems to increase investment in

preventive health check-ups of cash recipients. However, there is significant underinvestment among eligible non-recipients. The analysis of the cost of participation confirms this conclusion. From table 5.8 we saw that costs for eligible non-recipients related to health conditionality are even higher than the ones related to primary school attendance. I will now turn to analyse the costs of that health conditionality. Figure 5.5 summarizes the distribution of household monetary and non-monetary expenses in health, as well as the cash transfer and estimated costs of participation.

FIGURE 5.5 DISTRIBUTION OF HOUSEHOLD HEALTH EXPENSES, CASH TRANSFERS AND COST OF PARTICIPATION, BY HEALTH CONDITIONALITY



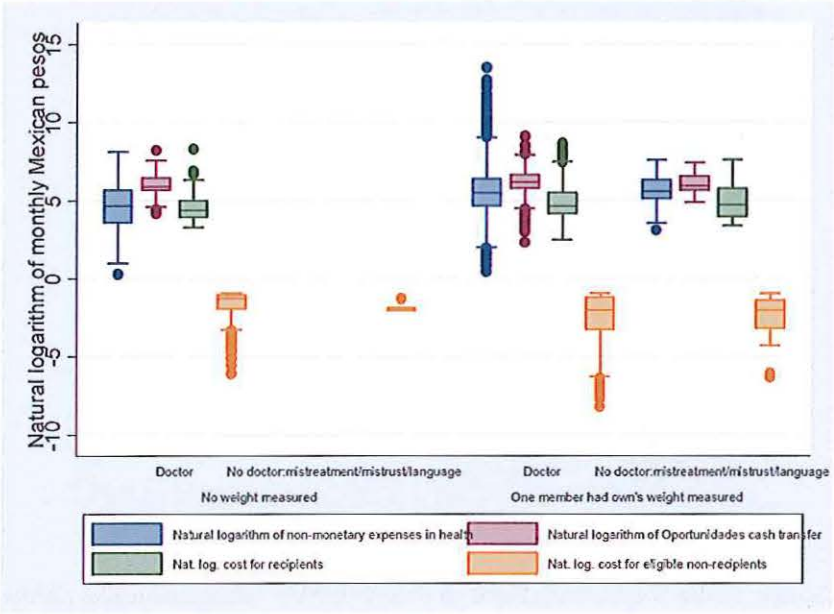
Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

In both groups, non-monetary expenses are larger than monetary ones and the median cash transfer is slightly larger than any of those expenses. The boxplots on the left correspond to households that do not comply with health conditionality. Their median non-monetary and monetary expenditure is lower in comparison to that of the households with access to preventive care. They receive slightly smaller cash transfers and their costs may reach the full amount of such transfer. Eligible non-recipients have negative costs which tend to be larger for those who comply with conditionality.

Eligible non-recipient with larger costs tend comply with all conditions. For most eligible non-recipients, costs get closer to 0 if they do not have health check-ups.

Health conditionality may be achieved at a high cost. The survey also includes information on the households that did not visit a doctor last time they suffered illness. Figure 5.6 plots the distribution of expenses, cash transfers and costs for households that did not visit the doctor because they do not trust him/her, they were mistreated or the doctor did not speak their language have cost as high as the ones who visit the doctor.

FIGURE 5.6 DISTRIBUTION OF HOUSEHOLD HEALTH EXPENSES, CASH TRANSFERS AND COST OF PARTICIPATION BY BARRIERS TO ACCESS HEALTH SERVICES



Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

Most households who did not have their weight measured did not find barriers to health care and were able to visit the doctor. But a few of them decided not to visit the doctor because of the stigmatizing treatment they would receive. Any of them received cash transfer, which is worrying as they did not invest in health expenses either. This puts them in a very vulnerable position. All of them have a negative cost as they are eligible but did not receive a cash transfer.

Nevertheless, most of the households who experienced stigmatizing treatment actually did comply with conditionality, and they are plotted in the right-hand side of the graph. Those who comply with conditionality and overcome stigmatizing barriers to health services are more likely to get the cash transfer. The ones who received the cash transfer have costs that are smaller than the cash transfer itself. Eligible non-recipients have larger negative costs, further away from 0. Stigmatizing barriers are associated with fewer expenses in health and the conditionality of the programme should also consider the existence of these barriers and the extent to which they discourage health investment.

Table 5.9 below presents the mean cash transfer received by households and their eligibility. The second column summarizes the mean cash transfer in pesos, which is similar for all recipients. The third column presents the mean costs, which are relatively larger in rural areas and for eligible recipients. Then, the fourth column presents net benefit (cash transfer after costs). The net benefit of recipients is negative, larger in urban areas and among ineligible households. The figures presented in table 5.9 are weighted averages of the figures for each individual household. For each individual the last two columns (mean transfer after cost and mean benefits after cost) should add up to the total mean cash transfer (second column). However, in this table, columns do not add up exactly to the total as they are weighted averages in the population.

TABLE 5.9 MEAN BENEFITS AND COSTS FOR RECIPIENTS

	Mean ln of cash transfer	Mean ln of cost	Mean difference between ln(cash transfer) and ln(costs)
Receives Oportunidades	6.2	4.8	1.3
Urban eligible recipients	6.2	4.5	1.6
Urban non-eligible recipients	6.2	4.4	1.8
Rural eligible recipients	6.2	5.2	1
Rural non-eligible recipients	6.2	4.7	1.4

Source: Own calculations based on ENIGH 2008

Table 5.10 presents the mean cost for households who despite of being eligible did not receive the cash transfer. We can see in the second column that their expected value is larger for rural households than for urban ones. Moreover, the third column shows that the mean costs are negative and closer to 0 for urban households than for rural ones, which imply larger costs of non-participation in rural areas. Consequently, costs offset the expected benefit and participation is not observed. The third column reports this mild net benefit for urban and rural areas. Yet, the entitlements would be larger than the expected benefit, so the net benefit that could have been obtained from participation would be significant. This net benefit could be larger than the net benefit of urban households who receive Oportunidades. Yet, given the low expected benefit, the marginal benefit of the programme is larger for households who participate in the programme.

TABLE 5.10 MEAN BENEFITS AND COSTS FOR URBAN ELIGIBLE NON-RECIPIENTS

Exclusion error	Mean ln of expected cash trasnfer	Mean ln of costs	Mean net benefit between ln(expected) and ln(costs)
Urban eligible non-recipients	1.2	-1.7	0.53
Rural eligible non-recipients	2.3	-2.8	0.51

Source: Own calculations based on ENIGH 2008

The following table 5.11 considers the costs of participation as a penalty that reduces the cash transfer each household receives, and summarizes the net benefits after those cost have been deducted from the cash transfer they receive or the legal entitlement for eligible non-recipients. The table shows that all recipients have some net benefit after considering the costs of participation. The smaller net benefits and larger costs are for eligible recipients who comply with the conditions established. This shows the opportunity cost of participation. In contrast, ineligible recipients incur in smaller costs if they comply with conditions, showing smaller opportunity cost of participation. Teenage education has the largest opportunity costs and begets the smallest net benefits.

Secondly, eligible non-takers have generally a positive entitlement and a negative cost of participation. If the entitlement considers such negative costs, the net benefit increases as presented in the last row of table 5.11. If they were to receive the cash transfer they are entitled to, the net benefit would be larger if they comply with conditions. On average, their benefit will be smaller than the mean value of the cash transfer (6.2) if they do not comply with the conditions. The larger net benefit would come from secondary education as secondary school attendance among excluded households is low. The opportunity cost of teenage education is then large as well for excluded household. There would also be considerable benefits from investing in children's education and health check-ups.

TABLE 5.11 MEAN LOGARITHM OF BENEFITS AND ENTITLEMENT AFTER CONSIDERING COSTS OF PARTICIPATION BY HOUSEHOLD ELIGIBILITY, CASH RECEIPT AND CONDITIONALITY

	Send children to school	Do not send children to school	Send teenagers to school	Do not send teenagers to school	Had members' weight measured	Do not have their weight measured
	Mean value of cash benefit after considering costs for households who…					
Eligible cash-recipients	1.1	1.2	0.9	1.3	1.1	1.5
Ineligible cash recipients	1.7	1.6	1.7	1.5	1.6	1.4
	Mean entitlement after considering costs for households who…					
Eligible non-recipients	8.2	4.8	10.1	6	7	5.3

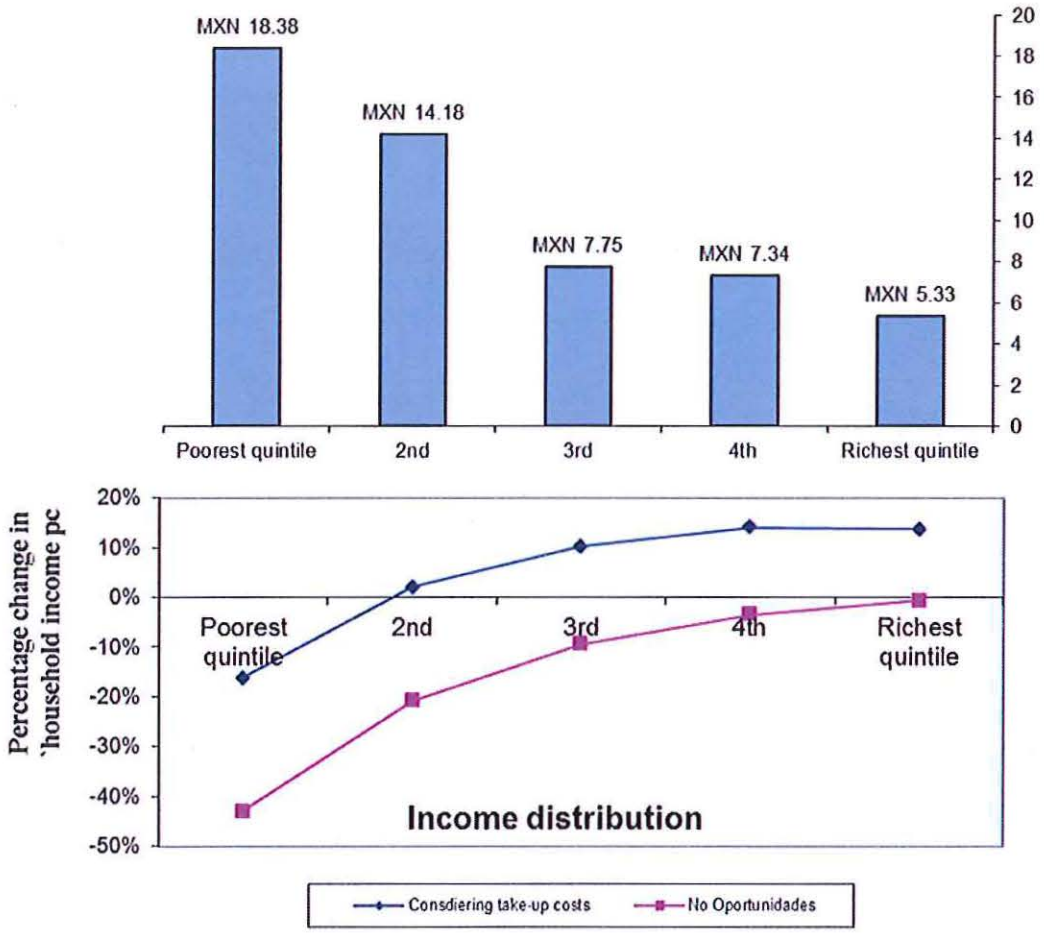
Source: Own calculations based on ENIGH (2008) and Oportunidades (2010).

For graphical purposes I have decided to convert the natural logarithm of costs into their cash value. I could obtain the actual monetary value by transforming the e^x where x is the value of the natural logarithm. Costs can get as high as 8.7 or 6003 MXN ($e^{8.7}$) and as low as -8.2 or 0.01 cents of MXN. Figure 5.7 portrays the distribution of costs with reference to quintiles of the income distribution[76]. In the upper panel we see that costs are larger for the first quintiles of the income distribution. Moreover, the blue line shown in the bottom panel represents the percentage change of mean income for each quintile if costs were to be considered as a reduction of welfare. The line shows that the reduction of income only affects the poorest quintile of the income distribution. The costs of participation do not change the mean income of the second quintile. From upper quintiles, the costs of

[76] The highest cost represented in the graph is 18.38 MXN, which is approximately £0.9. The highest entitlement household may have is 2200 MXN, which is roughly £108 per month.

participation are almost none. Most of recipients in such income quintiles would be ineligible, which explains why they see an actual increase in their income.
The costs do not offset the benefits of the programme in terms of redistribution of income. The pink line in the same graph shows that the reduction of income would be even steeper if the cash transfer was withdrawn. And this would affect mostly the poorest quintiles of the income distribution. Despite its cost, participation is progressive in terms of income distribution.

FIGURE 5.7 DISTRIBUTION OF COST OF CLAIMING URBAN OPORTUNIDADES



Source: Own calculations based on ENIGH (2008) and Oportunidades (2010).

We see that by considering costs we see a reduction in household welfare and this reduction of income is stronger than if Oportunidades cash transfers were to disappear. This raises the question of the impact that the programme has on alleviating poverty,

and whether that impact is affected by the costs of participation. The impact of the programme in the light of targeting efficiency is discussed in next chapter 6. I now conclude this chapter in the following section.

5.5 Conclusions

Sample selection of participants is not the only problem affecting the analysis of participation, as we also face limited information on the factors that determine take-up. In this chapter I have shown how expected value of cash transfers represents an 'informed' guess of the opportunity costs of participation in Oportunidades. Recipients face the highest expected values, whereas eligible excluded households face the lowest expected values. This on its own right is informative for the lack of take-up.

But I have also elicited the latent variable of costs as measured by the utility forgone when participating in the programme. I have followed Hernandez, *et al* (2007) compensating variation approach. In this approach, the claim of benefits is observed only if costs are compensated by benefits, which are estimated on the basis of household's entitlement and income. In this sense, lack of participation arises when costs are larger than benefits. This is precisely what is observed in table 5.10 above. Excluded households in rural areas find little expected benefit from participating in the programme and their costs of participation almost offset such benefits. Hence, they opt out the programme. Excluded household in urban areas would have the lowest costs of participation and the largest benefits. Still, they do not participate in the programme because the benefit they expect is also the smallest.

In the meantime, we observe cash receipt when opportunity costs of participation is smaller than benefits. On table 5.9 I provide evidence of large costs of participation among eligible recipients, especially in urban areas. But while costs of participation are high for recipients, expected benefits are high as well, leaving a positive net benefit that explains take-up. Moreover, costs are low for non-recipients.

This definition of cost of participation could include transaction costs (attaining information, eligibility self-assessment, time, transportation costs, among other), psychosocial costs (stigma and personal shame of participation) and cost of compliance with conditions (attending to school and health centres, employment opportunities forgone). It has been possible to elicit this information because of the property of the mean truncated normal distribution, upon which the Inverse Mills Ratio is also constructed. Unfortunately, the information available cannot be used to quantify the costs incurred by participants directly, and hence the different sources of costs cannot be disentangled. This monetary assessment also proved to be informative for further assessing the impact of Oportunidades on income poverty, which is carried out on chapter 6. Last but not least, this definition of costs is also in best tuned to the aim of considering individual determinants in addition to the administrative barriers and budgetary constraints to participation.

Most recipients face costs of participation that are smaller than the benefits they receive, especially for ineligible recipients. Costs begat from compliance with teenage school attendance are the largest, followed by health conditionality for recipients. Meanwhile, non-recipients face negative costs of participation. Eligible non-recipients have negative costs, which are larger in rural areas than in urban areas, highlighting that opportunity cost of participation is larger in rural areas. Teenage school attendance is the most costly condition, whereas child school attendance is more costly for excluded households than health conditionality. This speaks of the importance of behavioural change in terms of children schooling.

I have also focused on households who encountered barriers to have access to the health centre. Some households incur in costs when they refrained from visiting the doctor due to mistreatment, mistrust or because the doctor did not speak their language. Stigmatizing barriers to health care begat higher costs among recipients and eligible non-recipients. But the interaction between stigmatizing treatment and the lack of preventive health check-ups was particularly worrying. The group that faced

such barriers do not invest in health nor do they receive the cash transfer. For such households, there were neither health expenses nor cash transfers, and the median costs were particularly high. This group is of particular concern.

Hence my objective was to understand the costs faced by three of the four different categories of targeting in order to identify groups who are experiencing various difficulties in investing in health and education. From the analysis of the costs of participation I conclude that conditionality and barriers to health assistance represent the highest costs for participants in Oportunidades cash transfer. Costs are high, and higher than benefits for recipients, and close-to-0 for eligible non-recipients who do not comply with schooling conditionality. Costs for ineligible recipients are higher than for eligible recipients, especially if they do not comply with secondary school and health attendance. Yet, the highest costs were caused by the lack of compliance with health conditionality.

Previous research has argued that the main costs of Oportunidades is the opportunity costs that children and teenager face when attending school and giving up income earning activities. I have argued that children and teenagers combine both activities. Moreover, the value of children's income is lower than the cash transfer. The value of teenager's income may be higher than the cash transfer, but this does not explain why some non-eligible recipients attend frequently to school while eligible non-recipients seem to face lower costs if doing so.

This analysis is particularly interesting in the context of limited information available to the researcher. The researcher has to ground her model on assumptions that are not trivial and have to be justified in context, which limits its generalizability over time and space, but makes prospective analysis better grounded. This type of research has not often been applied to the case of Oportunidades, but it could be particularly useful in the light of the reform of the programme. Due to the high attendance rates at primary school, it has been recommended that primary school benefits are curtailed so resources are channelled to secondary education (Attanasio, *et al*, 2008). However, in

chapter 4 we have discussed that compliance with education conditionality has not resulted in higher investments in education than their peers, especially among eligible households who are also the ones with lower levels of investment in human capital. In table 5.7 I also showed that the costs of complying with education conditionality are high, especially in primary education. If we can assume that recipients are already complying with conditionality, is the increase of resources going to attract those eligible households who are not complying? We have also discussed that non-eligible recipients show the highest levels of investment in human capital, especially in secondary education, and the highest benefits of claiming regardless the costs. If a higher supply of benefits at secondary education is about to be deployed, is it going to attract households who would have not invested in teenage education in the absence of the programme? I would suggest that the answer to both questions is negative.

The aim of this thesis was to explore the role demand and supply side factors play in in take-up of Oportunidades. The exploration of the latent variables of costs also speaks about the structural factors that are more difficult to measure but that determine participation in Oportunidades. This chapter argues that factors such as conditionality and administrative barriers to healthcare can be accounted as structural factors that represent high costs for eligible non-recipients. They are particularly less likely to participate in the programme and invest in human capital. Then structural factors, in the specific context of Oportunidades, prevent behavioural change among eligible non-recipients of welfare. Yet, costs are also faced by eligible and ineligible recipients, who face similar administrative barriers and still take-up the benefit and comply with conditionality. In tune with Van Oorschot's model (1995), given the structure of benefits, individuals still shape their participation in the policy.

6 ASSESSMENT OF TARGETING EFFICIENCY BY GEOGRAPHICAL AREAS AND CONTRIBUTION TO EXPLAIN POVERTY IMPACT

The aim of this chapter is to explore the distribution of targeting outcomes and costs among Mexican regions. Poverty varies to a great extent according to the geographical context. Availability and relative prices of the food basket vary for different regions. And such prices are important in determining household consumption and therefore poverty. And if we include variables other than income to explain income poverty we may even get a more heterogeneous poverty profile. As Tarozzi and Deaton (2007) explain, the conditional relation between schooling, occupation or assets and income poverty is determined to a great extent by the local rates of return in each of the regions analysed. Preferences and tastes also vary across regions and determine the conditional relation of poverty. Attainment and usage of assets such as refrigerator and car are highly contextual. Moreover, regional demographic trends may create different poverty profiles. The spatial heterogeneity of poverty has therefore to be acknowledged when designing, implementing and evaluating an antipoverty programme.

Oportunidades means-test considered the economic geography of Mexico from its inception. The means-test model assigned lower scores to more affluent regions so households living in such regions were less likely to attain eligibility. Then, if eligibility score for Oportunidades varies geographically, a similar variation would be expected from targeting outcomes. Nevertheless, variation in eligibility score may not be the only source of regional heterogeneity in targeting outcomes. Welfare allocation may also vary due to the implementation of the programme in regional contexts.

The most immediate evaluation of efficiency of Oportunidades would consider the allocation of budget across the Mexican territory. I observe that poorest regions were allocated larger budgets, which is expected; yet this allocation may not have been the most efficient as the 2008 Oportunidades budget allocation was also associated with larger inclusion errors at state level. Still, budget allocation only accounts for part of the supply-side factors behind participation.

In chapter 2 I summarised the literature that focuses on individual characteristics to explain participation in welfare programmes. Other type of research has focused on revising the extent to which participation is determined by administrative design. I have mentioned that an additional approach, taken in this research, is to understand both individual and administrative factors into consideration, yet acknowledging how they work in a specific context. In this section I attempt to advance the understanding of errors of targeting (inclusion and exclusion errors) by underpinning both administrative and individual factors behind them.

According to Oportunidades aims, it is expected to have impact on two objectives: income poverty and human capital investment. The impact of Oportunidades on human capital investment of participants has been researched extensively by international organisations (The World Bank, 2004; Attanasio, *et al*, 2008) which have taken advantage of the randomized deployment of the programme for the purposes of impact evaluation. This initial randomized deployment of the programme made it possible to assess the treatment effect of the programme on health and education and understand the causality behind this effect. This treatment effect estimation assumes full compliance of participants in the programme, meaning that those who participate are eligible households who comply with the conditions of the programme. Moreover, it assumes that nobody in the control group was allocated treatment. It also assumes that no one who had not complied with the conditions got the treatment, including those who would have complied with the conditions regardless the cash transfer, known as the 'always-takers' (Zeitlin, 2010b).

I have offered evidence of substantial inclusion and exclusion errors, which questions the assumption of perfect compliance. This is why I argue that the effect of targeting efficiency on the programme may inform us on the extent to which the assumption of full compliance may not hold, biasing treatment effects. If inclusion and exclusion errors are negligible, then targeting efficiency may not be important in understanding the impact of the programme in terms of income or behavioural change. Yet, if targeting

inefficiency is related to changes in income or human capital investment, we may have further grounds to question the assumption of full compliance.

One could assume that budget allocation determines the implementation and outcomes of the programme. Yet, budget allocations do not seem to be associated to the outcomes of the programme in neither income nor human capital investment. I further discuss below that both income and human capital investment do not necessarily follow budget allocation but they are also associated to targeting errors. If targeting inefficiency is associated with impact on income poverty and human capital investment, then we may cast doubt on the assumption of full compliance. Moreover, I argue that demand-side factors that determine targeting efficiency and the regional impact of Oportunidades on income poverty in 2008.

This chapter is divided into 5 sections. The first section briefly reports on the regional distribution of eligible households, cash recipients and households that comply with conditionality of the programme. The second section justifies the use of the Targeting Differential as a summary measure of targeting efficiency and presents targeting differentials across regions. The third section presents the relation between the regional targeting differential, the means-test model and the budget allocation of Oportunidades, concluding that the cash transfer supply may account for inclusion errors at local level but cannot fully explain the regional variation of targeting efficiency. The fourth section presents the demand-side of human capital at regional level. It analyses levels of health and school attendance and behavioural change induced at local level, and its relation to targeting efficiency. It concludes that while inclusion errors may be attributed to income poverty levels and budget allocation, exclusion errors are related to local behavioural responses in relation to human capital investment health and education conditionality. In the fifth section I also analyse the extent to which targeting efficiency affects the impact of Oportunidades on poverty at local level. Targeting differential may impact on income poverty through different pathways. Impact on income poverty headcount may

be achieved via cash transfer allocation and via stimulation of human-capital-related behavioural change, although both pathways have important caveats.

6.1 The regional differences of the means-test and the targeting differential

As mentioned, geographical targeting is an important part of the targeting model of Oportunidades and little attention has been paid to it in the research on targeting efficiency. At its inception, Oportunidades was targeted towards rural areas. Eligibility to the programme was determined by a complex means-test model with 40 different regressions to fully account for economic conditions among rural and urban areas, and among regions across the country. Income poverty was assessed in 40 different ways because it was argued income poverty is different depending on the relative prices and affluence of the context in which a household lives.

However, as the programme expanded into urban areas, the means-test was simplified into a unique score system (SUP for its name in Spanish Sistema Unico de Puntaje) which was applicable to the whole of the country and was effective until 2008. This unique score system was also featured the regional differences in the means-test. In a nutshell, households were assigned an eligibility score in which negative values reflected higher household income and lower income poverty. The threshold of 0.69 and higher scores indicated that the household was income poor and guaranteed eligibility to Oportunidades.

Yet, the eligibility score assigned 0.653 points if the household was located in a rural area (2500 or less inhabitants). Given the eligibility threshold of 0.69, we can understand that many households in rural areas are eligible according to the SUP largely on account of their rural location. Moreover, the eligibility score is differentiated to account for regional variation in household income. More affluent regions were penalized in the discriminant analysis with a negative coefficient, given the fact that household income is higher in more affluent regions and therefore the statistical relationship between the poverty income line and socioeconomic indicators is weaker.

The poorest region, central Chiapas, gets no penalization, while the rest of the regions are penalized with a negative score as presented in the second column of Table 6.1. This discriminant analysis was carried out with information of the 1995 pilot deployment of what was then Progresa in rural areas. Through this chapter I will refer to these regions as Oportunidades' regions, as they differ from the main political divisions named states, as discussed below. Table 6.1 also includes the mean monthly household income per capita in pesos and pounds, obtained from the ENIGH 2008, to illustrate the relationship between penalisation score and Oportunidades regional score.

TABLE 6.1 SCORES FOR DIFFERENT GEOGRAPHICAL REGIONS IN THE MEANS-TEST MODEL

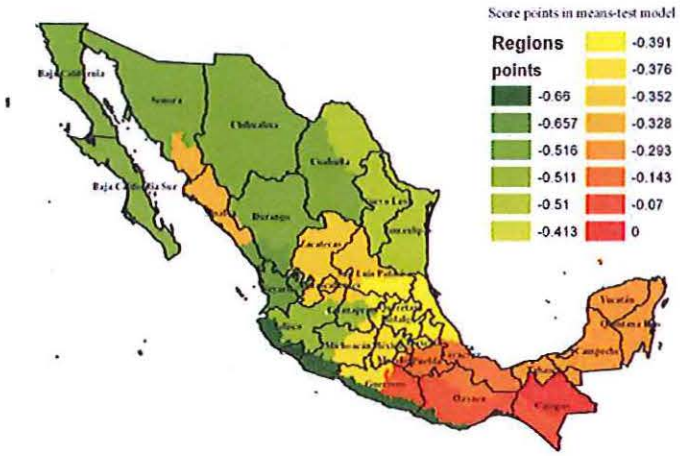
	Penalisation score	Mean household income per capita (MXN pesos Aug 2008)	Approximate value in £
Central Chiapas	0	1824.5	89.6
Mixteca Huasteca region	-0.07	2071.4	101.7
Gulf region	-0.143	2432.4	119.4
Mayan and Tabasco region	-0.293	3432	168.5
Sinaloan region	-0.328	3964	194.6
Semi-arid region	-0.352	3144.7	154.4
Balsas river region	-0.376	2832.1	139
Gorda mountains and Huasteca	-0.391	2153.1	105.7
Mexico city	-0.413	4731.1	232.2
Tamaulipas region	-0.51	4227.6	207.5
Bajio region	-0.511	3571.5	175.3
Northern region	-0.516	4351.0	213.6
Nayaritan region	-0.657	3065.4	150.5
South Pacific Coast region	-0.66	2776.0	136.3

Sources: own estimations based on ENIGH 2008 and Oportunidades (2010). Conversion rate 1GBP=20.4MXN

We can appreciate that less wealthy regions in the South of Mexico such as Central Chiapas, Mixteca Huasteca and Gulf of Mexico regions have a low penalisation score. Meanwhile, affluent regions such as South Pacific Coast, Nayaritan and Northern regions are penalised with a higher score that would make households less likely to be eligible. Figure 6.1 below illustrates the differences in score among regions. Regions with higher penalisation score are in green tones, whereas oranges and reds colours highlight regions with lower penalisation score. The broad picture tells us that households in Northern regions would show lower eligibility score due to higher penalisation. On the contrary, households located in Southern regions get a higher eligibility score due to the low (as low as zero) penalisation points. This trend actually

follows the pattern of regional inequality in which Northern regions of the country are more affluent than Southern and South-Eastern regions (Esquivel, 1999). And within each region, rural areas are assigned higher scores than urban ones.

FIGURE 6.1 MAP OF THE REGIONAL SCORE IN OPORTUNIDADES' MEANS-TEST



Source: Author's design based on Oportunidades 2010.

It is now important to mention some caveats of the analysis at regional level. The survey used for this analysis (ENIGH 2008) can only be disaggregated at state level. States are politically sovereign regions that make decisions over health and education budget allocation.

States are demarcated with a thick black line in figure 6.1. We appreciate that some states have only one penalisation score, while some others include up to four different penalisation scores. Given the structure of the ENIGH, I cannot make robust comparisons between regions as they are defined in Oportunidades. The level of analysis I can make inference of is the state level. This is a limitation to some extent as Oportunidades regions have much to say statistically speaking, and they capture much of the variation in terms of eligibility and participation in the programme. But due to the structure of the survey ENIGH, only state level comparisons are valid. As I shall discuss in the next section, I could use the methodology of poverty mapping to salve this obstacle.

Yet, states are the units of analysis of most interest for policy analysis purposes. States have increasingly gained control in the expenditure of health and education since the decentralisation of these services started in 1983 (Vargas, 2010). Oportunidades relies on this budget to provide schools and health centres where beneficiaries may comply with the conditionality. The model should capture such budget constraints if we are to analyse the barriers to participation at local level. Moreover, the expenditure of Oportunidades' cash transfers is limited by budget constraints. There has to be some degree of coordination with state level offices to distribute cash transfers that may vary with each region.

Regional scores then discriminate among households that would otherwise have equal eligibility score. An urban household with an eligibility score just below the threshold (0.684) is formed by 2 children that attend school and 3 working-age adults living in a three room house. It has a 48-years-old male head of household who did not received formal education, and the household also lacks social security. They have a private toilet without piped water and the household does not own a car. Their monthly income per capita is 1007 pesos, which puts them under the capability income poverty line. They have the demographic characteristics to be eligible but given the fact that they live in Tapachula, Chiapas, part of the Mixteca Huastecan region in table 6.1, their score gets penalized with -0.07 points which drives them just below the eligibility threshold. They do not receive Oportunidades cash transfers. Had they lived in a less penalised region, such as Central Chiapas or in a rural area, they would be eligible for Oportunidades.

Three hundred Kilometres away, in La Independencia, in Central Chiapas, a similar urban household receives the Oportunidades cash transfer. They are formed by a baby and a school-age boy that attends school and 4 working –age adults living in a two bedroom house. It has a 53-years-old male head of household who finished primary school. They have a private toilet without running water, they lack of washing machine and social security. Their income is topped up monthly with 575 pesos of

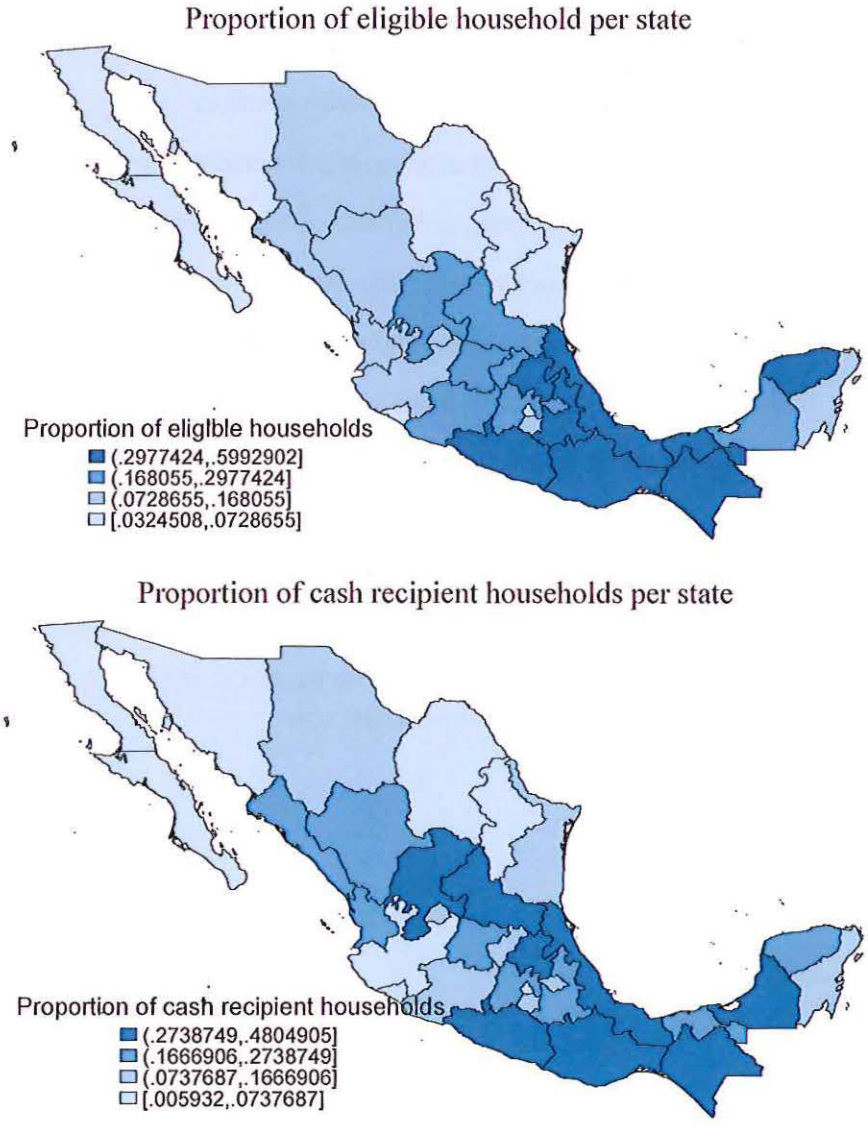
Oportunidades and it is this transfer which pushes the household just above of the alimentary income poverty line, but still leaves them below the capability poverty line. Their eligibility score 0.724 but it since they live in central Chiapas, their score is not penalised by geographical targeting. Had they lived in Northern regions of Mexico they wouldn't be eligible.

The mean eligibility score for households in central Chiapas is higher (1.6) than in the mean eligibility score for households in the Northern region (-0.63), so it is expected that there are more households that are eligible in the regions that are thought to be the poorest. As mentioned, this is considered to be a more objective assessment of poverty since social and economic conditions vary to a great extent across regions of Mexico. Attanasio, Meghir and Szekely (2003) acknowledge that variation of observable and unobservable characteristic across regions of Mexico is a cause of concern when evaluating Progresa. Variation at household observed and unobserved characteristics can be dealt with statistical methodologies, some of which are presented below. Yet, institutional conditions such as labour markets, infrastructure, public services, geographical features, the actual implementation and political economy that surround the policy also vary from region to region and are more difficult to account for. Judging each of these dynamics would deserve an exclusive research on its own. In this research I am only interested in the variation of observed households' characteristics, such as eligibility and conditionality of the programme, and unobservable variation captured by the costs of the programme.

This variation affects the distribution of eligible households across the country quite remarkably. Figure 6.2 show two maps. The one in the upper panel maps the proportion of eligible households in each state; the one in the lower panel maps the proportion of households that get the cash transfers by each state. The darkest blue indicates a higher proportion of population while the lightest blue indicates a lower proportion. In a broad sense, they follow the traditional pattern of income distribution and wealth already mentioned, where Northern regions show higher levels of income, and therefore lower

levels of eligibility and cash transfers of Oportunidades, an anti-poverty programme. A more careful examination has to consider that for instance Campeche, in the Yucatan peninsula, has a high proportion of cash recipients for a relatively lower proportion of eligible households. At the same time, the neighbour state of Tabasco, near the Yucatan peninsula, has a lower proportion of cash recipients than it has of eligible households. These differences precisely speak about the importance of examining targeting differentials.

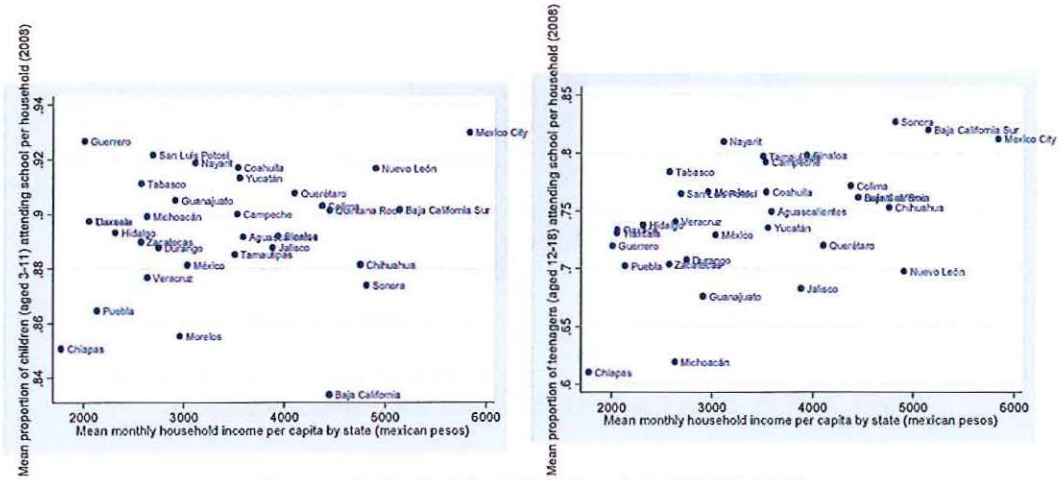
FIGURE 6.2 MAPS OF PROPORTION OF ELIGIBLE AND CASH RECIPIENT HOUSEHOLDS BY STATE



Source: Author's design based on ENIGH 2008 and Oportunidades (2010).

In previous chapters we have discussed that allocation of cash transfer seldom follows the eligibility model and one of the most important elements determining participation in Oportunidades is the compliance with conditionality. Therefore, it would be useful to reflect on the distribution of school and health check-ups attendance across the country. Research conducted on educational outcomes highlight the fact that school attendance for 3 to 14 years-old children shows a Pearson correlation coefficient of 0.5462 with GDP per capita at state level (Secretaria de Educacion, 2010, p. 86). Furthermore, Attanasio, *et al* (2008) highlight that the opportunity cost of school attendance increases for the teenagers due to the income forgone if the teenager does not enter the labour market. I would then expect that school attendance varies according to the distribution of income across the country. Figure 6.3 in broad terms confirms this hypothesis; on the left hand side panel we see the school attendance ratio for children aged 3 to 12 years old, and the right hand side panel plots school attendance ratio for teenagers aged 12 to 18 years old. While school attendance among households with children is quite high (from 83 to 94% per cent), school attendance among households with teenagers can be much lower (as low as 60%). The positive trend between teenage school attendance and household income is confirmed. However, poor states like Guerrero or San Luis Potosi can achieve levels of children school attendance as high as some other wealthy states such as Nuevo Leon or Mexico City.

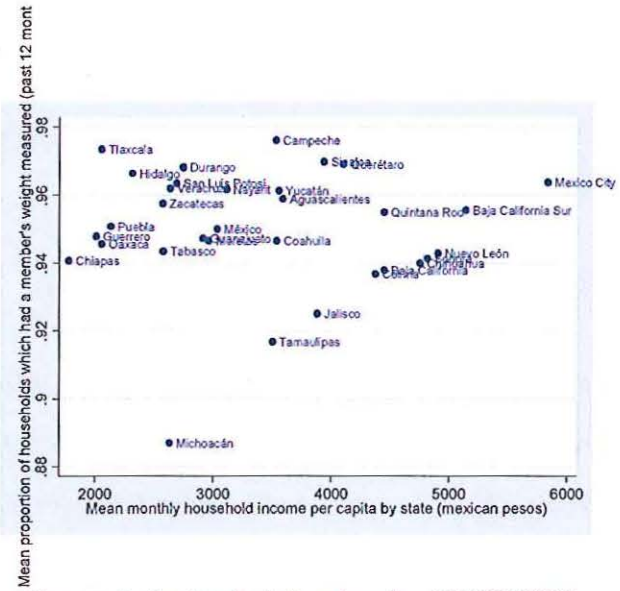
FIGURE 6.3 HOUSEHOLD INCOME PER CAPITA AND PROPORTION OF SCHOOL ATTENDANCE PER HOUSEHOLD BY AGE



Source: Author's calculations based on ENIGH 2008.

Still, the relationship between preventive health and income is not as straight forward. Vargas (2010) highlights that even after controlling for per capita government health expenditure, there are important differences in access to preventive health care. Vargas (2010) argues that is important to consider the provider of health care at state level, whether it is centralised or decentralised provision, in order to explain access to preventive health in rural areas. Figure 6.4 shows the proportion of households in which at least one member had her weight measured in the past 12 months against mean household income per capita at state level. The positive relationship between preventive health care and household income seems to be less straight forward. In short, households in wealthy states were as likely to have access to preventive health care as households in less wealthy states[75].

FIGURE 6.4 HOUSEHOLD INCOME PER CAPITA AND PROPORTION OF HOUSEHOLDS WHO'S MEMBER HAD HER WEIGHT MEASURED



Source: Author's calculations based on ENIGH 2008.

We have discussed that income distribution varies across Mexico and that this variation is related to determinants of targeting efficiency, such as eligibility and the actual proportion of cash recipients. Teenage school attendance is also positively related to

[75] The state of Michoacán, with the lowest level of preventive health, has a statistically significantly different mean than the rest of them but Tamaulipas, which is the second to last in the rank of preventive health in figure 6.4.

income levels, which speaks about the difficulties of eligible households in poor areas to comply with conditionality. Yet, this relationship is less straight forward in the case of children school attendance and preventive health care. Therefore, I cannot expect to observe higher participation and better targeting efficiency due only to the fact that a state is less affluent than another.

A fair assessment of regional differences would consider a specific model of welfare participation for each region or state to account for peculiar relationships between different factors behind participation. Designing area-specific models that explain welfare participation at local level is a potential area for further research (Huesca, 2010). In the case of this particular research, after facing hardware, time and technical[76] limitations, I have considered exploring the extent to which information on the demand of welfare can contribute to explaining the targeting efficiency observed at local level. But firstly I have to justify how targeting efficiency is going to be assessed. In the next section I summarize the targeting differential as the best summary measure of targeting efficiency, which was also discussed in brief in section 6 of Chapter 3. The subsequent sections discuss how the information on eligibility, conditionality and welfare costs can explain, to some extent, Oportunidades targeting efficiency.

6.2 The targeting differential and regional targeting efficiency

In order to evaluate targeting I have decided to take the targeting differential as a yardstick. The existence of other methodologies, such as the Concentration Index, to assess targeting is acknowledged in section 6 of Chapter 3. However, the targeting differential contributes to shed light on the main problem of this research, which is low participation of Oportunidades in urban areas. The targeting differential is built upon the errors of targeting and gives equal weight to both errors in its measurement. In a nutshell, it represents the correct targeting of the programme to which it subtracts the inaccurate targeting. The accurate targeting is the take-up, or participation rate or

[76] The exogenous variation of different regional scores is essential for estimating the model. If I limit the analysis to one region or state, I lose the variation and sample selection models find it difficult to converge.

coverage rate, which is the proportion of eligible households that get the cash transfer. The inaccurate targeting is the proportion of non-eligible households benefited by the programme, also known as the leakage rate.

I can also think about the targeting differential in terms of the errors of targeting. The inaccurate targeting is simply the error of inclusion. And the coverage rate is 1 minus the error of exclusion. So we have that the targeting differential TD=1-(exclusion errors + inclusion errors). This gives a similar weight to both errors of targeting in the summary measure. In a scenario in which all and only eligible households get the cash transfer, we have a case of perfect targeting and TD = 1. In the unlikely scenario of all and only ineligible households get the cash transfer, we would have TD= -1. A positive TD that is closer to 1 indicates better targeting.

Given the close link between the eligibility model and the targeting differential, I would expect that targeting differential varies across the Oportunidades' regions mentioned before[77]. Yet, as mentioned in section 6.1, the structure of ENIGH 2008 allows disaggregation only at state level. It is actually the state level the best unit of analysis if one wants to explore budget allocation, as budgets are assigned to administrative units in the states rather than to Oportunidades regions, which act more as a statistical concept.

Table 6.2 shows the targeting differentials and the inclusion and exclusion errors that compose such targeting differential and the mean monthly household income per capita in Mexican pesos (August 2008). The states are ranked from highest to lowest targeting differential.

[77] I would expect that regions with higher penalisation score have fewer targeting errors because there are fewer eligible households to asses. The data suggest quite the opposite, although this cannot be confirmed empirically unless the census data is use for projecting targeting differentials among regions. This is a potential area for further research.

TABLE 6.2 TARGETING ERRORS AND DIFFERENTIALS FOR 16 STATES

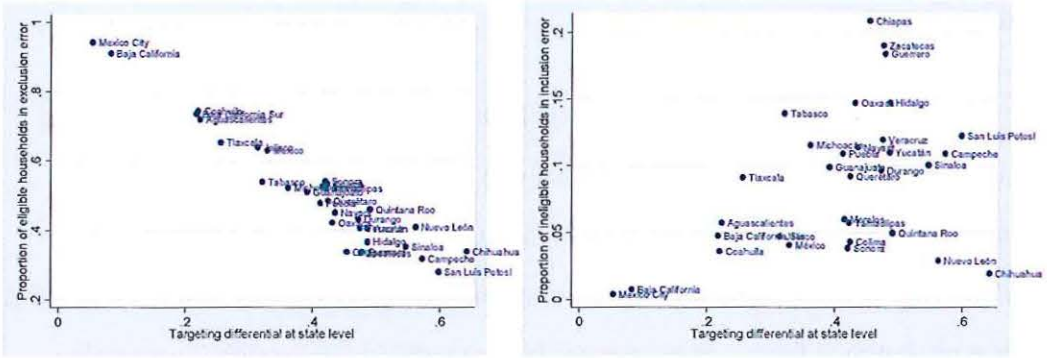
State	Exclusion error	Inclusion error	Targeting Differential (TD)	Region
High Targeting differential				
Chihuahua	0.338	0.020	0.642	Northern region
San Luis Potosí	0.280	0.122	0.598	Tamaulipas,Gorda,Semi-arid
Campeche	0.318	0.109	0.573	Mayan and Tabasco
Nuevo León	0.409	0.029	0.562	Tamaulipas region
Sinaloa	0.353	0.101	0.546	Sinaloan,Nayaritan
Quintana Roo	0.460	0.050	0.490	Mayan and Tabasco
Hidalgo	0.367	0.147	0.486	Gorda, Mexico City
Yucatán	0.405	0.110	0.485	Mayan and Tabasco
Medium Targeting differential				
Guerrero	0.338	0.184	0.478	South Pacific Coast,Balsas River,Mixteca Huasteca
Zacatecas	0.334	0.190	0.476	Semi-arid region
Veracruz	0.406	0.120	0.475	Gorda mountains, Gulf
Durango	0.431	0.097	0.472	Northern, Nayaritan region
Chiapas	0.338	0.209	0.454	Central Chiapas and Mixteca Huasteca
Nayarit	0.451	0.114	0.435	Nayaritan region
Oaxaca	0.422	0.147	0.431	South Pacific Coast,Gulf, Mixteca Huasteca
Colima	0.533	0.043	0.424	South Pacific Coast,Balsas River,Mixteca Huasteca
Querétaro	0.484	0.092	0.423	Gorda, Bajio, Balsas River
Tamaulipas	0.521	0.057	0.422	Tamaulipas region
Sonora	0.542	0.038	0.420	Northern, Sinaloan region
Morelos	0.525	0.060	0.415	Balsas River, Gulf, Mixteca Huasteca
Puebla	0.479	0.109	0.412	Gorda, Gulf
Guanajuato	0.510	0.099	0.391	Semi-arid, Gorda, Bajio, Balsas River
Michoacán	0.523	0.116	0.362	Bajio, South Pacific, Balsas River
México	0.631	0.041	0.329	Balsas River, Mexico city, Gulf
Worst Targeting Differential				
Tabasco	0.540	0.139	0.321	Mayan and Tabasco
Jalisco	0.639	0.047	0.313	Semi-arid,Bajio, South Pacific Coast
Tlaxcala	0.653	0.091	0.255	Gorda mountains
Aguascalientes	0.720	0.057	0.223	Semi-arid
Coahuila	0.744	0.036	0.220	North, Tamaulipas
Baja California Sur	0.735	0.048	0.217	Northern region
Baja California	0.911	0.007	0.082	Northern region
Mexico City	0.943	0.004	0.053	Mexico City

Source: own calculations based on ENIGH 2008 and Oportunidades (2010)

We see that the list is headed by states that are located in both Northern and Southern regions of the country and with different income per capita levels. The states located at the bottom of the list are also from both Northern and Southern regions. From this diversity of regions we also see there is no relationship between penalisation score and targeting performance. It is now important to briefly discuss the relationship between the errors of targeting and the targeting differential. Figure 6.5 attempts to do so by showing in the left panel a scatter plot of the targeting differential and the exclusion

errors in each region. In the right panel we can see the plot of targeting differential against inclusion errors.

FIGURE 6.5 EXCLUSION AND INCLUSION ERRORS AND TARGETING DIFFERENTIAL BY STATE



Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

In theory, targeting differential would decrease as targeting errors increase. And so it happens with exclusion errors that are negatively related to targeting differential. But while this last relationship is straight negative, the relationship with inclusion errors is more complex. There is a positive trend in which as inclusion errors increase so does targeting differential. This is confirmed for states with the worst targeting differentials. But for the highest targeting differential, states may have very low proportion of inclusion errors (such as Chihuahua, Nuevo Leon or Quintana Roo) or medium level of exclusion errors (such as San Luis Potosi, Sinaloa, Campeche and Yucatan). Moreover, the poorest states of Chiapas, Zacatecas and Guerrero show high levels of inclusion errors. As we shall discuss in the next section, targeting differentials show a complex relationship between income poverty, budget allocation and demand for the programme that is to be thoroughly explored.

At this point, it is important to remember that targeting differential is not the only measure to assess targeting efficiency although it is the one that consider inclusion and exclusion errors explicitly. However, other method of assessing targeting, the Concentration Index (CI), is in fact related to household income per capita by state. This is due to the fact that the CI is based on the income distribution and I expect wealthier states to have a wider income distribution that can drive the CI towards negative values,

when, in fact, many households in the low part of the distribution do not get the benefits[78]. Therefore, it is important to know that by considering targeting differential I gain a perspective that differs from the income evidence against the budget constraints hypothesis.

In the following sections I explore two main hypotheses that contribute to explaining targeting differentials at state level. The first one is based on the supply side of the programme and considers budget allocation and the objective population of the programme as the main factors behind the outcomes on targeting efficiency. I argue that the latter institutional factors are important but not sufficient in explaining targeting efficiency. So I then explore a second hypothesis which considers the demand side of the programme. I argue that the demand of the program is not only determined by the eligibility of households, but also by the extent to which conditionality compliance and behavioural change are incentivized. The last are factors that explain local targeting outcomes, especially exclusion errors, and they may also determine the impact of the programme at local levels, as it is discussed at the end of this chapter.

6.3 The supply-side as determinant of targeting efficiency

Some motivations behind the targeting of public policies are the need to increase efficiency under budget constraints and to provide information to policy makers about the recipients of public policies. It is thought that households with lower welfare levels would have the largest marginal benefit of a welfare programme. In the case of Oportunidades, it is also assumed that income poor households tend to underinvest in health and education and would therefore have a greater benefit of a programme targeted towards them.

Oportunidades' proxy means-test model is thought to overcome the information asymmetry as it considers observable characteristics that are closely related to income poverty, such as lack of education, assets or public services. Moreover, Oportunidades'

[78] CI is discussed further in section 6 of Chapter 3.

model also acknowledges that geographical location also informs about the risk of being income poor. For instance, rural areas lower income levels than urban ones, and rural areas are in greater risk of exclusion from public services. Therefore, rural geographical location receives a higher eligibility score than urban one. The model also discriminates according to the affluence of the geographical location, as we have discussed before, and wealthier areas are penalised in their eligibility score.

In the presence of budget constraint, budget allocations would be most efficient if they are assigned on the grounds of eligibility. If eligibility was the only factor behind cash transfer allocation, we would not expect to observe a pattern of errors of targeting. Put it differently, an accurately designed targeting model that is perfectly implemented would discriminate against affluent households and affluent regions and exhibit random pattern in targeting errors among households.

Along this research I have explored that cash transfers are not allocated purely on the grounds of eligibility. It is now useful to compare the exclusion errors with the inclusions errors to understand the relevance of budget constraints. Table 6.3 below shows households in Mexico by eligibility and cash receipt and the percentages add to 100%. Households that fell in inclusion error add up to 5.5% of households in Mexico, while excluded households add up to 9.5% of households. Even in the hypothetical case of perfect targeting, the number of headcount of cash transfers in inclusion error (5.5%) would not be enough to cover the headcount of households excluded (9.5%). Budget constraints then may be important to explaining exclusion errors, but probably less important in explaining inclusion errors.

TABLE 6.3 CASH RECIPIENT HOUSEHOLDS BY ELIGIBILITY

	Non-eligible	Eligible	Total
No Oportunidades	74%	9.5%	83.15%
Oportunidades	5.5%	16.8%	16.85%
Total		100	

Sample size: 28,463,012 households
Source: calculations based on ENIGH 2008 and Coneval (2010)

Budget allocation among regions could be determining the targeting differential because, as we have seen in figure 6.5, some states with good targeting differential also show medium levels of inclusion errors, when the opposite was expected. A subsequent hypothesis of the budget constraints is the trade-off between inclusion and exclusion errors. Policy makers who face budget constraints may wish to avoid inclusion errors in order to improve targeting efficiency. But their efforts may discriminate against eligible households near the eligibility threshold, falling into errors of exclusion, which in fact would just worsen targeting efficiency.

I have mentioned that wealthier states would have lower eligibility scores and therefore, lower probability of excluding poor people even if their budget is not enough. But more affluent states would also receive lower budgets from the central government to fight poverty, in which case they would face a budget constraint. Budget constraint may also explain lack of targeting efficiency. Even if the proxy means-test model is implemented accurately, budget available may not be enough to provide cash transfer to the whole population. If so, I would think that those constraints would be worst in, paradoxically, more affluent areas that face less demand for the programme. Policy makers facing budget constraints may have incentives to allocate cash transfers to the poorest population or the ones closer to the eligibility threshold

I have also mentioned that less prosperous states would face higher levels of eligibility and therefore even more probability of exclusion errors. Yet, given the fact that budget is allocated centrally, more resources could be devoted to fight poverty in states with higher proportion of eligible households. If higher levels of budget are not allocated efficiently, inclusion errors may arise pronouncedly in more deprived states[79]. In summary, the budget constraint hypothesis would expect exclusion errors that are higher for states that experience higher budget constraints, which are, paradoxically,

[79] An additional scenario is that policy makers do not target poor population and use budget to serve other aims beyond poverty alleviation, such as political clientelism. These are not the subject of this research, although it would be interesting to explore it in further research.

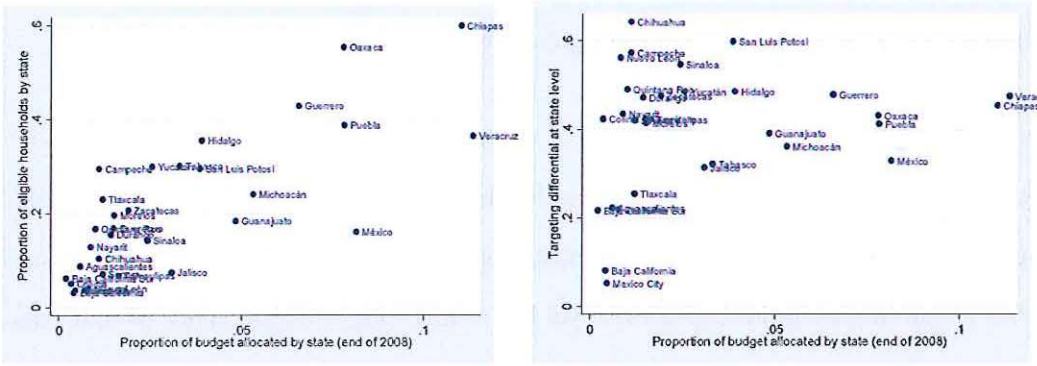
states with lower eligibility score. I would also expect higher inclusion errors in states that receive greater budgets due to their greater eligible population.

From figure 6.4, we saw high proportion of exclusion errors in more affluent states of Mexico City, Baja California and Baja California Sur. We also confirm high inclusion errors in the poorest states of Chiapas, Zacatecas, Guerrero and Oaxaca. Yet, inclusion errors are also important in less affluent yet better-targeting-efficient states of San Luis Potosi, Campeche and Sinaloa.

To illustrate the budget constraint hypothesis, I considered incorporating aggregate administrative data from Oportunidades on the budget assigned to each state by the end of 2008 (DOF, 2008). I now plot in the left panel of Figure 6.6 the budget allocation and the proportion of eligible households per state; in the right panel I plot the budget allocation against targeting differential by state. We see that budget allocation is positively related to the proportion of eligible households. Six states with highest proportion of eligible households receive proportionally higher budgets than the rest of them. Yet Mexico, Guanajuato and Michoacan receive comparatively similar budgets having a lower proportion of eligible households. I expected that higher budget would translate into higher targeting efficiency. But in the right panel I see that states with high budgets show average targeting differentials. Moreover, again in the right panel, states with low budgets show both low and high targeting differentials. The mechanism through which budget constraints would lead to low levels of targeting efficiency is less evident[80].

[80] I bear in mind that this applies to the particular measure of targeting differential. If concentration index is analysed instead, budget allocation may have more to say about efficiently targeting the income poorest households. This is an empirical question for further research.

FIGURE 6.6 ELIGIBILITY, BUDGET ALLOCATIONS AND TARGETING DIFFERENTIAL BY STATE



Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

Arguably, demand for the programme may actually be higher than what the budget assigned to each state could be able to cover. Research mentions that once the programme reached the whole of the territory, the expansion of the number of recipients was suspended. Nowadays it is only possible to incorporate a new household only if an old one has 'graduated' from the programme (Campos-Vazquez, *et al*, 2010). This imposes a budget constraint and it may be more problematic in states that have worse targeting performance, as it would leave them with less leeway to overcome those targeting errors. Yet, Attanasio, *et al* (2008) confirmed with Oportunidades officials that urban areas with initially high take-up rates experienced a drop in take-up rates after the initial figures. The budget constraint hypothesis would then be less supported.

It is now important to understand the budget allocation across states, especially among those who have higher and lower targeting differential. The first set of columns in table 6.4 informs the proportion of eligible households and its confidence intervals for the 8 best and 8 worst targeting differentials[81]. The second set of columns present the proportion of households that receive cash transfers and its confidence intervals[82].

[81] Appendix A contains the information for the complete list of 32 administrative divisions.
[82] The estimation of the variance is carried out following CONEVAL guidelines, which uses the conglomerate method for ultimate sample units. This method agglutinates the primary sampling units that have a similar socioeconomic status and estimates the Taylor-linearized variance. Practically, they recommend the use of Stata svyset command, defining 'linearized' as method, 'factor' as weight, 'est_dis' as strata, and 'upm' as psu (CONEVAL, 2010, p. 81).

TABLE 6.4 PROPORTION OF ELIGIBLE HOUSEHOLDS AND RECIPIENTS BY STATE

State	Eligibility headcount			Oportunidades cash receipt		
	Est	Confidence intervals		Est.	Confidence interval	
Chihuahua	0.104	0.079	0.130	0.087	0.062	0.111
San Luis Potosí	0.295	0.253	0.338	0.299	0.259	0.339
Campeche	0.295	0.258	0.333	0.278	0.244	0.313
Nuevo León	0.040	0.024	0.056	0.052	0.037	0.067
Sinaloa	0.144	0.112	0.175	0.179	0.148	0.210
Quintana Roo	0.168	0.140	0.195	0.132	0.108	0.155
Hidalgo	0.356	0.302	0.409	0.320	0.267	0.374
Yucatán	0.300	0.263	0.337	0.256	0.228	0.283
Tabasco	0.301	0.268	0.335	0.236	0.202	0.270
Jalisco	0.074	0.059	0.090	0.071	0.055	0.087
Tlaxcala	0.231	0.196	0.266	0.150	0.122	0.179
Aguascalientes	0.088	0.065	0.111	0.077	0.046	0.108
Coahuila	0.036	0.024	0.049	0.044	0.031	0.057
Baja California Sur	0.062	0.045	0.079	0.061	0.041	0.082
Baja California	0.032	0.021	0.044	0.010	0.003	0.017
Mexico City (Distrito Federal)	0.037	0.028	0.045	0.006	0.002	0.010

Source: own calculations based on ENIGH 2008 and Oportunidades (2010)

This comparison below is based on confidence intervals, which is not a formal test of difference in means. Such a test would consider the variance of both distributions. Yet, I considered the comparison of confidence intervals more illustrative and this is the reason why I presented this here. Cortes, et al (2007) and Campos-Vazquez, et al (2010) use the informal comparison of confidence intervals as well, and they also warn us about its limitations. There is some evidence of such budget constraint although it does affect states with both high and low targeting differential. For almost all the worst targeting differentials, the proportion of eligible households is higher than the proportion of households that receive cash transfers, and this difference is statistically significant for Mexico City, Baja California and Tlaxcala. Yet, some of the best performing states, such as Chihuahua, Campeche, Quintana Roo, Hidalgo and Yucatan, also experience higher proportion of eligible households than the proportion of households that receive Oportunidades. For San Luis Potosi, Nuevo Leon and Sinaloa, the cash transfer headcount is slightly higher than the eligibility headcount, although differences for better performing states is not statistically significant.

The mismatch between recipients and eligible headcounts affects most of the states, and it is statistically significant for the most deprived states, Chiapas, Oaxaca, Puebla, Morelos and Mexico, in addition to Tlaxcala and more affluent Mexico and Baja California, that had been already mentioned. If most of the states then face potentially higher demand from eligible households than the cash transfers already being delivered in the state, it would evidence that most of the states face potentially positive exclusion errors, because they cannot even cover the eligible population with the number of cash transfers delivered in 2008. Still, it is more difficult to explain efficient allocation of benefits only on budget basis when some states are more efficient than others despite facing similar constraints[83].

This analysis would lead us to conclude that more and less affluent states find it difficult to provide Oportunidades cash transfers to eligible population. Yet, some states transfer cash to a proportion of population that are not eligible. Assuming that states would pursue budget allocation on the grounds of potential demand for the programme[84], at least two different lines of research surge from it. The first one would enquire about institutional factors that contribute to a more or less efficient allocation of cash transfers given the aims of the programme, which are both income poverty alleviation and human capital investment. I won't focus on this type of research as it would imply to focus on the administrative capacity and performance of states.

I rather focus on the question of how households' characteristics and behaviour with regards to human capital shape targeting outcomes. I argue that information about the constraints households face when participating in Oportunidades at state level by reconsidering the behavioural change attained and the cost of participation of household. I also argue that such information has been elicited in chapter 5 and that by disaggregating it at state level I can understand local differences in targeting efficiency.

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[83] Yet, a definitive statistical test of this hypothesis would have to contrast the information inferred from the survey with the actual administrative records. This would also imply to project the proportions of eligible and cash recipient over the whole population in a census dataset, by using methodologies such as the poverty mapping. This is a potential area for further research.

[84] This may not be the case if policy makers pursue other goals than poverty alleviation, such as political clientelism, although this issue is not subject of this research.

In the next section I firstly describe human-capital related investments and behavioural change at state level and secondly I present the relation between behavioural change, costs of participation in Oportunidades and targeting efficiency outcomes.

6.4 Demand-side of the programme and targeting efficiency

Figure 6.6 showed that targeting differentials do not vary according to budget allocation. In this section I present evidence on the extent to which we could examine this problematic from the demand-side of the programme. There is an important caveat that I bear in mind when the following analysis. In the previous section of this chapter I have related targeting efficiency at state level to a variety of outcomes at state level. However, I will now be understanding individual behaviour –this is investment in health and education— aggregated at a state level. The problem at the moment of interpreting these results is that there will be a correlation between aggregate levels of human capital investment and individual tendency to invest in human capital due to a local effect that has no relevance for individuals. This could lead to draw incorrect conclusion at individual level from aggregated data. Hence, I should avoid the risk of jumping into those conclusions by remembering that elements such as the local structural factors, such as prices, rewards for human capital in labour market, or features of public administration, may be behind of high levels of human capital instead of pure individual propensity to such investment.

Having outlined this warning, I still justify the look at aggregate level of human capital investment. I firstly do so because as mentioned in the introduction, one of the aims of this thesis is to interpret how behavioural and structural characteristics in context in order to find the factors that affect participation in Oportunidades (Van Oorschot, 1995). In such case, given the structural constraints and behavioural determinants, I argue that individuals have still some degree of decision when participating in Oportunidades. Hence, the aggregate individual response may tell us not much about individual but more about the context in which they participate in Oportunidades and common or different set of incentives.

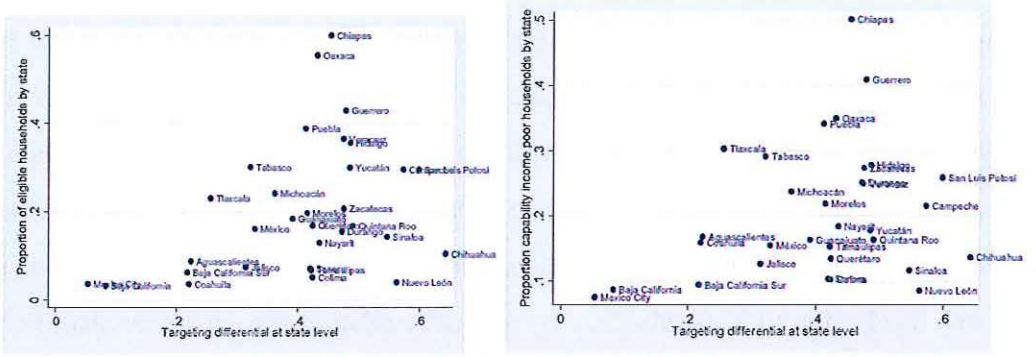
Secondly, I look at aggregate level of individual behaviour as research has argued that the cash incentive may not be enough to overcome the opportunity cost of participation and induce behavioural change (De Janvry and Sadoulet, 2006 and Attanasio, *et al*, 2008). Still, they recognize that it is difficult to quantify such opportunity cost of school attendance, especially among secondary school teenagers, as the search of alternative source of income is not the only reason to drop out school. I would like to enquire if such opportunity costs are reflected at aggregate level and if this is one of the factors behind low participation in Oportunidades.

In this section I present the levels of preventive health check-ups and school attendance and behavioural change at state level. I then analyse how those local characteristics are related to cash transfer allocation and costs of participation in Oportunidades. I argue that by disaggregating the national picture at regional level and including demand-side factors into this analysis I have gained a better understanding of targeting efficiency, in particular of the structural factors behind exclusion errors.

A first approximation to assess the potential demand of the programme at local level is to understand poverty at state level. Concentration of either income poverty or eligible households could be a factor behind the targeting differentials that we observe[85]. In previous chapters I have discussed that mean-testing does not determine the allocation of benefits and provided evidence that lack of compliance with conditions and behavioural change were behind lack of participation. Still, we have to inquire whether targeting efficiency is influenced by poverty headcount at state level. Figure 6.7 presents in the left-hand-side panel the proportion of eligible households by state and each state's targeting differential. In the right-hand-side we see the capability income poverty headcount and the targeting differential by state.

[85] As discussed in previous chapters, there are at least two different concepts of poverty involved in Oportunidades. Although Oportunidades targets income poverty, it does so by a proxy means-test that also captures underinvestment in human capital. As a result, the population considered income poor is not precisely the same as the one considered eligible. This thesis focuses on the definition of poverty as eligible households, although its relation to income poverty has been analysed above in chapter 4.

FIGURE 6.7 ELIGIBILITY, INCOME POVERTY AND TARGETING DIFFERENTIAL BY STATE



Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

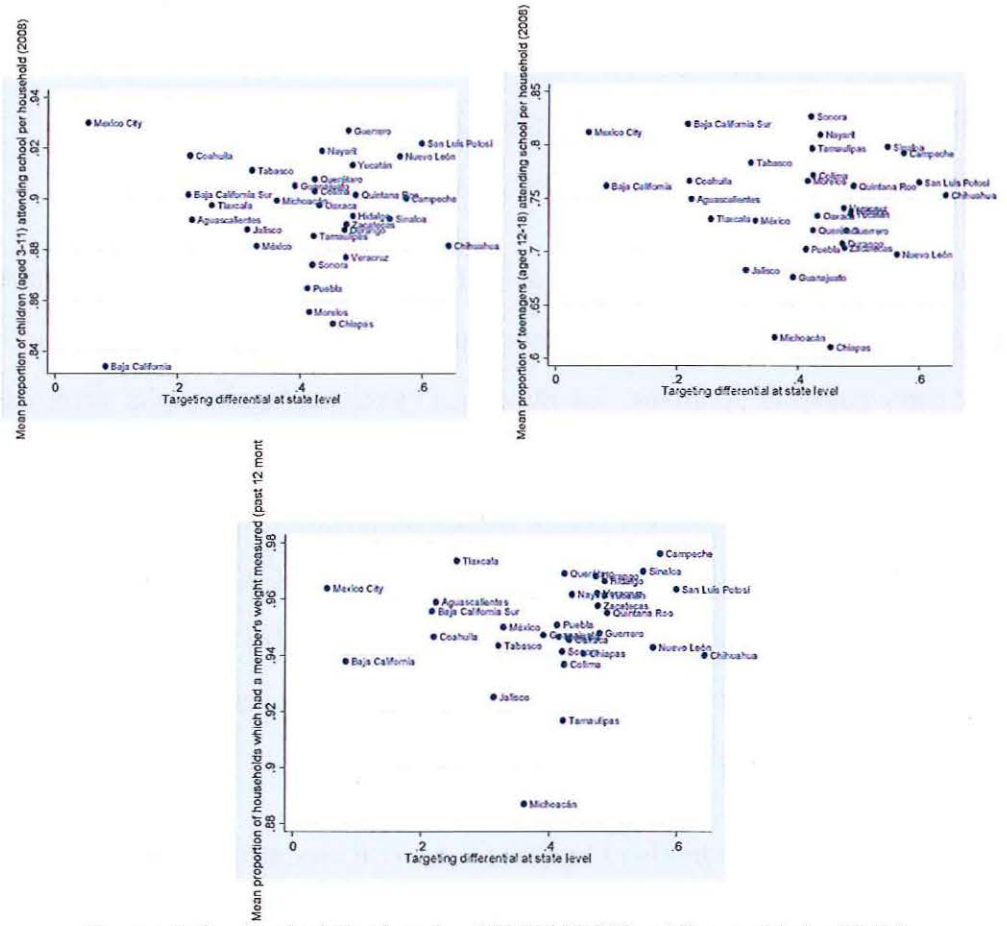
Regardless whether we speak about income poverty or eligibility, the poverty headcount does not seem to be a driver of targeting efficiency. There is a positive trend in which states with high deprivation headcount (either on income or eligibility) tend to have better targeting efficiency as opposed to those who have low deprivation incidence. Still, the states with better targeting performance have medium to low levels of eligibility and income poverty. In fact, since budget allocation follows roughly eligibility headcounts, I now can confirm that targeting efficiency could not be explained solely on either budget allocation or general poverty headcounts.

Oportunidades aims to increase human capital investment and, as mentioned in chapter 4 and 5, conditionality of school attendance is one of the most important factors behind lack of participation in Oportunidades. Success in increasing human capital investment may rely to a great extent in current levels of school and health check-ups attendance. Previously, we have discussed that school attendance for children and teenagers tend to follow levels of affluence, being higher in wealthier regions of Mexico. This evidence actually supports the introduction of cash transfers to incentivize school attendance. Yet, I have found also that preventive health check-ups do not necessarily increase or decrease with income levels.

The relationship between levels of human capital activities and targeting efficiency is not straight forward, as graph 6.8 below shows. On the top left panel we can see the mean proportion of child school attendance and state's targeting differentials, while the top right panel plots the mean proportion teenage school attendance and state's targeting

differential. Again, there is no simple relationship between school attendance and targeting differential. Although states with high targeting efficiency show high levels of school attendance for both children and teenagers, school attendance is also high in states with low targeting performance. In such states, even if individuals attend school and health check-ups, they coincide with low participation. Moreover, some of the income poorest states like Chiapas, Guerrero, Oaxaca and Puebla show no particular high or low levels of school attendance. Any particular relationship cannot be identified between health conditionality and targeting efficiency either, as presented in the bottom central plot in figure 6.8. Exclusion and inclusion errors do not show any clear relationship with school attendance either, and I do not present such graphics here to avoid repetition.

FIGURE 6.8 SCHOOL AND PREVENTIVE HEALTH ATTENDANCE AND TARGETING DIFFERENTIAL BY STATE

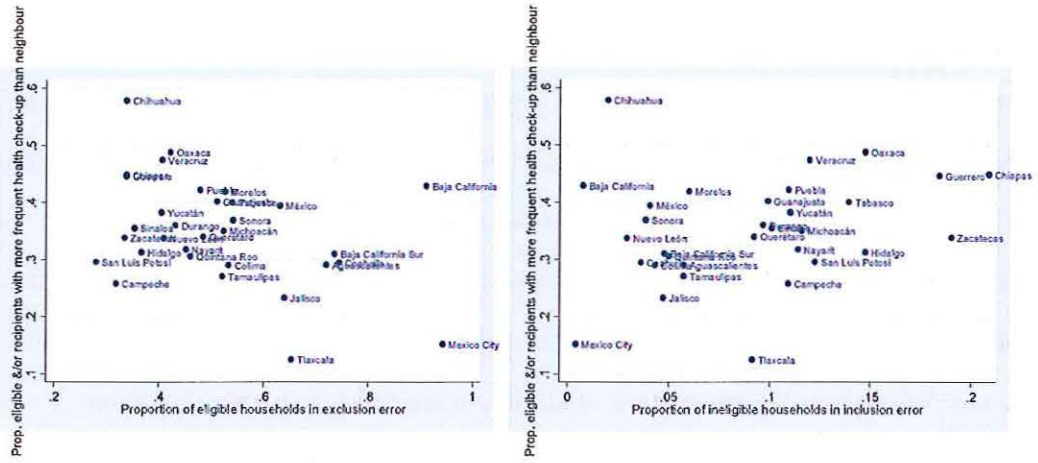


Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

In section 4.3.3 above I discussed that the aim of Oportunidades is engender investment in human capital but also in inducing behavioural change. And figure 4.5 showed that great proportion of households at state level attend health check-ups (only Michoacan shows attendance of less than 90% of its population). This leads me to infer that it is relatively frequent that households attend health check-up and that such condition would not be of concern for non-participation of eligible households. Moreover, the extent to which households attend more frequent to health check-up than their neighbours is limited, as most population attends frequently. I first measure state's behavioural change as the proportion of eligible households (whether recipients or not) and ineligible recipients who showed higher attendance than their neighbours. If I assume that health attendance is high enough to represent a concern for participation, then I expect that high health check-ups attendance would be associated to low proportion of exclusion errors.

In graph 6.9 I plot on the left-side panel health-related behavioural change and exclusion errors. We see that Tlaxcala and Mexico City have very low levels of health-related behavioural change and high exclusion errors. Yet, Aguascalientes, Baja California Sur and Coahuila with high exclusion errors achieved medium of behavioural change, and Baja California has high levels of behavioural change and a large exclusion error. In only those states some eligible households that have already achieved behavioural change are not claiming benefits, which is puzzling. I hypothesise there may be high costs associated with claiming for eligible households. Moreover, the lowest levels of exclusion errors are achieved at higher behavioural change in Chihuahua but medium to low behavioural change in San Luis Potosi or Campeche. In the right-side panel of figure 6.9 I plot health-related behavioural change and inclusion error. States with lowest inclusion errors, such as Chihuahua, Baja California and Mexico City experience both high and low proportions of health-related behavioural change. Whilst health-related behavioural change is achieved by most Oportunidades recipients and eligible non-recipients, its relation to targeting efficiency is not clear.

FIGURE 6.9 PROPORTION OF ELIGIBLE HOUSEHOLD THAT HAD HEALTH CHECK-UPS MORE FREQUENTLY THAN THEIR NEIGHBOURS BY STATE AND TARGETING DIFFERENTIAL

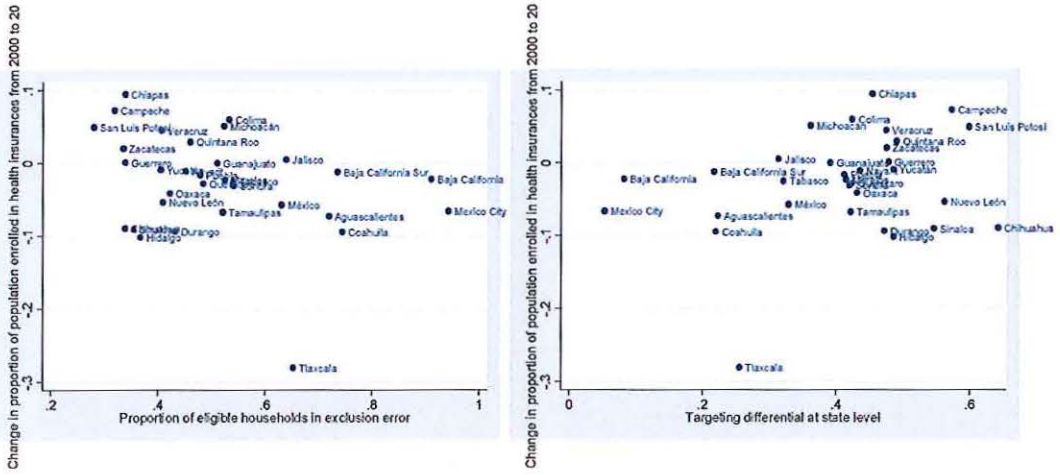


Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

It may be argued that this measure of behavioural change does not capture the change in behaviour over time. I have argued in section 2.7 that even nuanced information on participants' past behaviour may not necessarily elicit a policy-induced change of behaviour[86]. Yet, it has been suggested that I also compare targeting differentials with measures of changes in human capital investment over time. So I gathered the changes in the proportion of population enrolled in the main public health insurance provider (Instituto Mexicano del Seguro Social IMSS) from 2000 to 2010, at local level. Figure 6.10 portrays the relationship of those changes and the targeting differential of Oportunidades in 2008. On the left side we see the exclusion errors and the change in proportion of population enrolled. We see that those states with positive change or increase in the proportion of population enrolled experience the smaller proportion of exclusion errors. At the same time, states in which the proportion of population enrolled decreased tend to face medium to large proportion of exclusion error. The figure also shows the relation between changes in enrolment and targeting differential. States experiencing an increase in enrolment tend to also experience larger targeting differentials.

[86] Change in behaviour over time may reasons beyond the scope of policy. Hence the need to look at alternative measures of behavioural change that are related to policy, such as the change in behaviour across peers.

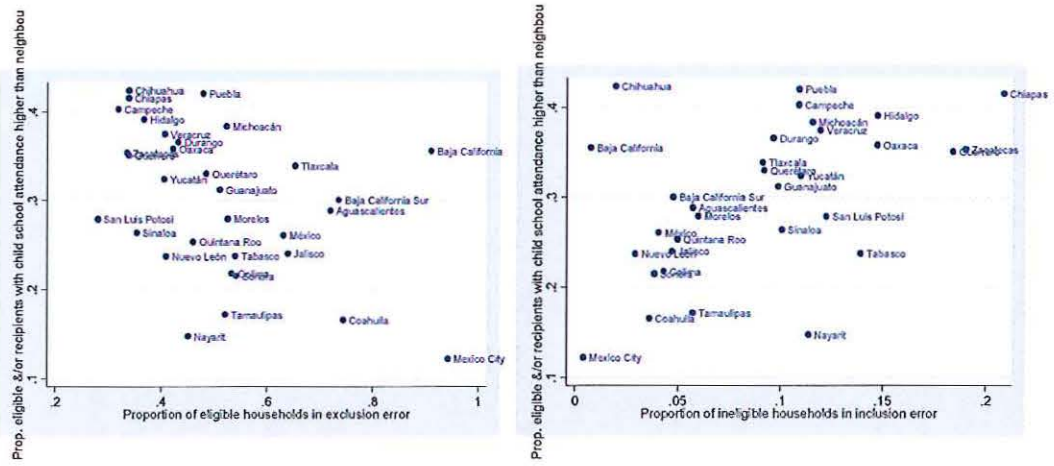
FIGURE 6.10 CHANGE IN PROPORTION OF POPULATION ENROLLED IN THE MAIN PUBLIC HEALTH INSURANCE PROVIDER (IMSS) BY STATE AND TARGETING DIFFERENTIAL



Source: Author's calculation based on ENIGH (2008), Oportunidades (2010) and INEGI (2014)

School-related conditionality may represent an additional barrier to participation in Oportunidades. We have discussed before that levels of school attendance at state level does not seem to explain targeting differentials. Yet, we have yet to explore school-related behavioural change. As mentioned in section 4.3.3, primary school behavioural change is higher among eligible households. Given the fact that most households send children to school, I would expect that barriers to child school attendance are relatively low and lack of participation due to lack of school attendance is not an issue. However, I presented evidence in table 4.3 about the lower levels of child school attendance among excluded households. Therefore, the association at state level is ambiguous. Figure 6.11 contains two graphs where school-related behavioural change of eligible households is plotted against exclusion errors. On the left side we see the proportion of eligible households that show higher child school attendance than their neighbours by state and the exclusion errors. In general, there is a negative relation between child-school-attendance behavioural change and exclusion errors. But I observe that the high levels of behavioural change can be associated to numerous exclusion errors, as in Baja California (and Baja California Sur and Aguascalientes to a lesser extent), or exactly the opposite in Nayarit.

FIGURE 6.11 HOUSEHOLD WITH CHILD SCHOOL ATTENDANCE HIGHER THAN NEIGHBOURS BY STATE AND TARGETING DIFFERENTIAL



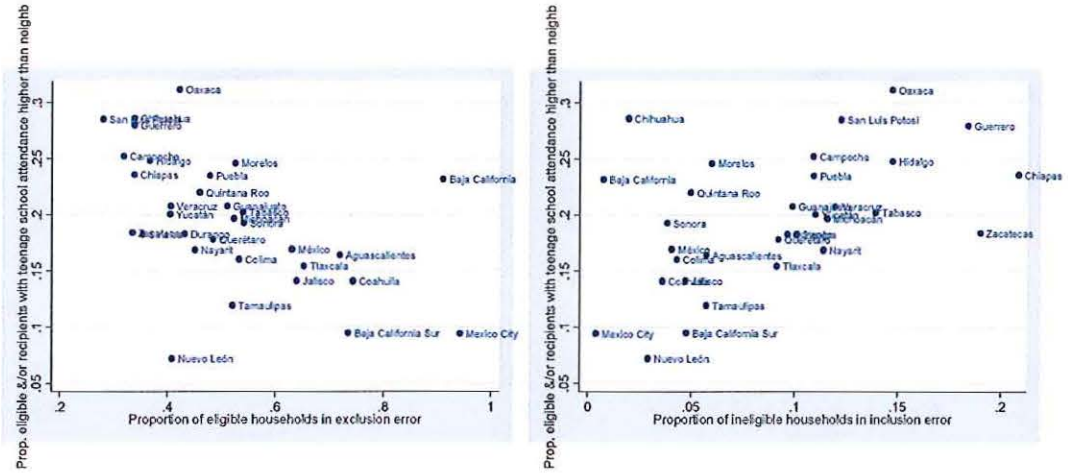
Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

On the right-hand side of figure 6.11 I plot the proportion of inclusion error and behavioural change related to child-school-attendance. The relation between them is negative. Mexico City shows the lowest behavioural change and inclusion errors, and states with higher levels of behavioural change showed also numerous inclusion errors. Again, Baja California show high child-school-attendance behavioural change and low inclusion errors, while Nayarit shows low levels of behavioural change and medium levels of inclusion errors. As I reflected when discussed figure 6.9, eligible households excluded may be facing high claiming costs in those regions.

Cash transfers are higher for secondary education school attendants, due to the fact that there is lower teenage school attendance and also lower proportion of households that attend more frequently than their neighbours. Figure 6.12 provides information about this, as we can observe, in the left panel, a negative association between teenage-school-attendance behavioural change and exclusion errors. Yet Baja California is an outlier that shows considerably high levels of behavioural change and high proportion of excluded households. Claim cost in such areas may prevent eligible households from receiving the cash transfer. And Nuevo Leon has a smaller proportion of households whose teenagers attend school more frequently than their neighbours, which also coincides with Nuevo Leon's low proportion of eligible households excluded from the

programme. The right side panel shows that the proportion of secondary-school related behavioural change is positively related to inclusion errors, but the relation is far from clear. Again, Baja California, Chihuahua and other states depart from this tendency.

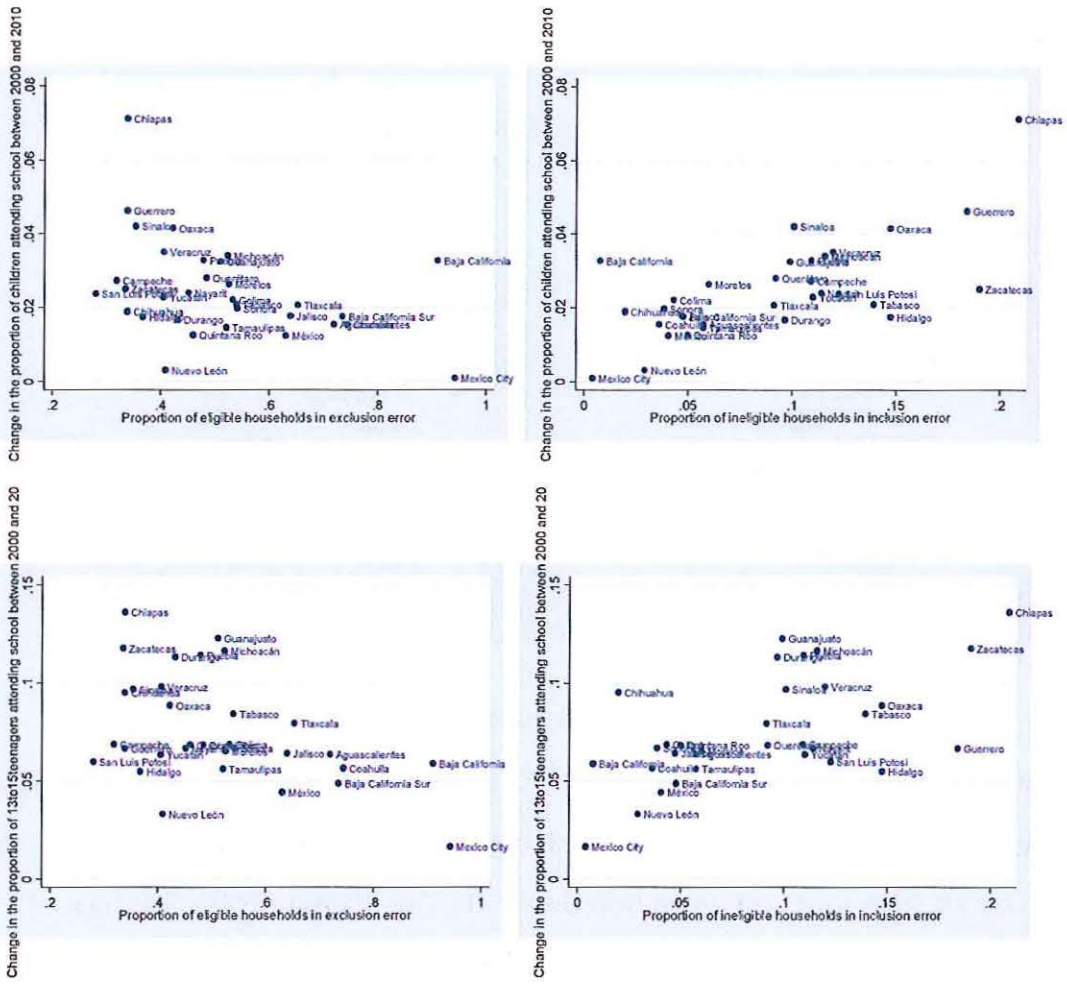
FIGURE 6.12 PROPORTION OF ELIGIBLE HOUSEHOLDS WITH HIGHER TEENAGER SCHOOL ATTENDANCE THAN NEIGHBOURS BY STATE AND TARGETING DIFFERENTIAL



Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

It may be argued that the data on graphs 6.11 and 6.12 do not fully account for changes in behaviour over time. I have mentioned that I understands behavioural change as the divergence from own peer-group behaviour following Manski (1993). But figure 6.13 adds indicators that capture changes in time and plot their relation to targeting differentials. On the upper panel we see the changes in the proportion of children aged 6 to 12 years old attending school between 2000 and 2010 by state. On the lower panel we see the changes in the proportion of people aged 13 to 15 attending school between 2000 and 2010. On the left-hand side I plot this information against exclusion errors, while inclusion errors are plotted on the right-hand side.

FIGURE 6.13 CHANGES IN PROPORTION OF CHILDREN AND TEENAGERS ATTENDING SCHOOL BETWEEN 2000 AND 2010 BY STATE AND TARGETING DIFFERENTIAL



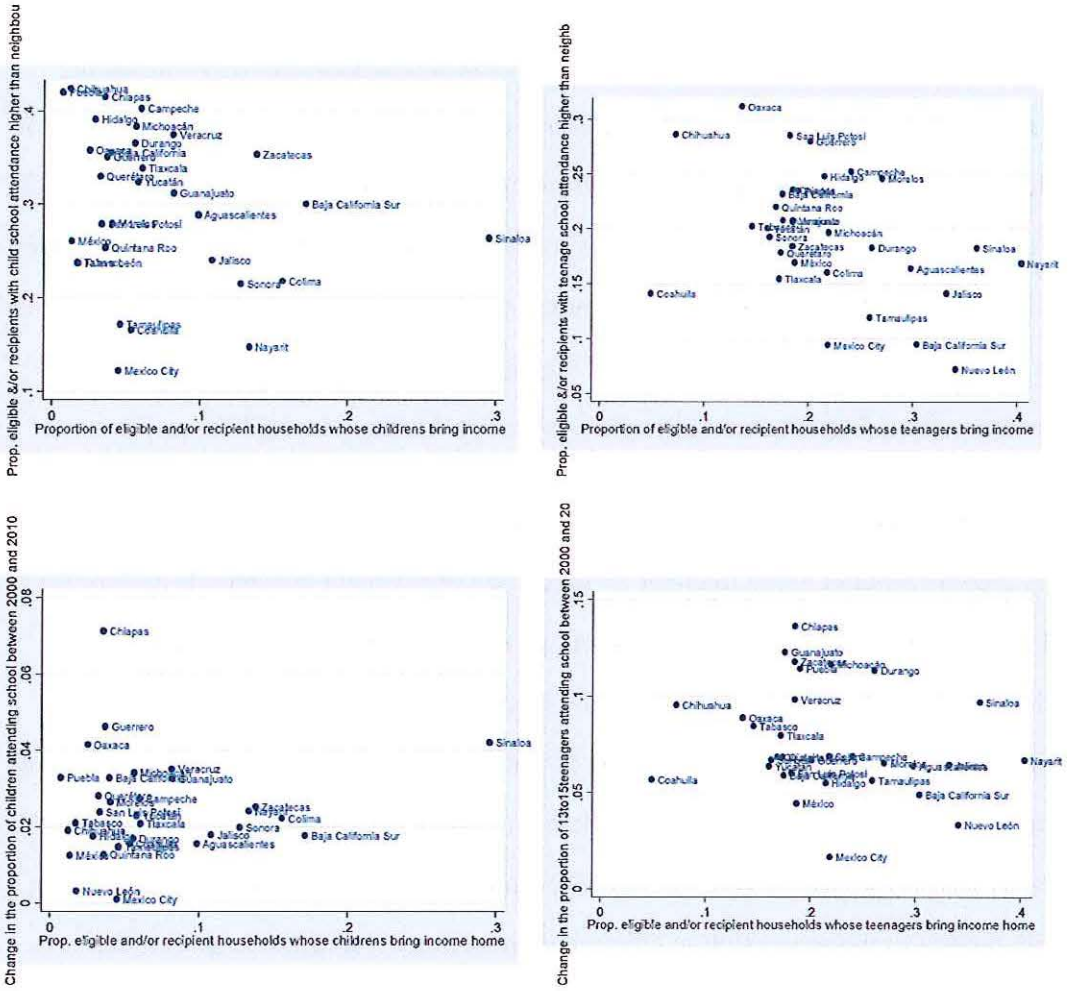
Source: Author's calculation based on ENIGH (2008), Oportunidades (2010) and INEGI (2014)

In general, the plots in figure 6.13 confirm our conclusions before: there is association between the extent to which behavioural change occurs and the exclusion and inclusion errors. The behavioural change is measured in terms of either investment in human capital larger than their neighbours, and in terms of changes in investment between 2000 and 2010. Positive behavioural changes are associated to smaller exclusion errors and larger inclusion errors, although this last association is less clear. As mentioned in chapter 2, this analysis reflects on the context in which individuals make such decisions. I would now explore why some states are fertile for such investment in human capital, which is associated with higher levels of participation of eligible households.

There are at least two different mechanisms through which behavioural change may be discouraged. One may be the high opportunity cost of the income forgone due to school attendance of children and teenagers. Attanasio, *et al* (2008) argued that the last is one of the main costs of participation. Note that this mechanism would privilege the reasoning that income poverty and monetary costs are behind underinvestment in human capital. Arguably, a conditional cash transfer can change the relative prices of education and preventive health *vis a vis* labour market in order to induce conditional action. However, Azevedo, *et al* (2009) pointed out that Oportunidades recipients tend to have both cash transfers and other sources of income rather than one or the other and De Janvry and Sadoulet (2006) warned that lack of money is not the only reason for teenage-school dropout among Progresa recipients in rural areas (2006, p. 7).

Figures 6.14 and 6.15 explore the relation between school-related behavioural change and income brought home to either eligible and / or recipient households. Figure 6.12 plots, in the upper left panel, the proportion of such eligible or recipient households whose children bring income home and the proportion of behavioural change with respect to child-school attendance. The lower left panel plots the proportion of eligible and recipient children who bring income home and the change in proportion of children school attendance from 2000 to 2010. The tendency of behavioural change is unclear with respect to the proportion of children earn income. The negative association between behavioural change and teenager's income is clearer. The upper right panel shows a negative relation for teenage school behaviour and income earned among teenagers in eligible and/or recipient households. The lower right panel shows how the proportion of teenagers who earn income declines as the proportion of those who attend school increases. This negative relation was expected, as there may be a trade-off between school attendance and the possibility of bringing income home.

FIGURE 6.14 SCHOOL-RELATED BEHAVIOURAL CHANGE AND INCOME EARNED BY CHILDREN AND TEENAGERS



Source: Author's calculation based on ENIGH (2008), Oportunidades (2010) and INEGI (2014)

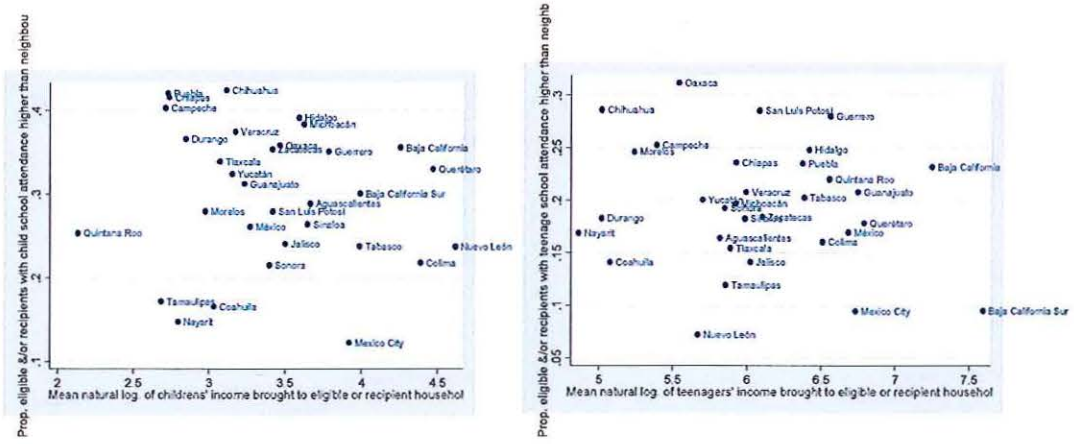
However, if income was the main driver for this participation, I would expect to see similar negative trends between income earned and behavioural change. But this is not always the case. Figure 6.15 plots, in the upper left panel, children behavioural change and mean natural logarithm of children's income brought to eligible and/or recipient households. It is interesting that the tendency is similar to the upper left side panel in figure 6.12: behavioural change diminishes as income brought home increases. Comparing both upper left and right side panel in figure 6.15, we see that mean value of the natural logarithm of income earned by children is smaller than the mean value of the natural logarithm of income earned by teenagers. The former goes from a scale of 2 to

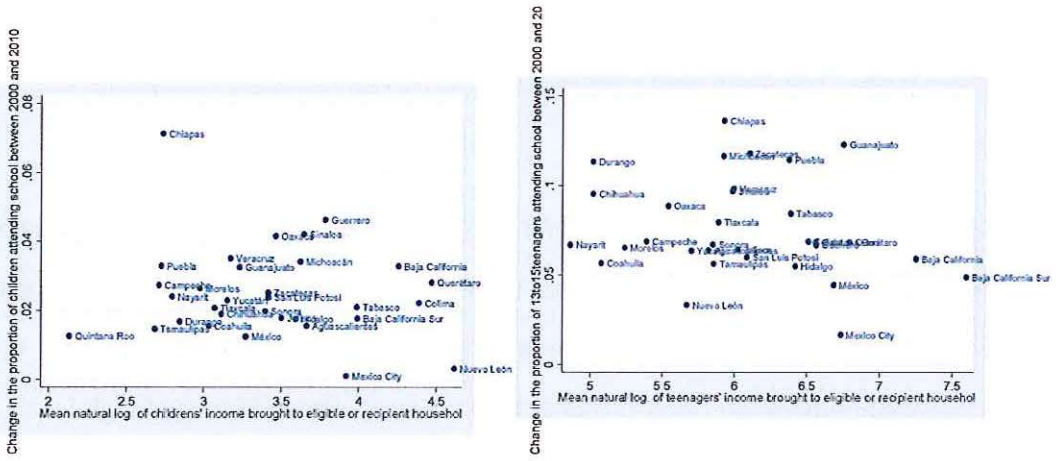
4.6, equivalent to 8 to 100 pesos; the latter goes from a scale of 4.8 to 7.5, equivalent to 129 to 1985 pesos. The mean cash transfer reported by Oportunidades recipients was 603 pesos.

In the upper right-side panel we also see that the extent to which teenagers attend school more frequently than their neighbours is not necessarily associated with higher or lower income earned by teenagers in the state. In the upper left-side panel, there is a slight positive association between lower levels of income earned by children and high levels of eligible households and recipients that send children to school more frequently than their neighbours.

The lower panels plot the indicators of behavioural change from 2000 to 2010 at state level, but there are no clear trends between such definition of behavioural change and income. I could pose the argument that even though children bring less income home, the extent to which it is possible to induce behavioural change would be slightly more responsive to the relative price of education, although the relationship is far from evident. Yet, the main conclusion I draw is that the possibility of bringing income home may not be the only deterrent to education-related conditional actions.

FIGURE 6.15 SCHOOL-RELATED BEHAVIOURAL CHANGE AND MEAN NATURAL LOGARITHM OF INCOME BROUGHT HOME BY CHILDREN AND TEENAGERS

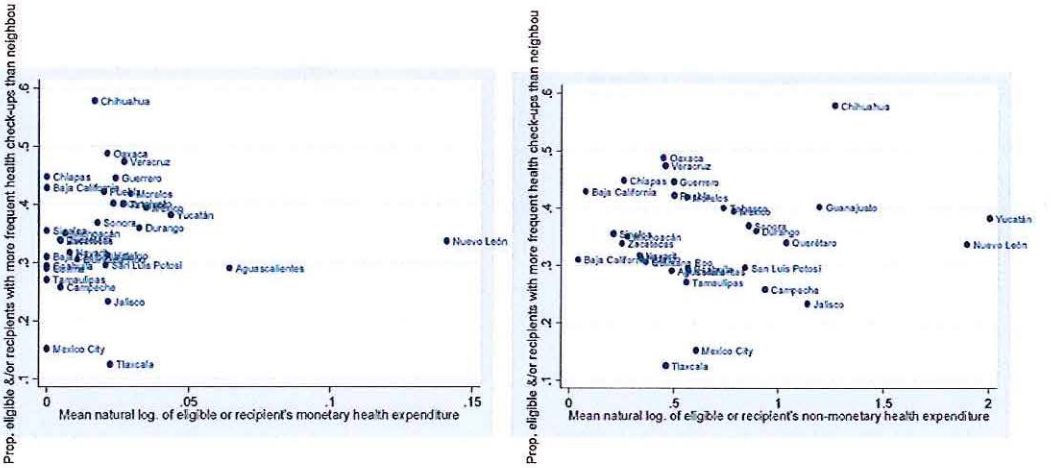


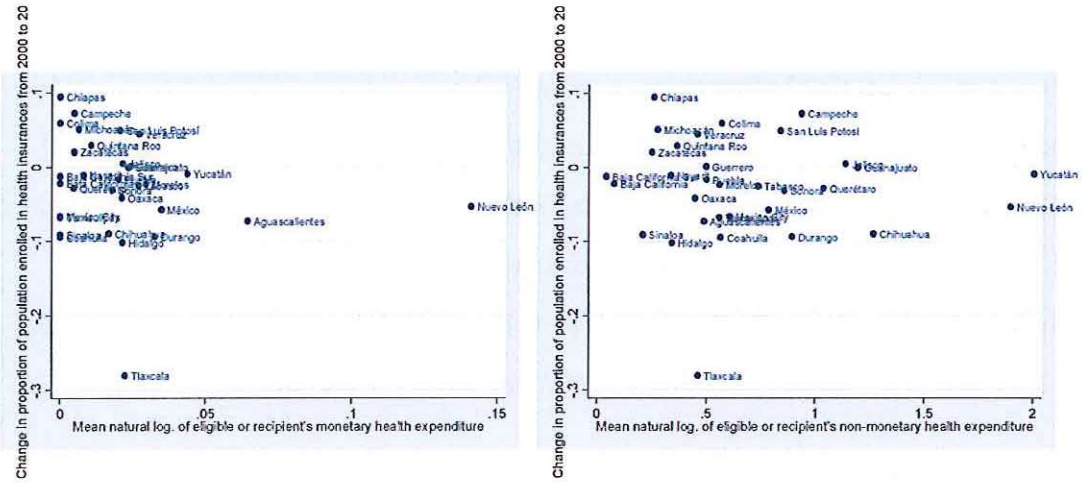


Source: Author's calculation based on ENIGH (2008), Oportunidades (2010) and INEGI (2014)

Neither monetary nor non-monetary health expenditures are related to the extent to which recipients and eligible non-recipients have health check-ups more frequently than their neighbours. Figure 6.16 plots, on the upper panels, the mean natural logarithm of both monetary (left-side) and non-monetary (right-side) health expenditure and health related behavioural change. The lower panels plot changes in enrolment to the IMSS from 2000 to 2010 and health expenditures. I do not distinguish a particular trend for eligible and/or recipients' behavioural change in terms of health and their health expenditure. Trends are particularly difficult to follow due to the presences of outliers like Tlaxcala, Mexico City, Chihuahua, Yucatan and Nuevo Leon that show precisely the opposite: the higher the non-monetary health expenditure, the higher the extent to which health-related behavioural change may be observed.

FIGURE 6.16 HEALTH-RELATED BEHAVIOURAL CHANGE AND MEAN NATURAL LOGARITHM OF MONETARY AND NON-MONETARY HEALTH EXPENDITURE





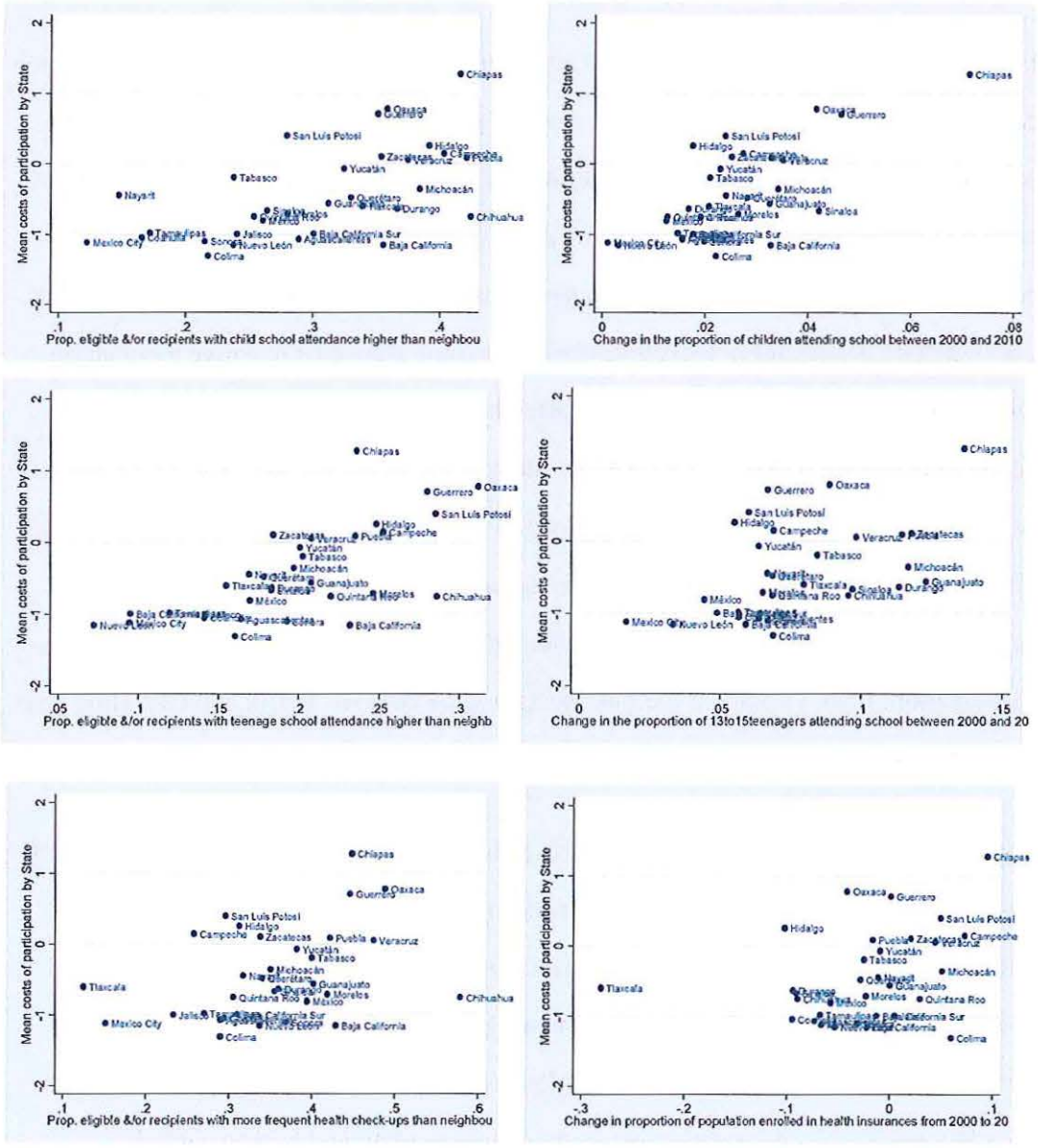
Source: Author's calculation based on ENIGH (2008), Oportunidades (2010) and INEGI (2014)

I have discussed that income earned in labour market does not seem to be related to the extent to which recipients and eligible non-recipients send children and teenagers to school more often than neighbours. Likewise, aggregate health expenditure does not seem to be associated to the extent to which such vulnerable households have preventive health check-ups more frequently than their neighbours. Yet, for teenagers, the aggregate headcount of teenagers earning income showed a negative association with the extent to which such teenagers attend school more frequently than their neighbours. This led me to enquire about the specific link between income and the so called opportunity cost or cost of participation in Oportunidades, which seems to exist given the headcount relationship, but it is not related to income levels. I am then particularly interested in exploring whether a dislike for the conditions is behind non-participation. If so, I would argue that the aggregate value of health and education goes beyond its relative price in the labour market.

I now bring back the costs of participation estimated in chapter 5, section 5.3.2, which is argued to capture the latent unobserved costs of participation. I put forward that this unobserved costs captures better the value of human capital. I argue that unobserved costs of participation may capture particular likes or dislikes towards human capital investment that are not related to the market value of human capital. In this sense, it goes beyond the simpler relation between income poverty and human capital investment. The unobserved cost of participation may speak about the fact that income

poverty is not the only mechanism that prevents human capital investment. I conclude this given the fact that I observe low levels of behavioural change in both high and low income poverty regions. The costs of participation highlight that there are contextual factors, beyond the price of human capital in the market, that contribute to exclude some eligible households from health and school attendance and from the cash transfers. Figure 6.17 plots three different behavioural changes and the mean cost of participation at state level for recipients and eligible non-recipients of Oportunidades.

FIGURE 6.17 BEHAVIOURAL CHANGES AND MEAN COSTS OF PARTICIPATION AT STATE LEVEL



Source: Author's calculation based on ENIGH (2008), Oportunidades (2010) and ENIGH (2014)

The first thing worth noting is the scale of the states' mean costs of participation. Most of the States lay between the -1 to 1 interval except for extreme cases such as Chiapas, Mexico City, Nuevo Leon, Colima, Baja California and Baja California Sur, which we have discussed that are the wealthiest and poorest regions. The analysis in chapter 5 highlighted that non-recipient households have negative values of costs of participation, while recipients have a positive value of costs. When obtaining the mean value at state level, clearly Chiapas will have an overall positive average as it accumulates the largest number of recipients. Meanwhile, the most affluent regions will have a negative value of costs as they are inhabited mostly by non-recipients.

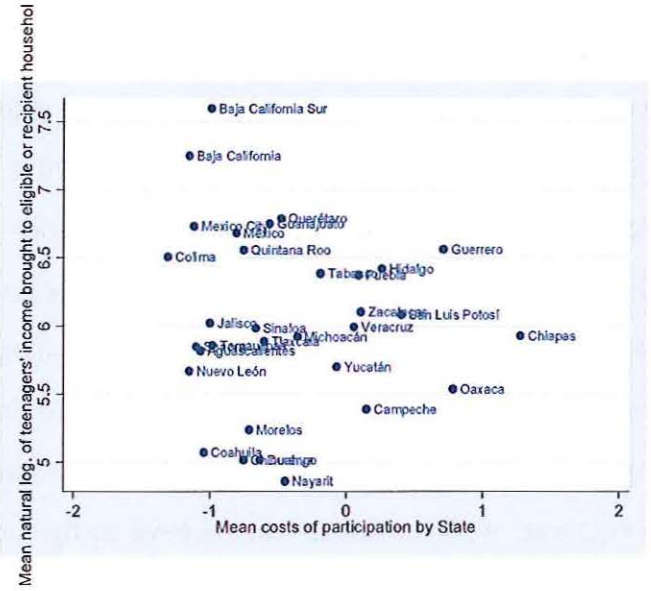
Secondly, I observe a positive association between mean costs and human-capital-related behavioural change at state level. This is true for both of the measures for behavioural change, either in terms of behaviour more frequent than neighbours or in changes in enrolment and school attendance from 2000 to 2012. On chapter 5 I discussed that costs were associated with compliance of conditions. If we understand from the graphs above that high mean costs are associated to high poverty levels, hence high poverty levels and costs may also be associated with larger investments in human capital. Yet, the relation is not that clear, especially if we consider the indicators that measure changes over time. If so, it may be relatively easier to claim Oportunidades if you already were to comply with conditions, but the most costly conditionality is, the larger errors of exclusion we may get.

Thirdly, one of the main assumptions of CCTs is the idea that income constraints determine human capital investment. Azevedo, *et al* (2009) highlighted that the income forgone by children and teenagers may not cause participation in the programme. As discussed, there is little relationship between income earned or health expenditure and behavioural change.

In chapter 5 I discussed that recipients' costs of participation are not as large as to discourage participation that may be higher than the income obtained as cash transfer or in the labour market. Below, figure 6.18 plots the relation between the cost of

participation in Oportunidades and the mean value of the natural logarithm of income brought home by eligible and recipient teenagers at state level. There is no clear pattern of association. It is an empirical question whether the unobserved costs of participation could be offset by a sufficiently high monetary transfer to induce behavioural change among households that underinvest in human capital, which is a source of inefficiency (De Janvry and Sadoulet, 2006). This would have to be assessed by a model of participation in school attendance or preventive health check-ups that needs information beyond the ones that the model of Oportunidades' participation can provide.

FIGURE 6.18 MEAN COSTS OF PARTICIPATION AT STATE LEVEL AND INCOME BROUGHT BY TEENAGERS

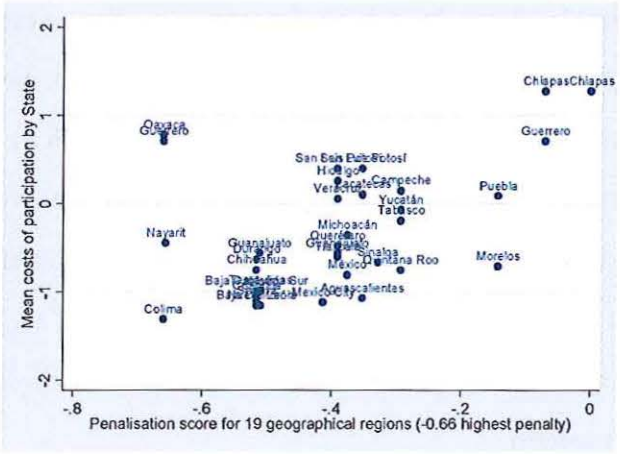


Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

It is important to mention that the costs of participation do have a relation to other regional relative prices that shape income poverty and eligibility. Figure 6.19 plots the mean cost of participation for recipients and eligible non-recipients and the regional penalty score presented in section 6.1 above. Some names of the states are repeated as they have more than one penalty score. We can see a positive relation in which costs of participation tend to increase as the regional penalty scores approaches to 0. The positive relation between penalty scores and cost of participation is not surprising if we

think that the unobserved cost of participation is a function of the residuals of the model in chapter 4. The residuals of those models are the allocation of cash transfer that cannot be explained by individual characteristics. And one important factor determining allocation of cash transfer that lies beyond the household is the regional score. Therefore, the unobserved costs considered the inequality among regions in Mexico.

FIGURE 6.19 MEAN COSTS OF PARTICIPATION AT STATE LEVEL AND REGIONAL PENALTY SCORE



Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

In conclusion, underinvestment in human capital is one of the most important factors behind lack of participation in Oportunidades. In this section I presented the levels of preventive health check-ups and school attendance and behavioural change at regional level. I then analysed how those local characteristics are related to cash transfer allocation and costs of participation in Oportunidades. By disaggregating the national picture into regions and including demand-side factors into this analysis I have gained a better understanding of targeting efficiency, in particular of exclusion errors. Exclusion errors are responsive to the likelihood of inducing behavioural changes, particularly to education-related behaviour. This is due to the cost of participation.

In the previous section I concluded that budget allocation cannot entirely predict variation of targeting differentials[87] so I looked at human capital investment and

[87] The Concentration Index would be more responsive to budget allocation, but it has been discussed in section 6 of Chapter 3 and in section 6.1 above that the targeting differential is better suited for our analysis.

behavioural change to understand exclusion errors. States with negative incentives for households to send children to school and participate in preventive health are performing worse in targeting although they are allocating resources to the income poorest households. This is also confirmed by the analysis of the costs of participation, in which higher costs for participants makes it less plausible to induce behavioural change.

Policy makers at local level may have fewer incentives to allocate cash transfers according to the eligibility model if they realize it does not reflect the poverty levels or the potential of households to comply with conditionality. In such a case, we could observe bad targeting differentials and, yet, good poverty outcomes of the programme. Policy makers may be interested in allocating cash transfers to households near to the eligibility threshold in order to attack poverty more efficiently. If so, a worst targeting performance could arguably lead to better poverty outcomes. These lines of thought are explored in the next section.

6.5 Targeting differential and impact on income poverty

Being the relationship between targeting efficiency and policy impact and empirical question, it can be explored in different ways. In the previous section I highlighted that at state level, exclusion errors are related to low levels of behavioural change due to high cost of participation in the states. An additional objective is to reduce income poverty by the means of cash transfers. In fact, the evaluation of the impact of the programme on income poverty has been the objective of significantly fewer evaluations if compared to the evaluation of the impact in recipients' health and education. Cortes, *et al,* (2007) found that Oportunidades cash transfers contributed to reduce alimentary income poverty headcount by roughly 1% in absolute terms (approximately 7.7% relative) from 2000 to 2005 and, to a lesser extent, the income poverty gap. This impact is considerably higher for rural areas (approximately 2.8% of the absolute headcount) than for urban ones (approximately 0.18% of the absolute headcount).

In previous chapters I engaged with the contentious meaning of income poverty and eligibility have for the programme. Although I have focused on eligibility as a definition of poverty, in this section I will explore the impact of reduction income poverty levels. This focus will also contribute to understanding the extent to which targeting efficiency contributes to the aims of the programme. Additionally, it will also contribute to understanding differences and convergences of both income and a proxy-mean-test definition of poverty.

As discussed in section 2.5, the Mexican government monitored income poverty from 2000 to 2008 based on the same food basket –adjusted for price changes and economies of scale– and using three indicators. These are the headcount, poverty gap and squared poverty gap proposed by Foster, Geer and Thorbecke in 1984. Its properties have been widely discussed, among which the most relevant is that the squared poverty gap is sensitive to the distribution of income among the poor.

Some assumptions made in the analysis below are that the cash transfer does not represent a significant component of household income and hence the lack of substitution of hours of labour among recipients (Attanasio, *et al*, 2008). An additional important assumption is that there are no non-monetary benefits that cannot be obtained without participating in the programme, this is, there are no cost for no participating other than the forgone cash transfer. This assumption is plausible as health and education services received could be accessed even though the household did not receive the cash transfer. Yet, information about eligibility to such services may only come from the programme, and the cash transfer may also serve to pay for side cost of these services, such as transportation, school uniforms or medicines.

Having these assumptions in mind, we now observe the poverty standards in the scenario in which no cash had been transferred to poor families. Table 6.5 gives headcounts figures before and after the cash transfer. In such scenario, the capability poverty headcount would increase from 20% to 21.5%, the poverty gap would increase from 7.5% to 8.9% and the squared poverty gap would increase from 0.008% to 0.01%.

These differences are statistically significant; if we analyse subgroups such as eligible households and households receiving the cash transfer we have the same conclusions. We also observe that the mean transfer to the poor households is higher than to the rest of the population.

TABLE 6.5 IMPACTS ON AGGREGATE CAPABILITY POVERTY MEASURES FOR MEXICO

	After Oportunidades (income including cash transfer)			Before Oportunidades (income minus cash receipts)		
	Est.	Confidence intervals		Est.	Confidence interval	
(a) Households						
Headcount index (percent)	0.2	0.194	0.206	0.215	0.209	0.221
Poverty gap index (percent)	0.075	0.072	0.078	0.089	0.088	0.089
Squared poverty gap index (x100)	0.00008	0.00008	0.00009	0.0001	0.0001	0.0001
Mean transfer to non-poor households	73.460	69.292	77.63			
Mean transfer to income poor households	215.91	203.49	228.33			
Mean income of poor households before Oportunidades	543.76	533.94	553.58			
Mean income of poor households after Oportunidades	595.42	586.70	604.14			
(b) Households with Oportunidades cash transfer						
Headcount index (percent)	0.441	0.423	0.459	0.009	0.517	0.553
Poverty gap index (percent)	0.176	0.166	0.186	0.007	0.252	0.277
Squared poverty gap index (x100)	0.00022	0.000207	0.000234	0.0000089	0.0003	0.0003

Source: Own calculations based on ENIGH 2008 and Oportunidades (2010)

Yet, we also see that the mean income of poor households after the transfer is 595.42 pesos, which is still far from both the urban and rural capability poverty lines. Households without Oportunidades would need a mean transfer of 393 pesos to reach the capability poverty line. Oportunidades only provides a third of this gap (31%)[88] . The programme wouldn't be enough to fight poverty, and this poses a question on targeting efficiency. Is the lack of resources determining targeting efficiency although the programme in fact assists the poorest households? In table 6.10 we saw Oportunidades had impact in reducing poverty at national level in 2008, although we should now disaggregate the information into states.

[88] The total gap to lift households to the capability poverty line is 10 thousand million pesos and Oportunidades cash transfers amount to 3.1 thousand million pesos.

Table 6.6 presents the correlation coefficients of measures of impact on poverty, exclusion and inclusion errors and targeting differentials, as well as the mean cash transfer per capita[89].

TABLE 6.6 CORRELATION COEFFICIENTS OF POVERTY IMPACT AND TARGETING MEASURES

	Impact on capability poverty headcount	Impact on capability poverty gap	Impact on capability squared poverty gap	Exclusion error by state	Inclusion error by state	Targeting differential by state	Capability poverty headcount	Capability poverty gap	Amount of cash transfer per capita in the state	Proportion of cash received by poor households	Children-school attendance behavioural change by state	Teenager-school attendance behavioural change by state	Health behavioural change	Mean value of cost of participation	% change child school attendance	% teenage school attendance	% enrolment in IMSS
Impact on capability poverty headcount	1.00																
Impact on capability poverty gap	0.88	1.00															
Impact on capability squared poverty gap	0.87	1.00	1.00														
Exclusion error at state level	-0.68	-0.61	-0.61	1.00													
Inclusion error at state level	0.86	0.87	0.86	-0.66	1.00												
Targeting differential at state level	0.49	0.40	0.40	-0.96	0.41	1.00											
Capability poverty headcount	0.81	0.92	0.91	-0.49	0.85	0.26	1.00										
Capability poverty gap	0.78	0.93	0.92	-0.47	0.85	0.24	0.99	1.00									
Amount of cash transfer per capita in the state	0.72	0.75	0.75	-0.41	0.59	0.27	0.67	0.66	1.00								
Proportion of cash received by poor households	0.51	0.68	0.68	-0.26	0.43	0.15	0.67	0.66	0.50	1.00							
Children-school attendance behavioural change by state	0.55	0.56	0.55	-0.45	0.49	0.35	0.55	0.54	0.48	0.40	1.00						
Teenager-school attendance behavioural change by state	0.66	0.67	0.68	-0.53	0.48	0.46	0.57	0.57	0.45	0.53	0.62	1.00					
Health behavioural change	0.28	0.41	0.42	-0.40	0.25	0.39	0.30	0.34	0.52	0.29	0.50	0.61	1.00				
Mean value of cost of participation	0.92	0.97	0.96	-0.65	0.90	0.43	0.89	0.88	0.72	0.61	0.59	0.68	0.37	1.00			
% child school attendance 2000-2010	0.58	0.74	0.73	-0.41	0.68	0.23	0.68	0.71	0.63	0.38	0.55	0.56	0.50	0.71	1.00		
% teenage school attendance 2000-2010	0.44	0.51	0.51	-0.46	0.60	0.33	0.56	0.55	0.47	0.34	0.61	0.42	0.46	0.49	0.67	1.00	
% enrolment in IMSS 2000-2010	0.26	0.30	0.31	-0.32	0.29	0.27	0.14	0.19	0.32	0.08	0.13	0.26	0.29	0.34	0.39	0.23	1.00

N=32 all coefficients are statistically significant at 5%
Source: own calculations based on ENIGH 2008, Oportunidades (2010) and ENIGH (2014)

Table 6.7 presents a series of linear (OLS) regressions of the impact of the programme on targeting measures, cash-transfers, costs of participation and measures of behavioural change. Regression on poverty gap and squared poverty gap impacts follow very similar conclusion, so the former is reported and the latter has been omitted. Firstly, there is negative relationship between impact measures (both headcount and poverty gap) and

[89] In an additional series of regressions I included the Concentration index as an independent variable, but it has no explanatory power if included in any of the specification of table 6.7

exclusion errors. This means that states that exclude most of the eligible households have the lowest impacts on poverty. But it is striking the positive relation of impact and inclusion errors, it means that the states that included more non-poor households in the benefits had higher impact on poverty. Conventionally I would expect the opposite: more accurate selection of eligible households would reduce poverty. Yet, larger inclusion error contributes to attain impact on the capability headcount.

TABLE 6.7 LINEAR REGRESSION OF POVERTY IMPACT ON TARGETING MEASURES

	Impact on the capability headcount index (% points)		Impact on the capability poverty gap index (% points)	
	(1)	(2)	(3)	(4)
Exclusion error at state level	-0.01**	-0.01	-0.003	0.0003
Inclusion error at state level	0.07**	0.06	-0.02	-0.01
Amount in pesos of cash transfer per capita in the state	1.2E-10***	5.3E-11**	7E-11	5.8E-11
Teenager-school attendance more often than neighbours	0.08***		0.02	
Health attendance more often than neighbours	-0.05***		0.003	
Children-school attendance behavioural change	0.001		-0.01	
Mean value of cost of participation	0.002	0.01**	0.02***	0.02***
% teenage school attendance 2000-2010		-0.03		0.01
% enrolment in IMSS 2000-2010		-0.01		-0.01
% child school attendance 2000-2010		-0.13		0.09
Constant	0.01	0.02**	0.01*	0.01**
R2 adjusted	0.9	0.86	0.93	0.93
N	32	32	32	32

Source: own calculations based on ENIGH 2008, Oportunidades (2010) and ENIGH (2014)
*Significance at 10%
**Significance at 5%
***Significance at 1%

In table 6.7 I am controlling for the amount of resources allocated to cash transfer, which are positively and statistical significantly related to impact on headcount but its coefficients are very small (between 1.2E-10 and 5.3E-10).

I included two different forms of assessing behavioural change. In the (1) and (3) models, I include the measures of investment higher than neighbour's investment. Teenage school and health attendances are statistically significant to explain impact on income poverty. Teenage-school behavioural change of eligible and /or recipient households is positive and statistically significant related to impact on headcount and income poverty gap, while health attendance is negatively related to impacts on poverty.

In model (1), cost of participation has no significant association to impact on poverty headcount after controlling for these measures of behavioural change.

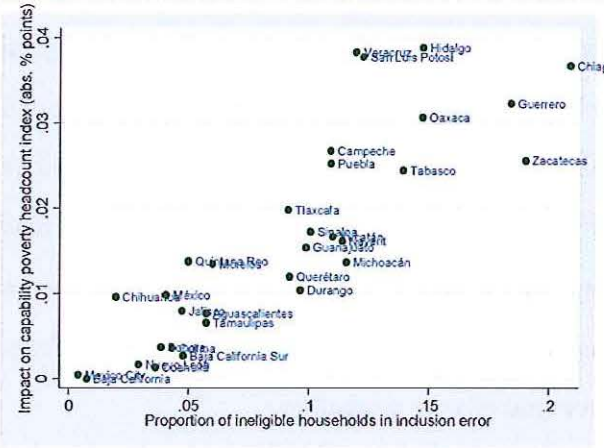
Yet, if instead of neighbours' comparison we include 2000 to 2010 changes in school attendance and health enrolment, behavioural change has no association different from 0 to headcount poverty impact. Errors of targeting are no longer associated to headcount poverty impact, which implies that errors of targeting and neighbours measures of behavioural change are correlated. Yet, costs of participation are now significantly related to poverty headcount impact, as well as the amount of resources allocated to cash transfer. It seems to be that the costs capture some of the effects of errors of targeting and of behavioural change altogether.

Models (3) and (4) understand the relation to impact on the poverty gap. Only the costs of participation are strongly and significantly associated with such impacts. Similar results are obtained for the squared poverty gap although the results are not reported. Although the aim of these regressions is to understand the factors behind impact of the programme, they should be interpreted indicating associations rather than causal relationship. For causal relationship I would have to control for other potential confounders, such as budget constraints, which are potentially important to explain participation in Oportunidades. This is an area of further research.

The following figures will help us to understand this complex relationship. Figures 6.20, 6.21 and 6.22 plot of the relationship between targeting measures and impact of the cash transfer on the capability poverty headcount. In figure 6.20 we see the positive relationship between inclusion errors and impact on poverty headcount and the negative relationship between exclusion errors and impact on poverty is present in figure 6.21. Yet, it is worth noticing that the states which drive the positive relationship in figure 6.22 are less prosperous states. More prosperous states, whether with high or low inclusion errors, experience less impact on poverty due to low initial poverty levels. However, less wealthy states have larger initial poverty levels and larger average impact on poverty. Moreover, in the case of having ineligible households included in

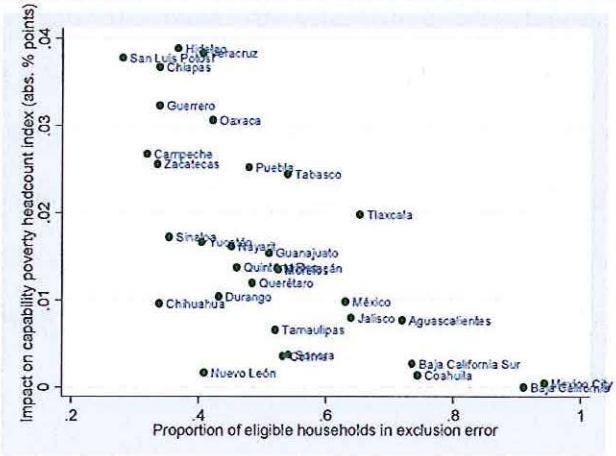
Oportunidades, there are greater chances that those households are poor in the poorest states of Mexico.

FIGURE 6.20 INCLUSION ERRORS AND IMPACT ON CAPABILITY POVERTY HEADCOUNT BY STATE



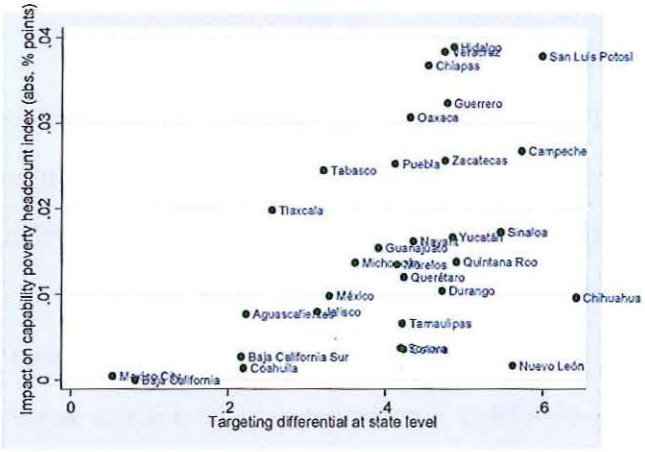
Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

FIGURE 6.21 EXCLUSION ERRORS AND IMPACT ON CAPABILITY POVERTY HEADCOUNT BY STATE



Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

FIGURE 6.22 TARGETING DIFFERENTIAL AND IMPACT ON CAPABILITY POVERTY HEADCOUNT BY STATE



Source: Author's calculation based on ENIGH (2008) and Oportunidades (2010)

Targeting differential is formed by both exclusion and inclusion errors, and we have discussed that exclusion errors are larger than inclusion errors and therefore dominate the targeting differential. In the second regression, controlling for exclusion error, the targeting differential is actually negatively related to the impact on poverty, which means states with a higher targeting differential have lower impact on poverty. But this is due to the fact that they are the poorest states, with the highest impact levels.

In columns (1) and (2) of table 6.7, inclusion errors are positive related to impact on poverty. Targeting differential increases when inclusion error increases, except for better targeted states. For them, targeting differential decreases as inclusion errors increases. It turns out that the states with the better targeting have also lower impact on poverty. After controlling for those with low inclusion error and high targeting differential (the states with best targeting performance), the final relationship between targeting differential and impact on poverty is positive. Again, this is driven by the less prosperous areas. States facing larger poverty index may have to incur in inclusion errors in order to have address its poverty levels. Put it differently, for any level of inclusion error, it is better to have lower exclusion error to alleviate poverty; yet, for any level of exclusion error, it is better to have higher inclusion error (or high initial poverty levels) to alleviate poverty. Exclusion error at state level has always a negative impact

on poverty outcomes, but this relationship is weaker than the inclusion error in the squared poverty gap, that accounts for inequality among the poor.

It is not evident why programme positive coverage of the eligible households does not lead to higher poverty impact. A hint to understand this comes from understanding the cost-effectiveness of the programme. In table 6.12 we see the correlation coefficients of the variables used in table 6.13. We see that amount of cash transfers goes to households below the poverty line is positively correlated with poverty impacts, negatively but weakly correlated to exclusion errors, but it also shows a positive and stronger correlation with inclusion errors. These can be due to the fact that wealthy states have more resources to distribute and have greater inclusion errors, but it would require reducing the exclusion errors in order to have impact on poverty.

An additional mechanism through which states can achieve impact on poverty indexes is increasing teenage participation in school attendance, and this effect is consistent for different indexes. I have reflected before about the uneasy relationship between both policy aims of income poverty reduction and investment in human capital. By looking at teenage school attendance we may conclude that indeed human capital accumulation leads to income poverty reduction. This conventional thinking would justify the existence of conditionality. However, we can also see that health-related behavioural change is negatively associated with impact on poverty, which contradicts the statement above. It is important to remember that teenage-school attendance is also strongly associated with inclusion errors as it has been quite difficult to attract eligible teenagers into school. In the meantime, behavioural change for child-school and preventive health attendance is more frequently achieve among eligible households, but does not have consistent impact on income poverty reduction. This may indicate that policy makers might well be facing a dilemma in between the two policy aims. Not only there is a lack of association between targeting differential and impact on income poverty, but also encouraging conditional action does not necessarily led to income poverty reduction. The evidence presented in this chapter has tried to problematize the mechanism that

links income poverty and human capital investment by highlighting the multiple pathways that relate one to another. Then, targeting efficiency is not a determinant factor in explaining impact of Oportunidades on income poverty.

6.6 Conclusion

In this chapter I have disaggregated the depiction of targeting efficiency at regional level and examined supply and demand-side factors that play a role in understanding targeting efficiency. Income poverty levels and budget allocation explain some regional variations of inclusion errors, but exclusion errors are more responsive to changes in human-capital related behaviour than inclusion errors. I presented the levels of preventive health check-ups and school attendance and behavioural change at state level. The likelihood of inducing behavioural changes, particularly to education-related behaviour is related to exclusion errors. Underinvestment in health and education occurs when poor households tend to be excluded from public services. Underinvestment in human capital is one of the most important factors behind lack of participation in Oportunidades. States with negative incentives for households to send children to school and participate in preventive health are performing worse in targeting although they are allocating resources to the income poorest households. This is also confirmed by the analysis of the costs of participation, which are argued to capture unobserved costs of investment in human capital that go beyond its value in the market. Higher costs for participants make it less plausible to induce behavioural change.

When Oportunidades was designed as a means-test programme, two contentions aims were defined: alleviating income poverty and breaking intergenerational reproduction of poverty by investing in human capital. I have presented an assessment of Oportunidades' impact on income poverty, following the analysis of Ravallion (2009) and Cortes *et al* (2007). High initial levels of poverty and cash transfer allocation, inclusion errors and teenage-school attendance behavioural change are main features that characterize the programme's impact on poverty. Targeting efficiency does not

explain impact on income poverty, which leads to question the pertinence of targeting mechanisms.

The empirical data in chapter 4.3.3 shows that there is a mismatch between the income poverty levels and the eligible population of the programme. It specifies that if the model is outdated, it will fail to identify the true poverty levels. In this chapter I have also highlighted that behavioural change cannot be taken for granted and it has an effect in determining participation in Oportunidades and its impact on income poverty. Ironically, behavioural change is not necessarily related to impact on income poverty. Hence, policy makers may find themselves in the dilemma between improving targeting efficiency and reducing income poverty at the same time. In such dilemma, larger inclusion errors may lead to both objectives, although it is questionable whether this is the best course of action.

This analysis has led us to question the conventional ideas about targeting efficiency. Targeting efficiency does not explain poverty impacts. In fact, Ravallion (2009) warns about them in relation to the *Di Bao*, a means-tested minimum guarantee income in China: 'standard measures of targeting performance are uninformative or even deceptive about the impacts on poverty, and cost-effectiveness in reducing poverty…it would be better to focus directly on the programme's outcomes for poor people than to rely on prevailing measures of targeting' (p. 205). But it is not the case that targeting inefficiency explains poverty reduction. We have discussed that allocation of Oportunidades cash transfer hardly happens on pure eligibility grounds. Budget allocation cannot entirely predict variation of targeting differentials[90] so I looked at human capital investment and behavioural change to understand exclusion errors. After these results, it is worth questioning in which ways a better-informed targeting model could contribute to poverty alleviation. Targeting errors seem to point towards multiple paths through which targeting efficiency can influence impact on poverty. Impact can

[90] The Concentration Index would be more responsive to budget allocation, but it has been discussed in section 6 of Chapter 3 and in section 6.1 above that the targeting differential is better suited for our analysis.

be enhanced through reducing exclusion errors, but it may also be enhanced through increasing inclusion errors in poor areas, what in practical terms means increasing cash transfers into poorer areas.

Oportunidades is managed and implemented following central rules of operations. Before 2009, it followed one single model of selection of target population across the territory[91]. Given the centralized design of the programme, we wouldn't expect differences in outcomes across the territory apart from the ones that define the distribution of poverty across regions. We wouldn't expect much leeway for local officials to allocate the benefits. However there are differences in administrative capacity and the local political economy that shape the performance of the programme and therefore its impact in solving the problem of poverty. Health and education are fully decentralized in Mexico. The decentralization of education started in 1992 with a homogenous process in the whole country. The health system decentralization was a fragmented process that occurred between 1983 and 1997[92]. Further research may explore how these local policy processes affect the supply of public services to Oportunidades recipients. In 2010, Oportunidades changed the targeting model, updating not only the weights of the indicators, but also including newly designed indicators and distinguishing urban and rural selection models. Whether that model is better suited to inform allocation of cash transfers is an empirical question that may be explored in further research

[91] From 2009 it has a different targeting model for urban and rural areas.
[92] Early decentralisation from 1983 to 1988, occurred in 14 states, late decentralisation occurred between 1995 and 1997 in the rest of the country.

7 CONCLUSIONS AND CONSIDERATIONS FOR FUTURE RESEARCH

In attacking poverty, Conditional Cash Transfers have even been recently suggested as a complement for universal and unconditional social policy implemented in Europe (European Commission, 2013). They are thought to be a powerful tool to combat poverty because they alleviate current income constraints by the means of a cash transfer but they also seek to encourage investment in human capital by making the cash transfer conditional on behaviour. In order to be efficient, most of CCTs are targeted with Means Testing Targeting (MTT) so the cash transfers reach the income poorest population. A proxy MTT was introduced in Oportunidades to prevent leakage and secure coverage of the income poorest households. Yet, the programme has not succeeded in reaching the target population and has important targeting errors that include non-eligible households and exclude eligible ones.

In this research I have argued that MTT entails a particular definition of a public problem. Oportunidades' means-test is shaped by the income definition of poverty, but its proxy variables focus on investments in human capital. I also argue that the concept of eligible household, derived from the MTT eligibility score, is a category in its own right that identifies the distribution of social characteristics that determine underinvestment in human capital and, hence, participation in Oportunidades. I also discussed the importance of changing focus to a distribution of Oportunidades conditional on being eligible. I argued that the marginal distribution of cash transfers is not the same as the conditional distribution. I presented the sample selection correction model which considers this conditional distribution in two steps: the first step models the means-test eligibility, and the second step uses that information to estimate the probability of receiving the cash transfer. The assumption behind this strategy is that there is a sample bias in Oportunidades cash receipt induced by the condition of being eligible. The model also has shed light on the targeting errors, a problem that is not central to traditional methods of analysis.

Chapter 3 justified the methodologies used in this research and exemplified its implications. Chapter 4 presented the assessment of participation in Oportunidades in urban areas and its distributional consequences. It also presented an alternative way of measuring behavioural change. I conceptualized behavioural change as a deviation from the average neighbourhood behaviour. This method elicited information about those households that invest in human capital more than the average of their neighbours, suggesting a behavioural change. It concluded that the proxy MTT is not the only factor for explaining cash transfer allocation and targeting efficiency.

In Chapter 5 I have also presented an analysis of the unobserved costs of participation and how they contribute to explaining lack of participation in urban Oportunidades. The main implication of the analysis of the unobserved costs of participation is that eligible households who do not take-up the programme also underinvest in human capital and therefore face higher costs of welfare participation. These costs of participation at state level are then reconsidered in chapter 6. I discussed how those cost of participation are related to the extent to which behavioural change among participants of the programme can be achieved. I also presented evidence that links this relation to the existence of exclusion errors, implying that cost of participation may deter eligible households. Moreover, these costs of participation are not related to the opportunity cost of forgone income of children and teenager, but they are closely associated to the regional penalisation scores of the means-test model. Chapter 6 also discussed two different ways of assessing targeting efficiency. The targeting differential explicitly considers the classification of recipients by eligibility and it is preferred over the concentration index.

According to Heckman and Vytlacil (2007a), there are two different ways of approaching the question of whether public policy has had the desired policy effect: the treatment effect methodologies and the econometric, 'structural' or 'economically motivated' methods. On the one hand, Treatment Effects analysis does not impose a functional form on the phenomenon that is being analysed, in this case the effect of the public policy. On the other hand, Deaton (2009) advocates use of structural models with

a well-defined functional form that is grounded on hypothesis to be tested. There is no ultimate valid method for approaching policy effects. As Heckman and Vytlacil (2007a) recognize, practical difficulties in specifying structural models have led to a more practical empirical approach to research questions that are narrow but valid. The internal validity achieved by sound statistical analysis may come at the cost of external applicability of its conclusions (Banerjee and Duflo, 2009).

On the other hand, alternative methodologies have their own limitations. Nevertheless, (Heckman and Vytlacil, 2007a: 4835) argue that treatment effects methods offer limited prospective analysis. By making arguably strong assumptions on the distribution of outcomes conditional on eligibility (which is assumed to be normally distributed) I have also drawn conclusion on behavioural outcomes that led to the policy outcomes we observe. This particular model that fits the participation in Oportunidades in 2008 might not be applicable to other contexts. However, it has allowed me to better understand the problem of take-up.

The model has its limitations. The ENIGH (2008) is accurate in providing information on income, health and education to a national scale, but it consists in only one observation in time. Secondly, I cannot confirm whether there is self-selection of the sample of eligible households on statistical grounds as I do not observe correlation between the selection and the final equation[92]. Still, I argue that there are still grounds to acknowledge that both steps are not independent. Lack of correlation also indicates that eligible households are not necessarily more likely to receive the cash transfer. Non-eligible households are as likely as eligible ones to get the cash transfer.

This would suggest that the targeting model and the receipt of cash transfer are not related. On the one hand, the poorest participants are less likely to invest in human capital, especially in education of all their children and teenagers. On the other hand,

[92] This is captured by the coefficient p (rho), which states the correlation between error terms of the selection and the regression equations. By rejecting the null hypothesis, we can argue that there is correlation between selection and final equation, and confirm the presence of selection bias (Cuddeback, *et al*, 2004).

officials may be targeting those households who would comply with the conditionality more frequently, which are not precisely the poorest ones.

In the presence of perfect targeting, no other variable should influence take up, and benefits would be assigned on variation of individual characteristics. However, we have shown that being eligible and receiving the cash transfer are not necessarily correlated[93]. Moreover, at least at primary school level, we have seen that almost all eligible households with children send them to school regardless of the expected benefit. This means that many households that are eligible and comply with conditionality end up not claiming the benefits. Yet, this cannot be concluded for secondary school level, since households with teenagers in neighbourhoods where most teenagers attend school are the ones more likely to get the cash transfer.

CCTs have policy objectives other than income poverty reduction and it is an empirical question if the policy objectives are congruent or conflicting with each other (Atkinson, 1995). Fine-tuning objectives and policy instruments has proven to be difficult for CCTs that use MTT, since they not only target income poverty but also behaviour. Oportunidades was designed to achieve two different policy objectives: alleviating income poverty and breaking intergenerational reproduction of poverty by incentivising investment in human capital. It was assumed that investment in human capital could be incentivised via cash transfer. In this research I have shown that Oportunidades does incentivize such an investment among recipients, but it also excludes eligible households (many of which are also income poor) that have the lowest levels of investment in human capital. An interesting question that remains unanswered is whether people who were not investing in human capital could have been investing marginally more than recipients before the receipt of the benefit. In any case, the lack of coherence among policy objectives and targeting instruments may limit the efficiency of MTT.

This is a source of inefficiency that some argue could be alleviated by increasing the cash transfer (Attanasio, *et al*, 2008; De Janvry and Sadoulet, 2006). However, evidence presented in this thesis has questioned the extent to which this could happen in the presence of the unobserved cost of participation that is not related to the relative price of children's and teenagers' time designated to income earning activities.

This research started with a very narrow objective of answering three sets of research questions for the case of Oportunidades in2008. Taking as a starting point the research questions proposed in section 2.6, I can conclude that means-testing is not the only factor behind participation in urban Oportunidades as participation in urban Oportunidades is also associated with the ability to comply with the conditions of the programme. Monetary barriers to access to public health assistance in urban areas has also a stake in explaining lack of take-up in urban areas, but they are not relevant throughout the territory. Participation in Oportunidades generates costs for participants and non-participants. The higher costs are for those who do not attend health check-ups, regardless whether they are eligible or not. Yet, high costs also come from school-related conditionality. The costs of participation for eligible non-recipients get closer to 0 if they do not comply with the conditionality of human capital. That is, eligible households who do not claim the benefit and invest in human capital face lower (negative) costs if they also do not claim the benefit. This finding may point towards the need to explore a particular distaste for welfare benefits among eligible non-recipients.

The costs of participation in Oportunidades at an aggregate level are associated with lower levels of behavioural change at state level. Therefore, the costs of participation were useful to understanding the aggregate variation of targeting efficiency at local level. Yet, lack of efficiency does not contribute to explaining the impact of Oportunidades in reducing income poverty at local level.

This research has led me to enquire into the definition of poverty used by Oportunidades and what good targeting is. MTT is often thought of as a tool for selecting recipients and sharply distinguishing a target population. However, when

implemented, proxy MTT turns out to be a fuzzy selection criterion that is associated to income poverty but also to underinvestment in human capital. In order to succeed, the programme should provide cash transfers to income poorest households and also induce behavioural change through conditionality compliance. But it turns out that households whose children and teenagers would have gone to school in any case may receive the cash transfer. And some eligible households are not taking up the programme. Both of the last problems reflect an inefficient implementation of the programme (De Janvry and Sadoulet, 2006). Given these results, one may question whether the eligibility should move from focusing on the income poorest household to focusing on the household who could make the most of this investment. It is also worth questioning in which ways a better-informed targeting model could contribute to poverty alleviation.

The budget constraint of the programme is a factor behind the exclusion errors throughout Mexico. Yet, some states are better at allocating the insufficient fund received than others, and higher budget allocation is associated with inclusion errors. And budget allocation does not fully explain exclusion errors and to understand them it was useful to look at the extent to which human-capital-related behavioural change occurs at local level. Areas with higher levels of behavioural change have fewer exclusion errors. And areas with higher levels of behavioural change are associated to low levels of costs of participation. But being this an aggregate level analysis, the conclusions should be related to areas, rather than individuals. Then, there are several pathways in which costs of participation can lead to behavioural change and from there to better targeting. One of the pathways is argued to be the cash transfer. I have argued that unobserved costs that have less to do with the labour market may be central to understand participation in Oportunidades. The unobserved costs of participation, which are negatively associated to behavioural change, may be preventing eligible households from claiming the cash transfer.

This analysis has also led me to question the conventional knowledge about targeting efficiency. Teenage-school-attendance behavioural change has a stake at explaining the impact of Oportunidades on reducing income poverty. But targeting efficiency does not

always imply effectiveness against poverty. In fact, inclusion errors are better at explaining impact of Oportunidades on income poverty indexes. Policy makers may have no incentives to improve targeting efficiency if they can reduce income poverty by increasing inclusion errors. In such dilemma, larger inclusion errors may lead to both objectives, although it is questionable whether this is the best course of action. It is not the case that targeting inefficiency explains poverty reduction. Targeting errors seem to point towards multiple paths through which targeting efficiency can influence impact on poverty. Impact can be enhanced through reducing exclusion errors, but impact may also increase as inclusion errors of income poor people increase, what in practical terms means increasing cash transfers to the non-eligible income poor.

Income poverty levels and budget allocation explain some regional variations of inclusion errors and impact of Oportunidades on income poverty. At a state aggregate level, high proportion of eligible households excluded is associated with low proportion of changes in human-capital-related behaviour. Hence, it is worth asking what has led to such outcomes at an aggregate level. Underinvestment in human capital prevents participation in Oportunidades. If negative incentives are in place for households to send children to school and participate in preventive health care, it is more likely that such areas perform worse in targeting. This is also confirmed by the analysis of the costs of participation, in which higher costs for participants are associated to a lower proportion of participants and eligible no-recipients that change their behaviour towards human capital investment.

Some research suggest that conditionality does not play a role in increasing attendance at primary education where high enrolment rates are already observed and that it is secondary education where behavioural change matters the most (De Janvry and Sadoulet, 2006; Attanasio, *et al,* 2008). In chapter 4 I argued that, at individual level, health-related behavioural change the one that is widely observed across recipients and eligible households and where Oportunidades may have had its greatest impact. I also observed important behavioural change in primary-school attendance despite the near-to 100% enrolment rates. In chapter 5 I argued that the costs of participation had a role to play in explaining the lack of take-up. Alternative research also could also explore the

lack of social mobility of Oportunidades recipients (Yaschine, 2012) as a factor behind the costs of participation.

In chapter 6 I also present evidence of the importance of teenage-school attendance behavioural change at an aggregate state level. High proportion of teenager's behavioural change at an aggregate level is associated with low proportion of exclusion errors in the state. The mechanism of such association should be thoroughly explored. Although participation of teenagers in the labour market has been the main mechanism explored to explain this relationship, I have concentrated in highlighting the importance of unobserved cost of participation.

To accomplish this research I have used concepts that have substantive meaning in social terms. The errors of targeting are not only mistakes or misclassification but represent people who would have the right to claim benefits and do not do it; or people who are not eligible but are presented with the benefit and decide to take it up. Put it differently, behind the aim of explaining exclusion errors is the aim to understand the extent to which eligible households decide to opt out of the programme and the extent to which they suffer deliberate exclusion. The statistical tools that have been used in this research highlight the different pathways or mechanism of exclusion, which involve a degree of both individual decision making and structural barriers to participate in public programmes.

I have also used concepts that are familiar in the Anglo-Saxon context. Research concerning welfare programmes in the US and UK is inclined to enquire about the phenomenon of eligible non-recipients and label it as non-take-up. In an European context, where most welfare benefits are assigned on the grounds of corporations or citizenship, would probably be less familiar with the concept of non-take-up. Research concerning developing countries (Cornia and Stewart, 1995) and specifically about Mexico (Skoufias and Di Maro, 2006) has emphasized that the phenomenon of eligible non-recipients is due to errors of exclusion or undercoverage from the administration of the programme. This difference is not trivial. The action of taking-up welfare benefits places responsibility on the individual. Meanwhile, the existence of errors of exclusion or undercoverage of eligible claimants places the responsibility on the administrators of

the programme. This portrays two different explanations for the same phenomenon. This research has searched to inform both perspectives. Hence, the use of the concept of 'take-up' takes for granted the nature of the phenomena in developed contexts that are not entirely replicable in developing ones.

Further areas of research that are closely related to the conclusion of this research are related to the definition of poverty and the accountability of anti-poverty programmes. Firstly, the relationship between the proxy variables and the income poverty line used in the means-test eroded over time (Azevedo and Robles, 2010) and this problem was addressed by changing the proxy MTT in 2009. It is an empirical question whether the problem of exclusion of the objective population continues after such changes. This research has focused in the 2008 proxy MTT model but its analytic tools could be used to assess the targeting efficiency of the new model.

Secondly, other statistical models could consider, for instance, the extent to which self-selection is induced on the grounds of income poverty, migration status or conditionality compliance, which are also powerful determinants of participation in Oportunidades. Instead, the model in this research has an explicit focus on eligibility criteria. A more thorough consideration would include such factors in the self-selection process.

Thirdly, some research may be interested in exploring how the legal framework is implemented in reality. As we have seen, the rules of Oportunidades are not thoroughly implemented, and policy makers may have reasons to implement the programme differently from what the rules say. Moreover, antipoverty programmes may also serve aims beyond the ones they have been created to achieve. This is a matter of public accountability that has not been explored above but that could be addressed in further research. Fourthly, the decentralisation of health and education and specific features of the public administration may be determining the budget constraints that were explored in section 6.3. Those features were beyond the scope of the information available for this research, but they deserve to be explored. Fifthly, the model presented here is adequate to depict the particular reality of 2008 urban Mexico. Finding ways of eliciting

the factors behind participation in Oportunidades at local level is a potential area of research that shouldn't be neglected.

Last, but not least, this research has led me to reflect on different perspectives of the problem of non-take-up or lack of participation in welfare. This research argues that conditionality of the cash transfer prevent human-capital-related behavioural change among eligible non-recipients of Oportunidades. Some households decide to take-up the benefit while other eligible households decide not to participate complying with the conditions. Given a structure of benefits and conditions, individuals make decisions that shape their participation in the policy process (Van Oorschot, 1995). This research could be considered as contribution to the discussion in the broader space of deprivation and disempowerment. If reducing budget deficit is the core objective of policy makers, it may well be that lack of take-up coincides with such policy objective, fewer cash transfers allocated may contribute to save public resources. Then it may be convenient to label certain households as undeserving recipients of welfare in order to deter their participation in welfare programmes. This may also erode dignity and self-esteem. In such scenario, the costs of non-take-up should consider stigmatisation of welfare recipients and the extent to which those costs are unevenly distributed and to affect the underprivileged. To enhance wellbeing, it is necessary to explore the mechanisms that cause non-take up, as well as the ways in which the social consequences of lack of participation may be reduced. Moreover, a better understanding of the causes and consequences of non-take-up are informative regarding the agency, expectations, and attitudes in which an entitled person decides to claim benefits. These are seldom accounted for in the design and implementation of antipoverty policies.

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9 APPENDICES

TABLE A.1 OLS LINEAR REGRESSION OF CASH RECEIPT CONDITIONAL ON ELIGIBILITY

	Dependent variable is receipt of cash transfer (0-1)					
	Coefficients					
Independent variable	National		Rural		Urban	
Predicted probability of cash receipt in neighbourhood	0.73	***	0.61	***	0.81	***
Residuals of predicted prob. of cash receipt in neighbourhood	-0.95	***	-0.92	***	-0.95	***
Mean inverse mills ratio in neighbourhood						
Inverse mills ratio for households	0.00		-0.02	***	0.02	***
Eligibility first-step						
Number of people per room	0.02	***	0.02	***	0.02	***
Demographic dependency	0.02	***	0.00		0.03	***
Female head of household	0.01	*	0.01		0.00	
Households with children aged 0-11	-0.01	***	-0.02	***	0.01	
Age of head of household	0.00	***	0.00	***	0.00	***
No social security benefits	0.04	***	0.04	***	0.05	***
No education of head of household	0.08	***	0.09	***	0.09	***
Incomplete elementary education	0.05	**	0.06	***	0.05	***
Shared or no toilet	0.06	**	0.09	***	0.06	***
Toilet without running water	0.06	**	0.09	***	0.05	***
Dwelling with dirt floor	0.03	***	-0.01		0.08	***
Household without gas or electrical stove	0.03	**	0.01		0.06	***
Household without refrigerator	0.02	***	-0.02	*	0.04	***
Household without washing machine	0.01	**	0.01		0.01	***
Household without car or truck	0.03	***	0.05	***	0.03	***
Rural effect	0.12	***	(dropped)		(dropped)	
Constant	-0.35	***	-0.21	***	-0.32	***
Endogenous effects (conditionality)						
Teenagers attend school	0.05	***	0.04	**	0.05	***
Teenagers in neighbourhood attend school	-0.02	***	-0.06	***	-0.02	***
School-age children attend school	0.03	***	0.02		0.03	***
School-age children in neighbourhood attend school	-0.02	***	-0.04	**	-0.01	***
Had a preventive health check-up in the last 12 months	0.09	***	0.24	***	0.05	***
Proportion of neighbors who had preventive health check-up	-0.10	***	-0.21	***	-0.05	***
Indigenous language spoken in household	0.02	***	0.04	***	0.01	*
Natural logarithm of the value of entitlement	0.02	***	0.03	***	0.00	***
Natural logarithm of income brought by teenagers into the household	0.00	***	0.00		0.00	***
Observations	64200		16030		48110	
Dummies for state level effects	Yes		Yes		Yes	
R-squared	0.54		0.40		0.38	

Source: Own calculations based on ENIGH (2008)

*** significance at 0.001 level

** significance at 0.05 level

* significance at 0.1 level

TABLE A.2 OLS LINEAR REGRESSION OF CASH RECEIPT ONLY FOR ELIGIBLE HOUSEHOLDS

	Dependent variable is receipt of cash transfer (0-1)					
	Coefficients					
Independent variable	National		Rural		Urban	
Predicted probability of cash receipt in neighbourhood	0.68	***	0.64	***	0.78	***
Residuals of predicted prob. of cash receipt in neighbourhood	-1.01	***	-0.91	***	-1.19	***
Mean inverse mills ratio in neighbourhood						
Inverse mills ratio for households	-0.02		-0.02		-0.04	
Eligibility first-step						
Number of people per room	0.01	***	0.01	***	0.01	**
Demographic dependency	0.01		0.00		0.02	
Female head of household	0.00		0.02		-0.02	
Households with children aged 0-11	-0.02	***	-0.02	**	-0.02	***
Age of head of household	0.00	***	0.00	***	0.00	***
No social security benefits	0.02		0.03	*	0.00	
No education of head of household	0.08	***	0.08	***	0.08	***
Incomplete elementary education	0.06	***	0.06	***	0.05	**
Shared or no toilet	0.09	***	0.11	***	0.06	**
Toilet without running water	0.08	***	0.11	***	0.04	*
Dwelling with dirt floor	0.00		-0.02	*	0.05	**
Household without gas or electrical stove	0.00		0.00		0.02	
Household without refrigerator	-0.01		-0.01		-0.01	
Household without washing machine	0.01		0.00		0.02	
Household without car or truck	0.03	**	0.02		0.07	***
Rural effect	0.09	***	(dropped)		(dropped)	
Cash receipt second-step	-0.19	**	-0.40	***	-0.31	**
Endogenous effects (conditionality)						
Teenagers attend school	0.11	***	0.06	***	0.17	***
Teenagers in neighbourhood attend school	-0.03	*	-0.03		-0.03	
School-age children attend school	0.07	***	0.04	**	0.10	***
School-age children in neighbourhood attend school	-0.11	***	-0.08		-0.14	***
Had a preventive health check-up in the last 12 months	0.25	***	0.28	***	0.20	***
Proportion of neighbors who had preventive health check-up	-0.22	***	-0.24	***	-0.12	
Indigenous language spoken in household	0.04	***	0.05	***	0.04	*
Natural logarithm of the value of entitlement	0.03	***	0.03	***	0.02	***
Natural logarithm of income brought by teenagers into the household	0.00		0.00		0.00	
Sample size	12671		8741		3930	
Dummies for state level effects	Yes		Yes		Yes	
R-squared	0.41		0.33		0.41	

Source: Own calculations based on ENIGH (2008)

*** significance at 0.001 level

** significance at 0.05 level

* significance at 0.1 level

TABLE A.3 PROBIT REGRESSION OF CASH RECEIPT CONDITIONAL ON ELIGIBILITY AND OTHER COVARIATES

	Dependent variable is receipt of cash transfer (0-1)					
	Marginal effects dy/dx					
Independent variable	National		Rural		Urban	
Predicted probability of cash receipt in neighbourhood	0.20	***	0.86	***	0.06	***
Residuals of predicted prob. of cash receipt in neighbourhood	-0.34	***	-1.33	***	-0.10	***
Mean inverse mills ratio in neighbourhood						
Inverse mills ratio for households	-0.01	***	-0.04	***	0.00	***
Eligibility first-step						
Number of people per room	0.01	***	0.03	***	0.00	***
Demographic dependency	0.00		-0.01		0.00	**
Female head of household	0.00		0.02		0.00	
Households with children aged 0-11	-0.01	***	-0.03	***	0.00	**
Age of head of household	0.00	***	0.00	***	0.00	***
No social security benefits	0.01	**	0.06	**	0.00	*
No education of head of household	0.04	***	0.13	***	0.01	***
Incomplete elementary education	0.03	***	0.10	***	0.01	***
Shared or no toilet	0.03	***	0.12	***	0.01	***
Toilet without running water	0.02	***	0.12	***	0.01	***
Dwelling with dirt floor	0.00		-0.02		0.00	**
Household without gas or electrical stove	0.00		0.00		0.00	
Household without refrigerator	0.00		-0.03	*	0.00	
Household without washing machine	0.00		0.00		0.00	*
Household without car or truck	0.02	***	0.08	***	0.01	***
Rural effect	0.04	***				
Cash receipt second-step						
Endogenous effects (conditionality)						
Teenagers attend school	0.04	***	0.08	**	0.01	***
Teenagers in neighbourhood attend school	-0.02	***	-0.08	***	0.00	***
School-age children attend school	0.02	***	0.03		0.01	***
School-age children in neighbourhood attend school	0.00		-0.06		0.00	
Had a preventive health check-up in the last 12 months	0.04	***	0.34	***	0.01	***
Proportion of neighbors who had preventive health check-up	-0.07	***	-0.32	***	-0.02	***
Indigenous language spoken in household	0.01	**	0.06	***	0.00	
Natural logarithm of the value of entitlement	0.01	***	0.05	***	0.00	***
Natural logarithm of income brought by teenagers into the household	0.00	***	0.00		0.00	***
Sample size	64140		16030		48110	
Dummies for state level effects	Yes		Yes		Yes	
Mean y predicted	0.04		0.53		0.01	

Source: Own calculations based on ENIGH (2008)

*** significance at 0.001 level

** significance at 0.05 level

* significance at 0.1 level

TABLE B.1 DATA ON TARGETING EFFICIENCY AND IMPACT ON INCOME POVERTY AT STATE LEVEL

Average penalisation score	State	Targeting differential (TD)	Exclusion error	Inclusion error	TD urban areas	TD rural areas	Proportion of households that receive Oportunidades cash transfer (A)	Proportion of household under the capability poverty line (B)	Significant difference between A-B	Proportion of eligible households ©	Significant difference between B-C	Mean household per capita income	Region
-0.52	Chihuahua	0.64	0.34	0.02	0.07	0.58	0.09	0.14	-0.05	0.10		4753.1	Northern region
-0.37	San Luis Potosí	0.60	0.28	0.12	0.38	0.32	0.30	0.26		0.30		2688.8	Tamaulipas,Gorda mountains,Semi-arid regions
-0.29	Campeche	0.57	0.32	0.11	0.41	0.27	0.28	0.22		0.30	-0.08	3529.6	Mayan and Tabasco region
-0.51	Nuevo León	0.56	0.41	0.03	0.34	0.36	0.05	0.09	-0.04	0.04	0.05	4905.1	Tamaulipas region
-0.36	Sinaloa	0.55	0.35	0.10	0.34	0.37	0.18	0.12	0.06	0.14		3937.4	Sinaloan, Nayaritan regions
-0.29	Quintana Roo	0.49	0.46	0.05	0.32	0.35	0.13	0.16		0.17		4446.9	Mayan and Tabasco region
-0.39	Hidalgo	0.49	0.37	0.15	0.41	0.32	0.32	0.28		0.36		2313.5	Gorda mountains and Mexico City regions
-0.29	Yucatán	0.49	0.40	0.11	0.39	0.29	0.26	0.18	0.08	0.30	-0.12	3556.2	Mayan and Tabasco region
-0.42	Guerrero	0.48	0.34	0.18	0.34	0.35	0.39	0.41		0.43		2008.4	South Pacific Coast,Balsas river, Mixteca Huasteca regions
-0.35	Zacatecas	0.48	0.33	0.19	0.34	0.24	0.29	0.27		0.21	0.07	2570.1	Semi-arid region
-0.24	Veracruz	0.47	0.41	0.12	0.37	0.35	0.29	0.25		0.37	-0.12	2634.3	Gorda mountains, Gulf region
-0.53	Durango	0.47	0.43	0.10	0.20	0.44	0.17	0.25	-0.08	0.16	0.10	2744.2	Northern, Nayaritan regions
-0.02	Chiapas	0.45	0.34	0.21	0.32	0.17	0.48	0.50		0.60	-0.10	1776.2	Central Chiapas and Mixteca Huasteca regions
-0.66	Nayarit	0.44	0.45	0.11	0.16	0.32	0.17	0.18		0.13		3110.7	Nayaritan region
-0.16	Oaxaca	0.43	0.42	0.15	0.20	0.34	0.39	0.35		0.55	-0.20	2054.8	South Pacific Coast,Gulf, Mixteca Huasteca
-0.66	Colima	0.42	0.53	0.04	0.30	0.50	0.07	0.10	-0.04	0.05	0.05	4372.9	South Pacific Coast,Balsas river, Mixteca Huasteca regions
-0.46	Querétaro	0.42	0.48	0.09	0.27	0.13	0.16	0.13		0.17		4102.4	Gorda, Bajio, Balsas river
-0.51	Tamaulipas	0.42	0.52	0.06	0.20	0.25	0.09	0.16	-0.07	0.07	0.09	3502.3	Tamaulipas region
-0.47	Sonora	0.42	0.54	0.04	0.24	0.32	0.07	0.10	-0.03	0.07	0.03	4813.7	Northern, Sinaloan region
-0.30	Morelos	0.41	0.53	0.06	0.34	0.31	0.14	0.22	-0.08	0.20		2960.8	Balsas river, Gulf, Mixteca Huasteca
-0.19	Puebla	0.41	0.48	0.11	0.25	0.17	0.27	0.34		0.39		2132.4	Gorda, Gulf
-0.50	Guanajuato	0.39	0.51	0.10	0.25	0.25	0.17	0.16		0.18		2912.0	Semi-arid, Gorda, Bajio, Balsas
-0.44	Michoacán	0.36	0.52	0.12	0.28	0.14	0.20	0.24		0.24		2630.7	Bajio, South Pacific, Balsas river
-0.38	México	0.33	0.63	0.04	0.15	0.33	0.09	0.15	-0.06	0.16		3032.3	Balsas river, Mexico city, Gulf
-0.29	Tabasco	0.32	0.54	0.14	0.10	0.21	0.24	0.30		0.30		2574.7	Mayan and Tabasco region
-0.50	Jalisco	0.31	0.64	0.05	0.05	0.34	0.07	0.13	-0.06	0.07	0.05	3883.1	Semi-arid,Bajio,South Pacific

-0.39	Tlaxcala	0.26	0.65	0.09	0.14	0.25	0.15	0.30	-0.15	0.23	0.07	2052.5	Gorda mountains and Huasteca
-0.35	Aguascalientes	0.22	0.72	0.06	0.13	0.19	0.08	0.17	-0.09	0.09	0.08	3589.3	Semi-arid region
-0.51	Coahuila	0.22	0.74	0.04	0.06	0.20	0.04	0.16	-0.12	0.04	0.13	3537.3	Northern, Tamaulipas
-0.52	Baja California Sur	0.22	0.74	0.05	0.24	0.12	0.06	0.10		0.06		5141.3	Northern region
-0.52	Baja California	0.08	0.91	0.01	0.09	0.04	0.01	0.09	-0.08	0.03	0.06	4446.8	Northern region
-0.41	Mexico City	0.05	0.94	0.00	0.05	0.02	0.01	0.08	-0.07	0.04	0.04	5837.3	Mexico City

Source: Author's calculations based on ENIGH (2008) and Oportunidades (2010)

TABLE C.1: DESCRIPTIVE STATISTICS OF THE DEPENDENT AND INDEPENDENT VARIABLES OF THE MODEL, WHOLE SAMPLE

Variables	Minimum value	Mean	Median	Maximum value	Variance	Skewness
Dependent variable						
Oportunidades cash receipt (dichotomic)	0	0.17	0	1	0.14	1.77
Natural logarithm of Oportunidades cash received	0	1.05	0	9.13	5.47	1.82
Instrumental variables						
Predicted probability of cash receipt in neighbourhood	0	0.17	0	1	0.07	1.47
Residuals of the predicted probability of cash receipt at neighbourhood level	-0.78	-0.1	-0.1	0.73	0.03	0.27
Mean inverse mills ratio in neighbourhood (only included in the first step)	0	6.38	7.68	12.2	10.93	-0.67
Inverse mills ratio at household level	0	5.08	5.49	12.2	12.29	-0.1
Eligibility first-step						
Number of people per room	0.04	1.87	1.33	16	2.13	1.87
Demographic dependency	0	0.73	0.5	10	0.52	1.81
Female head of household	0	0.24	0	1	0.18	1.2
Households with children aged 0-11	0	0.96	1	10	1.35	1.31
Age of head of household	12	47.72	46	97	238.31	0.48
No social security benefits	0	0.48	0	1	0.25	0.07
No education of head of household	0	0.09	0	1	0.08	2.79
Incomplete elementary education	0	0.21	0	1	0.17	1.41
Shared or no toilet	0	0.13	0	1	0.11	2.25
Toilet without running water	0	0.27	0	1	0.19	1.07
Dwelling with dirt floor	0	0.07	0	1	0.06	3.53
Household without gas or electrical stove	0	0.11	0	1	0.1	2.42
Household without refrigerator	0	0.18	0	1	0.15	1.66
Household without washing machine	0	0.49	0	1	0.25	0.06
Household without car or truck	0	0.58	1	1	0.24	-0.32
Cash receipt second-step						
Usage of health services						
Household member attends public health centres	0	0.85	1	1	0.13	-1.94
Health insurance (Seguro Popular)	0	0.23	0	1	0.18	1.25
Interaction of health insurance and entitlement	0	1.3	0	7.59	7.39	1.62
Indigenous language spoken in household	0	0.1	0	1	0.09	2.68
Households who did not go to the doctor in the last 12 month because it was expensive	0	0.02	0	1	0.02	6.34
Household who did not go to the doctor in the last 12 months because of mistreatment / mistrust / did not speak the same language	0	0.01	0	1	0.01	13.47
Endogenous effects (conditionality)						
Teenagers attend school	0	0.75	1	1	0.16	-1.12
Teenagers in neighbourhood attend school	0	0.75	0.81	1	0.07	-0.99
School-age children attend school	0	0.9	1	1	0.07	-2.59
School-age children in neighbourhood attend school	0	0.9	1	1	0.03	-2.59
Had a preventive health check-up in the last 12 months	0	0.93	1	1	0.07	-3.25
Proportion of neighbours who had preventive health check-up	0	0.93	1	1	0.01	-2.23
Support from neighbours to improve the community: impossible as reference category						
Difficult to find support to improve community	0	0.56	1	1	0.25	-0.24
Neither difficult nor easy to find support	0	0.03	0	1	0.03	5.39
Easy to find support	0	0.29	0	1	0.21	0.92
Very easy to find support	0	0.01	0	1	0.01	8.14
Other individual variables						

Household member lived in different state five years ago (migration)	0	0.06	0	1	0.06	3.7
Natural logarithm of the value of entitlement	0	6.04	6.92	7.59	5.54	-2.11
Natural logarithm of the value of non-monetary health expenses	0	0.72	0	13.53	3.74	2.52
Natural logarithm of income brought by teenagers into the household	-0.18	6.34	6.51	10.93	2.12	-0.58

Source: ENIGH (2008)

TABLE C.2: DESCRIPTIVE STATISTICS OF DEPENDENT AND INDEPENDENT VARIABLES OF THE MODEL, URBAN SAMPLE

Variables	Minimum value	Mean	Median	Maximum value	Variance	Skewness
Dependent variable						
Oportunidades cash receipt (dichotomic)	0	0.07	0	1	0.06	3.45
Natural logarithm of Oportunidades cash received	0	0.42	0	9.13	2.45	3.53
Instrumental variables						
Predicted probability of cash receipt in neighbourhood	0	0.07	0	1	0.02	2.54
Residuals of the predicted probability of cash receipt at neighbourhood level	-0.78	-0.12	-0.12	0.73	0.03	0.31
Mean inverse mills ratio in neighbourhood (only included in the first step)	0	7.63	8.38	12.2	5.61	-1.16
Inverse mills ratio at household level	0	6.06	6.58	12.2	9.95	-0.44
Eligibility first-step						
Number of people per room	0.04	1.7	1.33	16	1.74	1.95
Demographic dependency	0	0.67	0.5	10	0.46	1.8
Female head of household	0	0.26	0	1	0.19	1.11
Households with children aged 0-11	0	0.89	1	10	1.19	1.29
Age of head of household	12	47.46	46	97	231.9	0.49
No social security benefits	0	0.41	0	1	0.24	0.37
No education of head of household	0	0.07	0	1	0.06	3.49
Incomplete elementary education	0	0.17	0	1	0.14	1.79
Shared or no toilet	0	0.09	0	1	0.08	2.88
Toilet without running water	0	0.2	0	1	0.16	1.53
Dwelling with dirt floor	0	0.03	0	1	0.03	5.56
Household without gas or electrical stove	0	0.05	0	1	0.05	4.23
Household without refrigerator	0	0.12	0	1	0.1	2.37
Household without washing machine	0	0.42	0	1	0.24	0.34
Household without car or truck	0	0.54	1	1	0.25	-0.16
Cash receipt second-step						
Usage of health services						
Household member attends public health centres	0	0.83	1	1	0.14	-1.75
Health insurance (Seguro Popular)	0	0.17	0	1	0.14	1.76
Interaction of health insurance and entitlement	0	1.18	0	7.59	7	1.8
Indigenous language spoken in household	0	0.06	0	1	0.06	3.58
Households who did not go to the doctor in the last 12 month because it was expensive	0	0.02	0	1	0.02	6.96
Household who did not go to the doctor in the last 12 months because of mistreatment / mistrust / did not speak the same language	0	0	0	1	0	14.72
Endogenous effects (conditionality)						
Teenagers attend school	0	0.78	1	1	0.15	-1.33
Teenagers in neighbourhood attend school	0	0.78	0.91	1	0.08	-1.27
School-age children attend school	0	0.9	1	1	0.07	-2.67
School-age children in neighbourhood attend school	0	0.9	1	1	0.03	-2.61

Had a preventive health check-up in the last 12 months	0	0.93	1	1	0.07	-3.25
Proportion of neighbours who had preventive health check-up	0	0.93	1	1	0.02	-2.22
Support from neighbours to improve the community: impossible as reference category						
Difficult to find support to improve community	0	0.56	1	1	0.25	-0.23
Neither difficult nor easy to find support	0	0.03	0	1	0.03	5.2
Easy to find support	0	0.29	0	1	0.2	0.94
Very easy to find support	0	0.01	0	1	0.01	8.03
Other individual variables						
Household member lived in different state five years ago (migration)	0	0.06	0	1	0.06	3.64
Natural logarithm of the value of entitlement	0	6.8	7.08	7.59	1.41	-5.06
Natural logarithm of the value of non-monetary health expenses	0	0.76	0	13.53	4.02	2.44
Natural logarithm of income brought by teenagers into the household	-0.17	6.37	6.51	10.93	2.06	-0.53

Source: ENIGH (2008)

TABLE C.3: DESCRIPTIVE STATISTICS OF DEPENDENT AND INDEPENDENT VARIABLES OF THE MODEL, RURAL SAMPLE

Variables	Minimum value	Mean	Median	Maximum value	Variance	Skewness
Dependent variable						
Oportunidades cash receipt (dichotomic)	0	0.53	1	1	0.25	-0.12
Natural logarithm of Oportunidades cash received	0	3.3	5.43	8.21	9.8	-0.06
Instrumental variables						
Predicted probability of cash receipt in neighbourhood	0	0.53	0.55	1	0.07	-0.28
Residuals of the predicted probability of cash receipt at neighbourhood level	-0.59	-0.04	-0.05	0.64	0.03	0.11
Mean inverse mills ratio in neighbourhood (only included in the first step)	0	1.92	1.15	10.18	4.5	1.3
Inverse mills ratio at household level	0	1.56	0.21	11.22	4.85	1.45
Eligibility first-step						
Number of people per room	0.09	2.46	2	14	3.08	1.49
Demographic dependency	0	0.93	0.84	8	0.68	1.69
Female head of household	0	0.19	0	1	0.16	1.55
Households with children aged 0-11	0	1.22	1	10	1.85	1.17
Age of head of household	14	48.65	46	97	260.16	0.46
No social security benefits	0	0.74	1	1	0.19	-1.11
No education of head of household	0	0.19	0	1	0.16	1.56
Incomplete elementary education	0	0.37	0	1	0.23	0.53
Shared or no toilet	0	0.26	0	1	0.19	1.1
Toilet without running water	0	0.51	1	1	0.25	-0.03
Dwelling with dirt floor	0	0.19	0	1	0.15	1.56
Household without gas or electrical stove	0	0.35	0	1	0.23	0.62
Household without refrigerator	0	0.41	0	1	0.24	0.38
Household without washing machine	0	0.73	1	1	0.2	-1.04
Household without car or truck	0	0.72	1	1	0.2	-0.96
Cash receipt second-step						
Usage of health services						
Household member attends public health centres	0	0.92	1	1	0.08	-3
Health insurance (Seguro Popular)	0	0.47	0	1	0.25	0.12
Interaction of health insurance and entitlement	0	1.75	0	7.59	8.53	1.09
Indigenous language spoken in household	0	0.23	0	1	0.17	1.31
Households who did not go to the doctor in the last 12 month because it was expensive	0	0.04	0	1	0.04	4.95
Household who did not go to the doctor in the last 12 months because of mistreatment / mistrust / did not speak the same language	0	0.01	0	1	0.01	10.68
Endogenous effects (conditionality)						
Teenagers attend school	0	0.65	1	1	0.18	-0.62
Teenagers in neighbourhood attend school	0	0.65	0.67	1	0.05	-0.4
School-age children attend school	0	0.88	1	1	0.07	-2.35
School-age children in neighbourhood attend school	0.37	0.88	0.9	1	0.01	-1.24
Had a preventive health check-up in the last 12 months	0	0.92	1	1	0.07	-3.22
Proportion of neighbours who had preventive health check-up	0.32	0.92	0.95	1	0.01	-1.86
Support from neighbours to improve the community: impossible as reference category						
Difficult to find support to improve community	0	0.57	1	1	0.24	-0.29
Neither difficult nor easy to find support	0	0.02	0	1	0.02	6.26
Easy to find support	0	0.31	0	1	0.21	0.82
Very easy to find support	0	0.01	0	1	0.01	8.61
Other individual variables						

Household member lived in different state five years ago (migration)	0	0.05	0	1	0.05	3.95
Natural logarithm of the value of entitlement	0	3.34	5.89	7.59	10.97	0.01
Natural logarithm of the value of non-monetary health expenses	0	0.57	0	11.29	2.7	2.76
Natural logarithm of income brought by teenagers into the household	-0.18	6.26	6.49	9.44	2.3	-0.69

Source: ENIGH (2008)

TABLE D1: SCHEDULE OF ENTITLEMENT ACCORDING TO HOUSEHOLD DEMOGRAPHIC CHARACTERISTICS

Type of support	Amount of entitlement
Food support	$185 or £9
Energy support	$50 or £2.5
Support per elderly in the household	$260 or £12.7
Support per children in:	
Third grade	$125 or £6.1
Fourth grade	$145 or £7.1
Fifth grade	$185 or £9
Sixth grade	$250 or £12.3
First grade of secondary education	$360 (£17.6) for male and $385 (£18.9) for female
Second grade of secondary education	$385 (£18.9) for male and $425 (£20.8) for female
Third grade of secondary education	$405 (£19.8) for male and $465 (£22.8) for female
First grade of post-secondary education	$610 (£29.9) for male and $700 (£34.3) for female
Second grade of post-secondary education	$655 (£32.1) for male and $745 (£36.5) for female
Third grade of post-secondary education	$695 (£34) for male and $790 (£38.7) for female

Source: Oportunidades (2008) section 4.4.1. Conversion in Great British Pounds is the author's calculation at a conversion rate of 1GBP=20.4MXN.