

Tackling Social Exclusion

Evidence from Chile

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Abstract

This paper studies an innovative welfare program in Chile that combines a period of frequent home visits to households in extreme poverty, with guaranteed access to social services. Program impacts are identified using a regression discontinuity design, exploring the fact that program eligibility is

a discontinuous function of an index of family income and assets. The analysis finds strong and lasting impacts of the program on the take-up of subsidies and employment services. These impacts are concentrated among families who had little access to the welfare system prior to the intervention.

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Tackling Social Exclusion: Evidence from Chile

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1 Introduction

Households in extreme poverty are generally deprived in multiple dimensions. The lack of material resources, coupled with little access to information, and other constraints to their decision making ability (Mullainathan and Shafir, 2013), limit the actions they can take towards improving their lives (Bertrand et al., 2006, Duflo, 2012). Although this is well known, most anti-poverty programs address single aspects in isolation of all other ones, and focus mainly on the lack of financial resources.

In 2002, Chile implemented *Chile Solidario* (CS hereafter), an anti-poverty program which was progressive by the standards of most countries, even in the developed world. The target of the program were the 5% poorest families in Chile, who were perceived not only to be poor, but also alienated from the welfare services potentially available to them. The program had two main

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components. On the demand side, there was an intense psychosocial support through home visits done by local social workers to help households acquire the skills they need to autonomously participate in (and benefit from) the services available to them. On the supply side, the state committed to coordinate different government agencies providing the social services. Many other Latin American countries began looking at this system of integrated social services as an example for their own policies, and a few of them introduced programs that mimic several aspects of CS (such as *Juntos/Unidos* in Colombia, *Brasil Sem Miséria* in Brazil and the most recent version of the Oportunidades program in Mexico, called *Prospera*).¹

This paper studies short and medium run impacts of CS for households who participated between 2002 and 2006. Our main results focus on four sets of outcomes for which data are available: the take-up of monetary subsidies, which provide short term income support to beneficiaries; participation in training and employment programs; employment of heads of household and their spouses and housing conditions.² We find that CS participants increase their take-up of a family allowance for poor children (the Subsidio Único Familiar, hereafter SUF) by 11%, relative to an average take-up of 65% among comparable non-participants. We are also able to detect an impact on the uptake of employment programs, of about 5-6% (from a baseline enrolment of 1%), especially in the short run. However, this is not accompanied by improvements in employment outcomes. Finally, we find no evidence of impacts on housing conditions of participant families.

Program impacts on the take-up of subsidies are driven by families not accessing welfare services before CS was implemented (the primary target group), and for families enrolling in CS only after 2004, when the supply of social services expanded. The increase in the uptake of SUF is 22% for families who did not take up these subsidies before 2002, and it lasts up to 4 years after the family entered in CS. If we further condition on having enrolled in CS after 2004, the impact on the take-up of SUF rises to 32%.³ Similarly, there is a 20% increase in the employment rate of females (spouses of the household head), that were not employed before 2002 and for those in families that enrolled in CS after 2004.

To evaluate the program we use a regression discontinuity design. Families are eligible to participate in CS if a poverty index is below a given threshold, which varies across municipalities and across years. Thus we compare, within municipality and cohort, the outcomes of families

¹Short term income support combined with the activation and employment support of low income groups are becoming an increasingly important policy tool of social protection strategies in OECD countries (Immervoll and Scarpetta, 2012).

²The choice of outcomes is dictated by the use of administrative data, which has a more limited set of outcomes than a household survey. The benefit of using administrative records is that we can study the universe of participants in the welfare system, as opposed to a small sample of them. We also analyze nine variables (available for only three years) concerning school enrolment of children, health coverage of children and adults in the household, and participation in employment centers.

³Since the baseline take-up of SUF is 65%, this means a change towards nearly universal coverage of this subsidy among eligible families registered in the social welfare system.

who are just eligible with the outcomes of those who are just ineligible for the program. The discontinuity in the probability of participation in CS induced by the poverty index is not sharp but fuzzy, since not all families identified as target of the program in 2001 were immediately served. This happened due to supply constraints. CS was rolled out for a period of 5 years and about 20% of the 225,000 target families were enrolled each year, giving priority to the poorest (more than 95% of families invited to participate in CS accept the invitation). Thus, the cutoffs are unknown in the initial years of the program. We estimate the effective cutoffs using the procedures applied in Chay, McEwan and Urquiola (2005) and Card, Mas and Rothstein (2008). We show graphically that there are striking discontinuities in participation at the estimated cutoffs in several of the municipalities in Chile. In addition, for those municipalities where the discontinuity is not visible at the estimated threshold, it is not visible in outcomes. In other words, for municipalities where the discontinuity exists, which account for 80% of our sample, we are able to accurately estimate its location.

We show that the average impact of eligibility for CS on participation in the program in a given year/cohort is about 21% (for households with a poverty index in the neighborhood of the discontinuity). We then produce intention to treat (ITT) and instrumental variables (IV) estimates of the impact of CS, using eligibility to CS as an instrument for participation in the program. We use administrative records that cover a period of 10 years (2000-2009). We start observing families at least a year before the introduction of CS, and we follow them two, four and up to six years after entry into the program.

It is important to learn from a program such as CS, given its targeting of the extreme poor in a middle income country, and the intensity of the engagement with the target population. Governments in middle and high income countries repeatedly express concern with the poorest families in their countries, and with the difficulty of designing effective support policies for this group.

Although there have been attempts to study CS in previous papers, our study represents a substantial improvement over past studies for three main reasons. First, we are studying the program's effectiveness almost 10 years after it was first implemented. This is important because programs of this type often take a few years until they become fully functional. In addition, the fact that we have a relatively long data horizon allows us to study both short and medium impacts of the program.

Second, the regression discontinuity estimator we implement is likely to produce more credible parameters than alternatives such as difference in difference estimators, which are likely to be affected by violations of the common trends assumption (since participants and non-participants in CS are probably on different trends, especially in the period studied in our paper, during which Chile experienced very high growth). It is unlikely there are serious threats to the internal validity of our estimates, but there are two challenges to their external validity. On one end, as in any

RD estimator, the population affected is located just in the neighborhood of the discontinuity. However, since we observe many different discontinuities across municipalities, this problem is much less serious. The group of families we are probably missing are those located at the very bottom of the income distribution, who are never in the neighborhood of any threshold, even in municipalities where this threshold is relatively low. On the other end, there are municipalities where the observed discontinuity is large, and others where it is not. Only the first ones contribute information to our estimates. We show that there are not large differences between the two types of municipalities except in one dimension: municipalities with large discontinuities are more likely to be urban. Therefore, our estimated impacts are more likely to be relevant for the urban than for the rural poor.

Finally, previous work has overlooked unique aspects of the program, in particular, the effectiveness of the program with respect to the level of social exclusion of families relative to the social services. We also show that the demand side component of CS is only productive when the corresponding supply of social services is enhanced and reorganized.

That said, it is useful to briefly review the existing literature on this topic.⁴ Galasso (2006) conducts the a short term evaluation of this program focusing on the first two years of program rollout. She uses a household survey that was specifically collected for the evaluation of this program, and two empirical methods, namely matching on the propensity score, and a regression discontinuity procedure analogous to the one used in our paper. She finds that the program induced significant impacts on the education and health of households, and the take-up of social benefits. This last result is consistent with the findings in this paper, whereas the administrative data we use do not allow us to analyze either health or education outcomes in detail.⁵ Our ability to use administrative records for the whole population of welfare recipients in Chile is a substantial improvement over the data used in her paper if the goal is to rely on an RD type estimator. The cost of using this data is that the set of outcomes that we can possibly observe is much smaller. Larrañaga, Contreras and Ruiz Tagle (2009) and Hoces, Hojman and Larrañaga (2011) were developed contemporaneously with our paper. They use the same administrative dataset as us, although in the latter paper they also complement it with the household survey originally used in Galasso (2006). They rely on a different method, differences-in-differences and matching, and focus only the two first cohorts of

⁴Colombia introduced in 2007 a program similar to CS (*Juntos*), but which unfortunately suffered from a number of implementation problems, limiting the comparison we can make with our study (see Econometria, 2011). This is especially unfortunate because, unlike CS (for which we must rely on quasi-experimental of evaluation), the evaluation of *Juntos* had originally an experimental design. However, during the evaluation period there was incomplete treatment with most families receiving at most 6 home visits (instead of the 20 originally expected), so only a very weak version of the program could be studied. In addition, social workers had a caseload that was much heavier than that of CS. Finally, although households were encouraged to take-up social services, in many cases such services were not available to them.

⁵The survey used in Galasso (2006) was designed for the use of a matching estimator, and it is neither particularly suitable nor large enough for a credible implementation of her RD estimator.

the program. They find small impacts of the program across a variety of dimensions. As mentioned above, the exclusive focus on the early stages of the program may be misleading because CS may not be working at full capacity. Similarly, their estimator that may be subject to likely violations of the common trends assumption.

Sarsoza and Urzua (2012) also study this program using the RD strategy introduced in Galasso (2006) and refined in this paper, and similar administrative records, but their focus is on children's outcomes. They use only on the first cohort of CS and focus on test scores of children, which are obtained from school records merged with program records.

Thus, CS is a program of general interest because it is a serious attempt to integrate several welfare services to tackle social exclusion, and because of its objective of connecting the most disadvantaged families in society to the welfare system in a sustained way. The central ideas behind a program such as CS are stressed in Banerjee and Duflo (2007) and in Duflo's (2012) Tanner lectures. The program seeks to tackle information and psychological barriers that the extra poor face in activating the demand for social programs (Mullainathan and Shafir, 2013).⁶ The home visits are central to help households acquire the skills they need to autonomously participate in (and benefit from) the welfare, education and health systems available to them. The monetary subsidies obtained as a result of the program may have beneficial effects on the school achievement for poor children as demonstrated in many settings (see Paxson and Schady, 2010, for the effects of a cash-transfer in Ecuador, and Dahl and Lochner, 2012, for the US).⁷ The increase in access to monetary subsidies and services for households previously disconnected from the welfare system is important not only because families are able to supplement their income through them, but also because they become more linked to the social protection system.

However, even a program as innovative such as CS is not able to transform the lives of the poorest families along key long term welfare outcomes, such as employment and housing. The take-up of employment programs is not accompanied by improvements in employment outcomes. Finally, we find no evidence of impacts on housing conditions of participant families. The target population is difficult to work with, lacking the right set of cognitive and non-cognitive skills, and working capital and psychological endowments. Our results provide support that the personalized social support provided by a program such as CS helps to stimulate the use of social services by families. We suggest that the use of employment services may improve employment outcomes when the supply side is tailored to the needs of the families and made available to them. A more

⁶The literature examining the take of social programs describes three leading causes of low take-up of social programs among the poor: lack of information about the program, high transaction costs, and stigma (Moffitt, 1983). Currie, 2006, puts emphasis on the costs of learning about and applying for a given program as a major deterrent for take-up of social programs. Currie's review suggests that stigma plays a smaller role compared to other motives.

⁷The extra income brought home by access to SUF can explain why Sarsoza and Urzua, 2012, find a decrease in school dropout among children 14 years old associated to CS.

intensive version of the program might be needed to promote sustained exit from poverty. Employment activation needs to be comprehensive to have a sizable and permanent effect on the earning opportunities of the poor. In low income settings, there is growing evidence that integrated programs that combine capital and skill enhancement can have sustained effects on self-employment and income (Bandiera et al., 2012 for poor adolescents and Bandiera et al., 2013, and Banerjee and Duflo, 2013 for the extreme poor).⁸ However, this evidence comes from more costly programs per family and localized interventions. In middle income countries, as in Chile, social programs are not integrated within the same intervention agency as in the case of the programs mentioned above. Instead provision of social services is the responsibility of the welfare system, that coordinates a set of programs to serve the needs of the poorest. In large scale programs serving an heterogeneous population, the identification of needs on the demand side (done in Chile by social workers) needs to be matched by supply side programs that are tailored to and correctly sequenced to serve the demand of the poor.

The paper proceeds as follows. In the next section we describe the program. In Section 3 we explain the empirical strategy; Section 4 describes the data. In Section 5 we present and discuss our results. Section 6 concludes.

2 Chile Solidario

CS was designed by the Chilean government to reach the families who lived in extreme poverty in 2002. We focus on the first five cohorts of entrants (2002-2006), since the mechanism of targeting and the structure of CS changed substantially after 2006. CS promotes the demand for social services through home visits and by offering preferential access to these services. On the institutions' side, CS promotes the coordination of different social services at the local level for a more effective targeting of the neediest families.

To be eligible a family needs to have an index of wealth, called CAS score, below a threshold, which is municipality specific. Since, we exploit this feature to identify the program impacts, we explain the details in Section 3. We describe here the main components of the program.

Home visits The home visiting component of CS lasts for 24 months. It consists of a total of 21 home visits of 40-45 minutes, with the interval between visits increasing over time. The visits are done by social workers.

The home visiting period has 2 phases: the initial 6-8 months are a period of intensive work be-

⁸The large theoretical literature on poverty traps shows how tackling both capital and skills constraints can alter the poor's occupational choices and make them exit poverty (Banerjee and Newman, 1993, Besley, 1995, Banerjee and Duflo, 2007).

tween families and counselor and the visits in the final 16-18 months serve to monitor the progress of the family, working on the weakest issues and to activate the demand for subsidies and social services families are eligible to. During the initial home visits, the social worker and families agree to fulfil 53 minimum conditions which range from family dynamics, housing and employment, to health and education. In practice, many of these minimum conditions are already satisfied at entry in the program, which highlights the importance of initial conditions, as each social worker ended up working on different set of conditions with each family (see table A.1 in Appendix)⁹. The social worker provides information and guidance on how to access existing programs and services in the community to help improve on these outcomes.

Each year a social worker is responsible for 50 families on average (SD 25) and there are about 2400 social workers working under CS in each year between 2002 and 2006. The turnover of social workers is substantial, and during these 2 years 20% of participant families received visits by more than one social worker.¹⁰

The direct cost of home visits (including the cost of the visit itself, the training of social worker, and supervision; and excluding the salary of social workers) amounts on average to USD\$131 per family/year (or, equivalently, USD\$263 for the two years of home visits, with per family cost ranging from USD\$200 in accessible municipalities to as much as USD\$1000 in very remote municipalities). In the first year of operation, the home visits accounted for almost all budget of the program. After 2004, the financial resources allocated to this component remained almost unchanged, and its weight decreased to 4% of the total budget leaving way to the supply side component of the cost (Raczynski, 2008, and Camacho et al., 2014). The home visitation component of the Colombian program *Juntos/Unidos* was cheaper at USD\$80 per family/year.

Guaranteed access to monetary subsidies Participating families receive a monthly cash transfer (called *Bono Solidario*) during the first 24 months, with the size of the transfer ranging between USD\$8 and USD\$21 per month (decreasing over time).¹¹ For the subsequent three years, families

⁹The conditions are set at a very low level of exigency, so that, for example, most of the 19 minimum conditions to be met by families at the end of the intervention regarding education and health are fulfilled prior to home visits by CS social workers (see table A.1 in Appendix A).

¹⁰There is substantial variability in the average caseload of social workers across Chile. Take the year of 2005. In the regions of Aysen and Magallanes the average load is 16 (0.8) and 14 (1.1) families, respectively. On the other extreme, there the following regions: Tarapaca with 53 (7.3), Coquimbo with 58 (1.5), Bio-Bio with 60 (50.2) and Araucania with 61 (27.3). The numbers in parenthesis are the number of inhabitants/ km^2 in 2002, to relate the caseload with the sparsity of the population. Data on the demographic characteristics for 559 social workers who worked on CS between 2007 and 2009 shows that 88% of them are women, 80% have a degree on social work, and the average age is 26.

¹¹The amount of the Bono is: \$21 per month for the first 6 months, \$16 per month between month 7-12, \$11 per month for months 13-18, and \$8 for the last 6 months. These amounts are for 2006 but they are adjusted yearly for inflation. Transfers begin at about 15% of the average income of eligible families in the first six months after enrolment in CS, and gradually decline to about 10% of income by the end of the two years of home visits.

receive the *Bono de Ingreso* (exit grant), which amounts to roughly \$8 per month. The transfer is uniform across families. The amount of the CS transfer is much lower than that of other well known cash transfers in Latin America. The goal of the monthly transfer is to compensate families for the costs of participating in the program, instead of consisting of a subsistence transfer, as in other conditional cash transfers.¹²

If eligible, families in CS are guaranteed access to a monthly (non-contributory) allowance for poor families with children less than 18 years of age (SUF - *Subsidio Único Familiar*); the pension for the elderly poor, for the disabled, and for individuals with mental disabilities (PASIS - *Pension Asistencial*); and the water subsidy (SAP - *Subsidio de Agua Potable*), which covers the water bills for up to 15 cubic meters of monthly consumption. The eligibility to SUF and PASIS is based on categorical requirements¹³ and they are means-tested, based on income and the CAS score is used to determine priority in allocation of slots. In practice, the cutoffs for SUF and PASIS are well above than those of CS, so that all households used in our regression sample are "income" eligible to SUF and/or PASIS.¹⁴

Since 2004, the CS budget includes a provision on the amounts dedicated to SUF, PASIS and SAP for families in the program (see Law 19,949 of 2004¹⁵). In 2005 and 2006, the total of payments relative to these three social subsidies and CS grants (*Bono Chile Solidario* and *Bono de Ingreso*) added to 63,5% of the total budget of CS.

Preferential access to social services and the reorganization of the supply side Participating families have preferential access to a whole array of social services available in their municipality of residence. In theory, the set of services available to participant families covers the seven areas of program intervention (see table A.1 in Appendix). However, between 2002 and 2004, municipalities and local services providers simply improved the coordination of different programs serving the target population with no increase in the supply of services. With the approval of the law that regulates CS in 2004, there was an improvement in the quantity and quality of the supply of such auxiliary services. The programs (i) re-directed the existing supply geographically, in proportion to the needs of CS families in each municipality; (ii) were tailored to the needs of the target population; and (iii) new programs were created.¹⁶ The budget share allocated to the provision of programs to CS beneficiaries increased sixfold between 2003 and 2007 (Mideplan, 2009, Cama-

¹²For example, depending on the family structure, cash transfers for the poor from Mexican Oportunidades may exceed \$150 per month, and the Bolsa Familia monthly transfer in Brazil varies between \$40-\$60 per family.

¹³Presence of children in household in case of SUF and elderly or disable in case of PASIS.

¹⁴SUF awarding is conditional on school enrolment and health check-ups of children. The amount SUF transfer is about USD\$8 per month (as of 2006) per child. Eligibility to SAP is conditional on water being supplied by the public network and in having an up-to-date bill (this last requirement is not verifiable in the administrative records).

¹⁵See <http://www.leychile.cl/N?i=226081&f=2012-05-17&p=>.

¹⁶See table A.2 in the Appendix A.

cho et al. 2014), so that in 2005-2006, the financing of these services represented 1/3 of the budget of the program.

Employment and training programs are of particular interest due to their potential effects on individuals' labor market outcomes and the long term earnings profile of program participants. As a result of the increase in funds provided to social services, the coverage of the potential demand for employment programs among CS beneficiaries increased from 24% in 2004 to 100% in 2007. We obtained access to individual records of participation in these programs from the provider of employment services for the years of 2004 and 2007. Two types of programs are available: those exclusively for individuals in CS families and programs where CS individuals were given preferential access. We study the effect of CS on the take-up of this last type of programs. To understand the impact of the supply expansion due to CS we disaggregate our analysis by cohorts of entry, depending on whether families entered the program before or after 2004.

3 Empirical Strategy

Our goal is to estimate β from the following equation:

$$Y_i = \alpha + \beta CS_i + f(X_i) + \varepsilon_i \quad (1)$$

where Y_i is the outcome of interest for family i , CS_i is a dummy variable indicating whether the family participated in Chile Solidario, X_i is a vector of controls (entering through function $f(\cdot)$), and ε_i is an unobservable. β is the impact of the program on Y which, in principle, can vary across individuals. Even if β does not vary across individuals, its estimation by ordinary least squares (OLS) may be biased if participants and non participants differ in terms of their observable and unobservable characteristics. Participants in CS are indigent, and therefore they are on average much poorer than those who do not participate in CS. It could also happen that, among the eligible, those who participate are the ones more eager to improve their situation. In order to estimate the causal effect of the program, we use a regression discontinuity design, exploiting the fact that the program eligibility rules imply that the probability that a household participates in CS is a discontinuous function of its CAS score. That is, in each municipality and time period, we compare outcomes of families just below (just eligible for entry at time t) and just above their respective cutoffs (see, for example, Hahn et al., 2001; Imbens and Lemieux, 2008; Lee and Lemieux, 2010).

Selection of families and coverage The CAS score is constructed from an instrument used to select families to several social programs in Chile, the Ficha CAS (see section 4 for details about Ficha CAS). The number of families to serve in each municipality was assigned in proportion of

the percentage of the population in extreme poverty, P_m , based 2000 income distribution (see Law 19949 of 2004). Then, the official cutoff score of CAS for each municipality is the value of CAS such that the proportion of families below that CAS score within the municipality is exactly equal to P_m .¹⁷

In practice, there were capacity constraints and not all eligible families with score below P_m were invited in the first year of operation. Thus, out of the 225,000 families deemed eligible by 2002, the government decided to serve around 50,000 families per year up to 2005 (there was an additional provision to serve 50,000 more families in 2006). Within each municipality, families with lowest CAS were the first to enroll (see Law 19949 of 2004). This sequencing implied that the cutoffs P_m were not binding in the first few of years of implementation of the program.

Effectively, in the initial years of CS every year each municipality had to determine the number of slots to fill in accordance to the resources available. However, when selecting families for the program it was not always possible to serve the poorest families first. Municipalities had to decide a criteria to identify who is poor enough to enter the program in a given year, and at the same time they were constrained to select families who are located in a catchment area of an existing social worker, or in an area where the municipality was planning to introduce a social worker.¹⁸

Eligibility Thresholds Each year municipalities need to set ex-ante a threshold such that all slots are exhausted. Information from program administrators and empirical evidence (figure B.1 in Appendix and Raczynski, 2008) show that the threshold is set as a compromise between maximizing the number of poorest families to be served every year and minimizing the costs of allocation of social workers. For example, suppose there were X slots in a municipality, and that all families living there were ordered according to their CAS, with the X poorest families being served. If social workers were sent to find additional families until all slots were filled, this procedure could imply that there would no discontinuity which we could empirically explore, since program participation would decline smoothly with the CAS score. However, it is unlikely that this is the case, for two

¹⁷In the initial stages of CS there was an effort to register indigent families with the CAS system. However, new registration occurred only in few isolated instances (Larrañaga and Contreras, 2010).

¹⁸In order to maximize the number of families served by the program, municipalities needed to optimize the amount of travel done by each social worker. This was done by assigning work areas to different social workers, to avoid that all social workers were travelling within the entire area of the municipality. New families enrolling in CS in each area were assigned to the social worker(s) already working in that area, provided that their caseload allowed for it. These work areas were small in dense urban locations, and large in sparsely populated rural locations. Accordingly, worker caseload in urban areas is usually larger than in rural areas, where a social worker needs to travel large distances to see all families. If a potential new family is located in a neighborhood with no social workers, or where social workers are operating at their maximum capacity, it is unlikely that it will be served by the program that year. Figure B.1 in Appendix shows that there is a negative relation between the average share of eligible families per neighborhood in a given municipality and the proportion of neighborhoods served in a municipality in 2002, but not in 2005. This suggests that the initial strategy of each municipality was to serve first clusters of neighborhoods with a higher share of eligible families. This is consistent with the existence of catchment areas for social workers in order to minimizing their transportation costs.

reasons. First, there is substantial entry and exit of families from the CAS system, and among the existing families there are frequent changes in their CAS scores (each family's CAS needs to be adjusted at least once every two years). So, even if it was possible to maintain a workable and up to date database which could be used to manage program entry in real time right at the outset of the program, the thresholds for eligibility could potentially have to be adjusted each time a new family entered the system, left the system, or experienced a change in the score. As a result, the set of families eligible or not for the program could be continuously changing throughout the year, with some families starting to receive the program and then deemed ineligible, and others told they would not receive the program and suddenly becoming eligible. It is not likely that it would be possible to exclude families already invited even if they became ineligible in the meanwhile, and it would be equally difficult to exclude newly eligible families from entering the program (and thereby exhausting one of the existing vacancies).

Second, because there is a limited number of available social workers in each municipality, their workload had to be geographically organized. This means each social worker was allocated families in a particular location, or contiguous locations, and that social workers working in areas where families were spread apart (say, rural areas), had lower caseloads, because more of their time was used in travel.

Therefore, each municipality estimates the number of beneficiaries to be served in a given year taking into account the number of families in the bottom of the distribution of CAS not served by CS at the start of the year, potential entries, exits and changes in the CAS system, and it combines these estimates with the existing geographical distribution of potential beneficiaries and social workers.

Empirically, we show, first, that there exist striking discontinuities in participation in CS as a function of CAS scores in several of the more than 300 municipalities in Chile. Second, the participation rate of eligible families in the program in each year is well below 100%. This is unlikely to be a demand problem, since virtually every family invited to CS is reported to accept the invitation (out of all invited families only 4.7% did not participate; see table A.3 in the Appendix A). Then, the low participation rate is likely to be a supply problem, driven by the limited resources and imperfect setting of the threshold which prevent full coverage of the eligible population.

Thus, for each municipality between 2002 and 2006 the *effective* cutoff are not observed and must be estimated from the data. We rely on a method used in Chay et al. (2005) and Card et al. (2008), which is similar to identifying structural breaks in time series data. For each municipality and year, we define a grid along the CAS distribution. To ensure that there are families on either side of each grid point, g , we construct the grid on the range $[CAS_{mt}^{\min} + 20, CAS_{mt}^{\max} - 20]$, for municipality m in year t , $t = 2002, \dots, 2006$. Then, for each g , we define a hypothetical eligibility dummy for potential entry, $E_{imt}^g = 1 [CAS_{imt} \leq g]$, and estimate the following equation for each

municipality m in year t , $t = 2002, \dots, 2006$,

$$D_{imt} = \alpha + \psi E_{imt}^g + \varepsilon_{imt} \quad (2)$$

where D_{imt} takes a 1 if family i in municipality m enters CS in year t , and 0 otherwise. We then select as the cutoff score the value of CAS g that maximizes the R^2 of (2). Hansen (2000) shows that if equation (2) is correctly specified, this procedure yields a consistent estimate of the true cutoff.¹⁹ We use the estimated threshold as the true thresholds in a standard regression discontinuity analysis.

Figure 3 plots the proportion of families participating in CS as a function of their CAS score measured in 2002 for 6 (randomly selected) municipalities. There are two vertical lines in each graph: a solid line indicating the estimated effective cutoff, and a dashed line indicating the official cutoff. If these cutoffs coincide the lines are superimposed. In the 3 municipalities at the top of this figure there are clear discontinuities in participation at the threshold, while in the bottom three the discontinuities are not visible. Similar figures for all municipalities in Chile, and for 2002 and 2005 are shown in figures B.2 and B.3 in Appendix (the figures for 2003 and 2004 are available from the authors). For example, in 2002, for 90% of the municipalities the estimated discontinuity size, $\hat{\psi}$, is at least 7 percentage points. Municipalities with small discontinuities end up contributing with no information to our estimates, which are relevant only for the set of municipalities where there is a discontinuity in participation at the threshold. In the empirical application, our results are aggregated across the different municipalities just shown (we also present results separately for those municipalities with and without a discontinuity at the estimated cutoff).²⁰

Eligibility and Participation The regression discontinuity is fuzzy since municipalities had some discretion in the order of invitation of eligible households, according to the geographic matching of families to social workers.²¹ This means that the mapping from eligibility to participation in CS is not perfect. We address this problem by presenting instrumental variables estimates

¹⁹For the sake of precision, we exclude municipalities with less than 50 families and municipalities without CS participants. This implies that we drop between 6 (in 2002) and 13 (in 2005) municipalities, depending on the cohort.

²⁰Simple univariate correlations, show that municipalities with a high discontinuity, where the estimate for ψ at the value of CAS-score that maximizes the R^2 of (2) is at least 0.1, are more likely to be predominantly urban municipalities. However, this is the only observable dimension on which we could detect any relevant differences.

²¹Another way of looking at the sequencing of entry into the program across cohorts can be found in figure B.4 in the Appendix B, which plots the distribution of estimated CS cutoffs across municipalities, for each year between 2002 and 2005. As expected, the distribution gradually shifted to the right over time. In 2002, the *effective cutoff* is higher than the official cutoff in 86.5% municipalities and in 2005 this proportion is reduced to 60%. The average difference between the effective and official cutoff is 16 points in 2002, but it drops to 2.9 points in 2005.

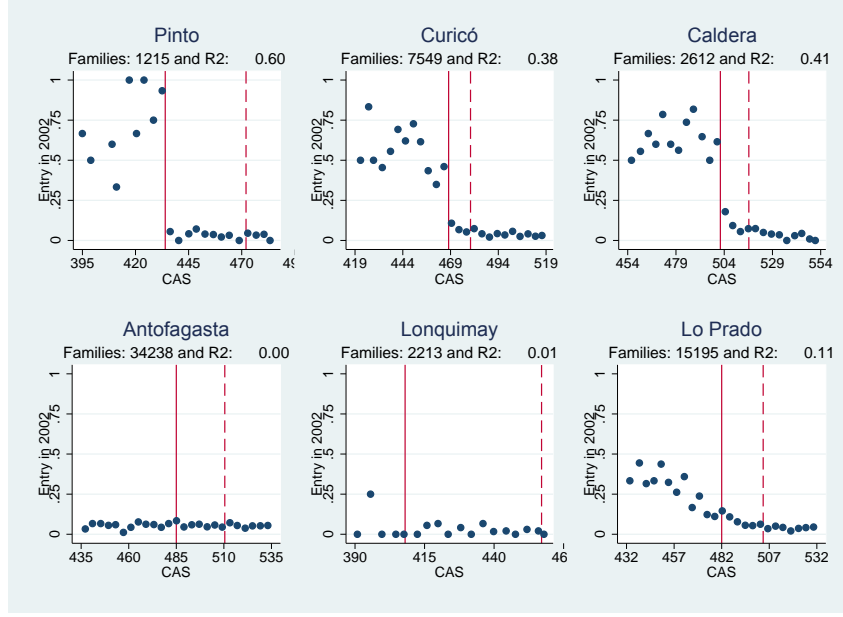


Figure 1: Participation in CS and *effective cutoff* and *official cutoff* in illustrative municipalities (2002).

Note: The dots on the graphs are the proportion of families in municipality entering in CS in each year (see y-axis) by intervals of CAS score of 4 points in distribution of CAS in 2002 in each municipality. The solid vertical line represents the point identified as *effective cutoff* and the dashed line is the *official cutoff*. Where only one line is shown, the two coincide. The R^2 in the top of each graph is the R^2 for the cutoff score of CAS that maximizes the R^2 of equation (2) - the *effective cutoff*. These figures are zoomed around the *effective cutoff*, so that only families at most 40-points apart from it are depicted (the CAS score varies between 380 and 770 points).

of the program computed as described in expression (3) (for very small ϵ):

$$\frac{\lim_{\epsilon \rightarrow 0^+} \Pr(Y_i = 1 | CAS_{imt} = \overline{CAS}_{mt} - \epsilon) - \lim_{\epsilon \rightarrow 0^+} \Pr(Y_i = 1 | CAS_{imt} = \overline{CAS}_{mt} + \epsilon)}{\lim_{\epsilon \rightarrow 0^+} \Pr(CS_{imt} = 1 | CAS_{imt} = \overline{CAS}_{mt} - \epsilon) - \lim_{\epsilon \rightarrow 0^+} \Pr(CS_{imt} = 1 | CAS_{imt} = \overline{CAS}_{mt} + \epsilon)}. \quad (3)$$

Families just above and just below the cutoff differ in their eligibility to CS, but they are likely to be similar in all other (observable and unobservable) dimensions.²²

We also restrict the sample to those families whose CAS is near the cutoff for the program, since

²²See table A.4 in Appendix A for the main correlates of participation in CS. This table uses only the sample of families who were eligible according to the official cutoff and information for the first time observation in the Ficha CAS. Families who are selected to CS are more likely to be connected to the welfare system. Within municipality, families who are selected to CS are less likely to have adequate walls or ceilings in their homes, less likely to be legal occupants of their home, and more likely to have a connection to the sewage network. Participant families have lower CAS scores than non participants, but heads and spouses in participant families are more likely to be working than those in non-participating families. Selected families are more likely to have younger heads, married heads, female heads, and children. They are less likely to belong to the dominant ethnicity in the neighborhood, and to live in urban areas (typically, more accessible).

points away from the discontinuity should have no weight in the estimation of program impacts (see e.g., Black, Galdo, and Smith, 2005, Lee and Lemieux, 2010). Thus, we focus on the sample of families whose CAS was at most 20 points apart of their municipality’s cutoff (we also present estimates using alternative bandwidths).

Finally, standard applications of regression discontinuity compare boundary points of (non-parametric) regressions of the outcome Y_i on CAS, estimated on each side of the discontinuity point. Since we have several discontinuity points, one alternative (which we implement) is to normalize all of them to zero, and instead of the absolute value of CAS, consider instead $CAS_{im} - \overline{CAS}_m$, which is the difference between a family’s CAS and the municipality cutoff in the relevant year. We start by estimating the following model:

$$Y_{imk} = \phi + \gamma E_{im} + f(CAS_{im} - \overline{CAS}_m) + u_{imk} \quad (4)$$

where E_{im} is an indicator of eligibility for the program and u_{imk} is an idiosyncratic shock. We control for a non-linear function of CAS (normalized by the threshold). In practice, we use a quadratic in $(CAS_{im} - \overline{CAS}_m)$, different in either side of the cutoff, but we also present a robustness analysis using other parametric functions of distance to cutoff. Our models include municipality-year effects, which absorb municipality-year shocks that may affect the outcome independently of eligibility (for example, shocks in the local supply of social services, or shocks to the local labor market).

We then compute program impacts using a standard two-stage least squares procedure. All coefficients are estimated using a linear probability model in the first stage, where we regress a dummy variable indicating participation in CS on the eligibility dummy, controlling for distance to cutoff through $f(CAS_{im} - \overline{CAS}_m)$. We then obtain the predicted probability of participation in CS estimated, \widehat{CS}_{imt-k} . In the second stage we estimate:

$$Y_{imk} = \alpha + \theta \widehat{CS}_{imt-k} + g(CAS_{im} - \overline{CAS}_m) + \varepsilon_{imk} \quad (5)$$

where $k = 2, 4, 6$ (which means that we study the effects of CS two to six years after the start of home visits), and participation at lag k is instrumented by eligibility for the program at lag k in their municipality of residence.

We estimate models pooling all cohorts together as well as separately by cohort. When we pool them, we restrict the coefficients of the model to be the same across all cohorts.²³ We define cohorts of potential entrants each year in terms of potential entry into the program. Since 2002 is

²³Once a family enrolls in CS, it remains in the program for 5 years in total, even if its CAS score rises above the eligibility threshold during this period. This means that, at each period t , eligibility only determines participation for those not yet enrolled in CS. Our estimates are valid for a sample which is changing over time, which is only important if program impacts vary substantially across families.

the first year of the program, every family who is in the CAS database in that year is a potential entrant, and it is labeled as belonging to the 2002 cohort. To define subsequent cohorts, we consider every family in the CAS database in that year, but who has not enrolled in CS in any prior year. Throughout the paper we refer interchangeably to the 2-year impacts as short run effects, the 4-year impacts as medium run effects and the 6-year impacts as long run effects (the latter are referred to in the paper but only presented in the appendix). All models include standard errors clustered at the municipality level (the municipality is measured at the time of eligibility).²⁴

Multiple Hypotheses Testing In the presentation of our results the stars next to each coefficient indicate whether it is statistically different from zero, after accounting for multiple hypothesis testing using the procedure in algorithms 4.1 and 4.2 of Romano and Wolf (2005). This procedure requires to test at once all the null hypotheses. We test the null hypotheses for short and medium run estimates separately, and the null hypotheses of the different subgroups are tested together, to control for splitting the population into multiple subgroups. In practice, to adjust for multiple hypothesis testing, we estimate for each bootstrap sample the models corresponding to tables 3, 4, 5, 6 and 7. This implies that we test simultaneously 111 hypothesis.²⁵

Algorithm 4.2 is a iterative rejection/acceptance procedure, for a fixed level of significance. In the tables presented, the stars are reported for levels of 1%, 5%, and 10%, which means that we apply the algorithm three times, once for each level. The critical values are adjusted for a two sided test. We use 250 bootstrap replications to obtain the adjusted critical values and we account by potential correlation of residuals within municipality using block-bootstrap.

Robustness Analysis We perform a battery of checks to assess the validity of our empirical strategy. We start by performing standard balancing checks, by analyzing whether there are any differences between families just above and below the cutoffs in terms of variables measured before 2002.

We show that our results are not driven by the choice of the functional form for $f(CAS_{imt-k} - \overline{CAS}_{mt-k})$, nor they are sensitive to trimming the sample around cutoff and the choice of the bandwidth. We have similar results regardless of whether we control for interactive municipality-year effects, or whether we include only additive municipality and year effects, which suggests that municipality specific shocks are not likely to be correlated with how CS is rolled out across years.

²⁴Bai, 1997, shows that sampling error in the location of a change point can be ignored in estimation of the magnitude of the break. We rely on this result and do not adjust our standard errors for the estimation of the cutoffs, as Card et al., 2008.

²⁵There are 16 outcomes used for placebo estimates for corresponding to the samples of short and medium run; 19 outcomes used for short and medium run estimates; for short and medium run we condition on the fulfilment or not the condition before 2002; and, although not presented, also allow the effects for type of area of residence (rural or urban).

Estimates are also similar if we include neighborhood fixed effects (neighborhoods are defined within municipalities). We estimate the effects separately for the sample of families living in municipalities where the size of the estimate of ψ in equation (2) is high and for those where it is low. Our results are driven by the first set of municipalities. In our main set of estimates we restrict the sample to those families who were present in the CAS system prior to the introduction of CS (in 2000 or 2001).

Finally, our results are robust to alternative methods to obtain the cutoff. In particular, we follow a split-sample approach suggested in Card et al., 2008, to obtain the thresholds.²⁶ That is, we re-estimated the *effective cutoff* using a random sample of families in each municipality-year. The *effective cutoff* for eligibility for each year between 2002 and 2006 and for each municipality is estimated as in equation 2. However, instead of using all families in a given year and municipality with a valid CAS, a random sample of 2/3 of families is used. For each municipality-year the cutoff is similar to that obtained using the whole sample (it is available from the authors). Then, using these thresholds we estimate models 4 and 5, but restricting the sample to those families not used to estimate the municipality-year cutoff relevant to assess the impacts of CS.

Most of these robustness checks are included in Appendix A, but we refer to the most important ones in the main text.²⁷

4 Data

Our analysis is based on administrative data: the *CAS Consolidado* (for 2000-2006), *Ficha de Proteccion Social* (FPS) (for 2007-2009), and the registers of participants in CS and other welfare programs.

CAS Consolidado covered about one-third of the Chilean population in 2006. The FPS expanded the coverage from 2007 onward, reaching two-thirds of the population in 2009. These records include all families (and their members) applying to any publicly provided social program in Chile. We can link individuals across years through their national ID number (the RUN-Rol Unico Nacional). The data include individuals surveyed between March 1998 and December 2009, covering over 14 million individuals, corresponding to nearly 60 million observations (see more

²⁶This split-sample method corrects also inference for estimated cutoffs, which may result from identifying a change point through structural breaks, since the introduction of specification search bias may lead to over reject a break of zero too often.

²⁷In Appendix C we adapt the standard RD procedure to a dynamic version similar to Cellini, Ferreira and Rothstein (2010), to allow for the fact that individuals who do not receive CS in a given year may receive it in subsequent years. The gradual rollout of CS means that subsequent program entry by ineligible can lead to underestimate program impacts, since we assume that the initial group of ineligible does not receive any additional subsequent treatment beyond what we observe in the first year (defining the cohort). On the other end, if there is additional entry by eligible and if this is not accounted for, our estimates may be too large relatively to true program impacts. Estimates presented in table C.2) in Appendix C are not substantially different from our main results.

details about the dataset in Appendix D). We have access to both the detailed information on the CAS and FPS forms, but also to the overall scores computed using that information. The scores are used to construct eligibility for CS and most targeted social programs.²⁸

The government has been using the Ficha CAS as a targeting instrument since the 1980s. It consists of a two-pages form that households must fill if they wish to apply for benefits. It contains information on housing conditions (e.g., material used for the construction of the house, access to water, sanitary services); characteristics of household members (occupation, educational level, date of birth, and income); and ownership of assets (housing property, refrigerator). This information is used to construct a score ranging from 380 to 770 points. Households with a CAS score below 500 are considered indigents, and those with a score between 500 and 540 are considered poor. The CAS score is valid for 2 years. Up to 2007, the CAS-score was used to determine eligibility not only for income transfers (pension assistance for old age - PASIS, and family allowance - SUF), for the water subsidy (SAP), access to social housing, and childcare centers (Larrañaga, 2005).

In 2007, the Ficha CAS was replaced by a new targeting instrument, the FPS.²⁹ The information in FPS is administratively updated every month, using cross-checks with other administrative records. We obtained information taken in 3 dates: August 2007, December 2008 and December 2009.

The Chilean national ID allows us to merge the CAS and the FPS to (i) the register of families participating in Chile Solidario since its inception until May 2009 and to (ii) the register of all individuals participating in social promotion and training programs offered by FOSIS³⁰ between 2004 and 2007.

The final sample we use includes about 4.3 million families whose head is aged 18 to 75 in 2002 and who are observed at least once between 2002 and 2006 (the years of potential entry in CS; some are observed between these years, and also in the FPS 2007-2009). We restrict our sample to families who we observe at least twice (in the year of potential entry, when eligibility is measure, and at least one additional year after that, when outcomes are measured). We are left with 2.7 million families after imposing this constraint. Finally, we restrict our main analysis to those families located at most 20 CAS-points apart of the eligibility thresholds, which implies that our main sample includes nearly 0.5 million families.³¹

²⁸The year of 2006 was a transition-year, and starting in 2007 eligibility to CS was based on a national threshold for a new score. Because of this change, in 2006 there were no families newly surveyed for a CAS score and the register contains about half the number families than in previous years.

²⁹Given that the introduction of the new targeting mechanism was associated with new eligibility rules to CS in this paper we do not focus on the effects for families that entered in CS in 2007 or after.

³⁰FOSIS stands for Fondo de Solidaridad e Inversion Social – Fund for Solidarity and Social Investment, which implements several programs in the areas of entrepreneurship, employment and social empowerment.

³¹Of these families, one third is observed twice, another one third is observed three times, 23% are observed 4 times, and the remaining families are observed 5 or more times between 2002 and 2009. We also present in Appendix estimates for a sample of families which are more than 20 CAS-points apart of the eligibility thresholds.

Descriptive Statistics Table 1 includes some descriptive statistics, for the overall sample as well as for families who were ever eligible for CS. There is one observation per family in the table. We show the characteristics of families measured the first time they are observed in the data (2000, or 2001 if the family did not have a valid CAS score in 2000, before CS was implemented nationwide). We present separate statistics for the whole sample and for those families who are eligible to CS at least once between 2002 and 2006 according to the official cutoff in the municipality of residence. The information is divided into five areas: (1) use of subsidies, (2) housing characteristics, (3) variables related to CS, such as participant rate and CAS score, (4) employment and income related variables, and (5) demographic characteristics.

As expected, eligible families are more likely to be disadvantaged along multiple dimensions. They have on average a lower CAS score and they are more likely to be receiving subsidies, and to be illegal occupants of the house where they live. Their houses are less likely to have adequate ceiling and walls, less likely to have water provided by the public network, less likely to have a fridge or to have water heating, and a higher density of occupation as measured by the ratio of persons in the house to the number of rooms. Eligible families also show a different employment profile than the general population: heads are less likely to be working, and, when working, they are more likely to be self-employed than the average individual (57% among eligible vs. 41%; the alternative to self-employment is wage work). Spouses (of the head) are also less likely to be employed (13% among eligible vs. 24%). Finally, eligible families are on average younger, they have children, and are headed by individuals with below average education.

About half of the families who were present in the CAS in 2000/1, and who were ever eligible to CS (according to the official eligibility condition), ended up participating in the program.

5 Results

5.1 Eligibility and Participation in CS

We start by showing that eligibility for CS predicts participation in the program. Figure 5.1 shows how the proportion of families participating in CS varies with the distance between each family's CAS score and the municipality cutoff score for participation in CS. We present a plot for each of the cohorts of CS between 2002 and 2005 (2006 is an incomplete cohort as we mentioned in section 4, therefore we present only regression estimates for its first stage).

The dots in the figures correspond to cell means for participation in CS, after we divide the sample around the cutoff into groups. The groups are obtained by dividing the CAS values around the cutoff into bins of size 2 (the mean CAS for this sample is 478, and its standard deviation is 36 for the sample around the cutoffs). We consider only families with CAS scores within 20

Table 1: Baseline Characteristics of Families (2000-2001).

Variable	(1)	(2)	(3)	(4)	(5)	(6)
	N	All Mean	S.D.	N	Ever eligible Mean	S.D.
Any subsidy	1788715	0.35	0.48	390382	0.51	0.50
SAP	1788243	0.16	0.37	390184	0.09	0.29
SUF	1094825	0.24	0.43	258880	0.47	0.50
Housing						
Legal occupation of house	1788706	0.60	0.49	390380	0.39	0.49
Owner of house (condition on legal occupation of house)	1072670	0.82	0.39	152437	0.77	0.42
Adequate walls	1788717	0.45	0.50	390382	0.24	0.43
Adequate roof	1788717	0.64	0.48	390382	0.33	0.47
Overcrowding	1783921	1.24	0.78	389537	1.59	1.03
Water from public network	1788717	0.90	0.31	390382	0.73	0.44
Fridge	1788706	0.59	0.49	390380	0.27	0.44
Sewage connected	1788717	0.65	0.48	390382	0.30	0.46
Heating	1788706	0.26	0.44	390380	0.02	0.15
CS and CAS						
CAS	1788706	542.15	55.22	390380	478.06	34.28
Ever in CS	1788717	0.22	0.41	390382	0.47	0.50
Labor Market and Income						
Employed (head)	1788599	0.71	0.45	390349	0.69	0.46
Self-employed (head)	1788599	0.41	0.49	390349	0.57	0.50
Dependent worker (head)	1788599	0.31	0.46	390349	0.12	0.33
Employed (spouse)	1152166	0.21	0.41	239648	0.13	0.34
Self-employed (spouse)	1152166	0.11	0.31	239648	0.11	0.31
Dependent worker (spouse)	1152166	0.10	0.30	239648	0.03	0.17
Imputed income	1639269	0.57	0.50	323166	0.79	0.40
Monthly Income per capita	1788717	30317.15	27087.26	390382	16453.52	14854.09
Demographics						
Age of head	1788717	45.82	14.41	390382	45.73	15.06
Single headed	1788717	0.36	0.48	390382	0.39	0.49
Male head	1788717	0.70	0.46	390382	0.70	0.46
Years of Schooling of Head	1788714	7.36	3.82	390380	4.85	3.26
Years of Schooling of Spouse	732779	7.44	3.74	150185	7.43	3.75
Presence of children	1788717	0.61	0.49	390382	0.66	0.47
Family Size	1788717	3.64	1.71	390382	3.83	1.87
Minutes family takes to nearest health center	966954	24.05	23.19	220796	31.48	32.28
Family belongs to dominant ethnicity in neighborhood	513308	0.89	0.31	120593	0.89	0.31
Rural	1788717	0.17	0.38	390382	0.34	0.47

Note: The table includes the mean and standard deviation for selected variables for the whole sample of families in the data (columns 1-3) and for the set of families that were eligible for CS at least once according to the official cutoff between 2002 and 2006 (columns 4-6). There is one observation per family in the table which is measured prior to the introduction of CS in 2002 (in particular, we include the characteristics of families when they were first surveyed in 2000 or 2001). The only variables which were measured after 2002 are "Minutes family takes to nearest health center" and the indicator for whether "Family belongs to dominant ethnicity in neighborhood", which are measured in FPS (2007-2009). The time it takes from family's residence to the nearest health center is the average for the observations a family has in the FPS data, whereas ethnicity of family is the ethnicity of the head the first time a family has FPS data (which can be 2007, 2008 or 2009).

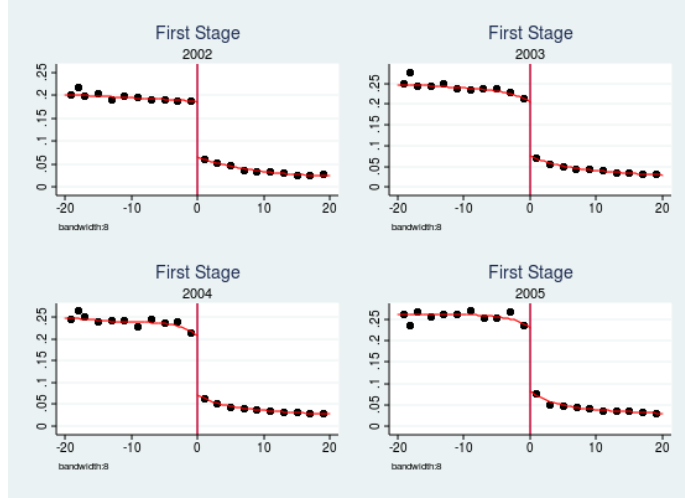


Figure 2: Participation in CS among eligible and non-eligible: Eligibility defined by the effective cutoff.

Note: The continuous lines are local linear regression estimates of an indicator for entry in CS in the year indicated on the top of each panel on distance to cutoff in that year. The bandwidth is set to 8. Circles in figures represent the mean outcome by cell within intervals of 2-points of distance to cutoff. The kernel used is Epanechnikov.

points of each cutoff point, which means that there are 21 bins in total (11 to the left, and 10 to the right of the cutoff). The lines in each figure are local linear regressions estimates of an indicator of participation in CS on the distance to the effective cutoff, run separately for eligible ($CAS_{ijmt} - \overline{CAS}_{mt} \leq 0$) and ineligible ($CAS_{ijmt} - \overline{CAS}_{mt} > 0$) families (we use a bandwidth equal to 8). In each year, there is a clear discontinuity in participation in CS around the (normalized) cutoff. This means that program eligibility is a strong predictor of program participation.³²

Table 2 complements these figures, by showing estimates of equation (4), where the outcome variable (Y_{imt}) is an indicator for CS participation, and $f(CAS_{imt-k} - \overline{CAS}_{mt-k})$ is a quadratic polynomial in its argument. In addition, we include municipality fixed effects, and run separate regressions for each year, so the variation we use is within municipality and year. There are 5 panels in the table, one for each cohort (2002-2006). For each cohort we present two columns. The first one shows our estimate of the impact of eligibility on participation, where \overline{CAS}_{mt-k} is the *effective* cutoff. The second shows the same estimate when we use the *official* cutoff for each municipality (see figure B.5 for correspondent nonparametric estimates).

The discontinuities in the proportion of families enrolled in CS around the *effective* eligibility cutoff are large and statistically significant, ranging from 0.12 in 2002, to 0.22 to 2006. The

³²Since we showed in section 3 that not all municipalities in Chile contributed to the identification of the effects of CS, in figure B.6 in Appendix presents estimates for γ from the first stage equation $CS_{imk} = \phi + \gamma E_{im} + f(CAS_{im} - \overline{CAS}_m) + u_{imk}$ estimated separately for each municipality. For simplicity, all cohorts (2002-2006) are pooled together. It is evident from this graph that for about 3/4 of the municipalities the size of the first stage it is at least 0.1.

discontinuities around the *official* cutoff are statistically significant, but much smaller in magnitude. Figure B.5 which represents participation in CS as a function of distance to the official cutoff for different cohorts, also shows that for the first years of the operation of CS, eligibility as determined by *official* cutoff is a worse predictor of participation than eligibility determined by the *effective* cutoff. The bottom row of table 2 presents the F-statistic on the eligibility coefficients. It shows that eligibility defined by the adjusted cutoffs is associated with a higher F-statistic than when eligibility is defined by the official cutoffs.

Table 2: First Stage Estimates.

Year of entry Cutoff	(1)	2002		2003		2004		2005		2006	
		Effective	Official	Effective	Official	Effective	Official	Effective	Official	Effective	Official
1[Eligible]	0.121*** (0.008)	0.018*** (0.003)	0.140*** (0.007)	0.058*** (0.007)	0.153*** (0.008)	0.087*** (0.008)	0.166*** (0.009)	0.101*** (0.010)	0.217*** (0.012)	0.122*** (0.012)	
Observations	229,804	326,324	267,339	315,641	264,205	296,529	256,517	276,733	96,830	103,199	
Mean	0.057	0.029	0.064	0.044	0.057	0.049	0.065	0.064	0.061	0.075	
SD	0.232	0.167	0.244	0.206	0.232	0.216	0.246	0.244	0.239	0.263	
Test on Eligibility											
F	99.54	13.92	175.7	42.23	160.5	48.16	135.3	54.70	130.5	59.81	

Note: The dependent variable is an indicator that takes value 1 if the family started CS in a given and 0 otherwise (for the years of 2003, 2004, 2005 and 2006 entrants in previous years have missing in the dependent variable since entrants in the previous years cannot re-enrol in the intensive phase). Controls excluded from table include quadratic in distance to cutoff, their interaction with eligibility to CS and municipality-year of residence effects. We present estimates using both the effective and the official cutoff, and the variable distance to cutoff is defined as the difference between the CAS-score of the family and the effective or official cutoff. Robust standard errors are reported in brackets clustered at municipality of residence when eligibility is evaluated. * significant at 10%; ** significant at 5%; *** significant at 1%.

5.2 Intent-to-Treat Estimates of Program Impacts

We start by presenting intent-to-treat (ITT) estimates, which can be read directly from figures showing outcomes as a function of the distance to the municipality's cutoff. We investigate three groups of outcomes for which we have information in the Ficha CAS and the FPS: the take-up of subsidies and of employment programs, labor market outcomes, and housing conditions. We estimate program impacts measured 2 and 4 years after a family first enrolled in the program (in the Appendix we also present impacts measured 6 years after program enrolment, which can only be calculated for families that (potentially) started the program in 2002 or 2003). All variable definitions are given in table A.5 in the appendix.

Figure 5.2 shows estimates of the relationship between outcomes and the distance to the municipality and cohort specific cutoffs. The vertical line shows the point in the x-axis where this distance is equal to zero, i.e., the point of discontinuity. Outcomes are measured two years after potential program enrollment (figure B.7 in Appendix presents similar figures but for outcomes measured four years after potential entry). The dots in the figures correspond to cell means for the outcomes after we divide the sample according to CAS scores into bins of size 2. The lines in the figures are local linear regressions estimates of the outcomes on the distance to the effective cutoff, separately for eligible and ineligible families.³³ The figure also include 95% confidence intervals.

Figure 5.2 suggests that there is an increase in the take-up of SUF (child allowance) and SAP (water subsidy) at the eligibility cutoff. We show below that the regression estimates are statistically significant for SUF³⁴, even though there is overlap in the pointwise confidence intervals on each side of the discontinuity. The next panel in the figure concerns the take-up of employment programs by FOSIS, by either the head of the household or the spouse. They show an increase in the probability of participation in the employment programs at the cutoff, which is stronger for the head than for the spouse. In the regressions below we can reject the null hypothesis that these impacts are equal to zero. The third row concerns the probability of legal ownership of the house and access water from public network, and, the bottom panel concerns the employment status of head and spouse. We do not find any statistically differences on average outcomes on either side of the cutoff for these four outcomes.

Table 3 summarizes our results. The first column of table 3 shows the control-mean, that is, the mean of the variable considered for the sample of just ineligible families (with CAS score at most 4 points above the cutoff). The next 8 columns present estimates and sample size for the samples used to study effects of two (columns 2-5) and four (columns 6-9) years of exposure. Columns (2)

³³We use a bandwidth equal to 8. We have also tried using bandwidths equal to 6 and 10, which resulted in fairly similar figures, see figure B.8 in the Appendix.

³⁴The sample used for the impacts on SUF conditions on the presence of children in family before 2001, since poor families with children are the target of this subsidy.

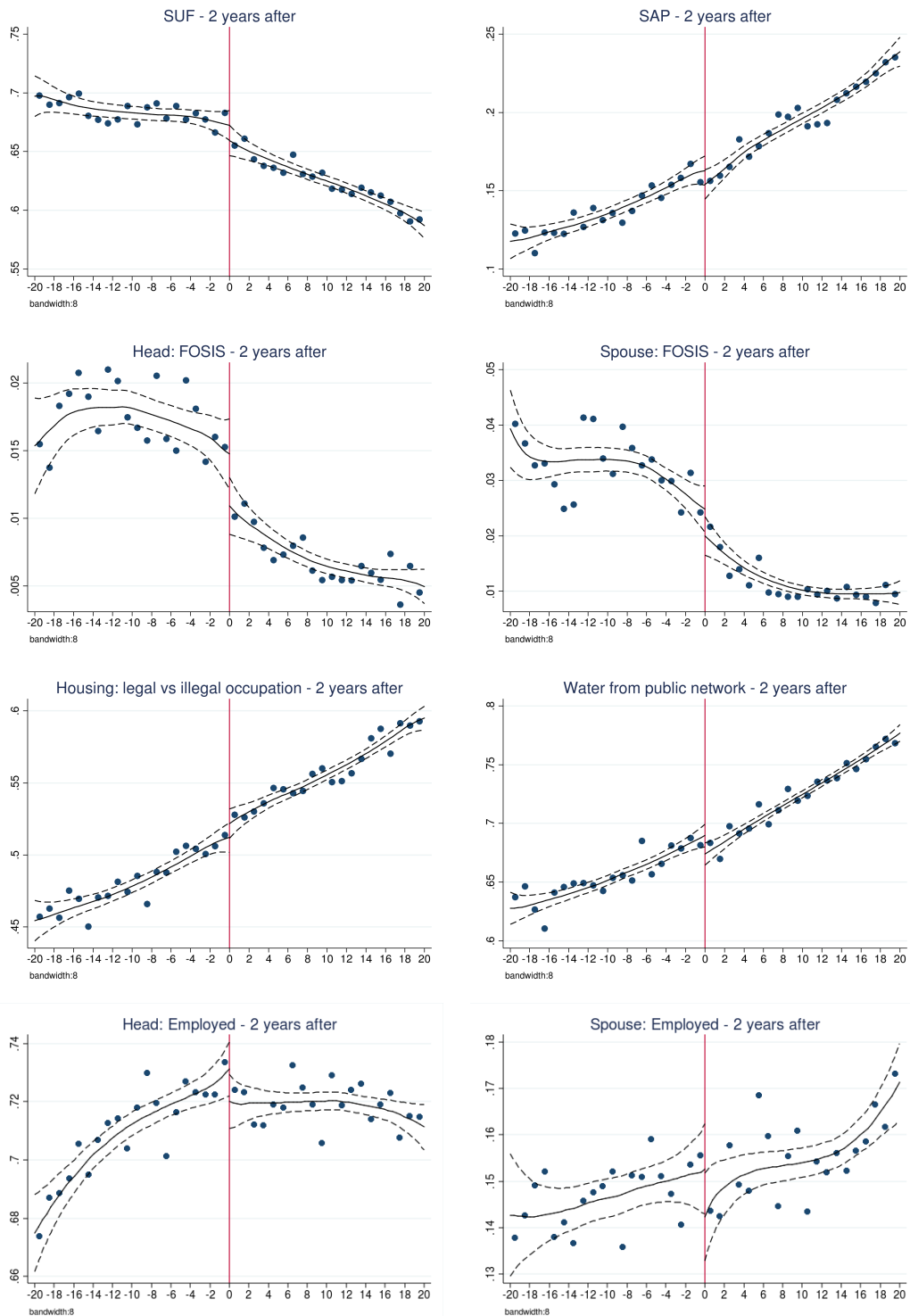


Figure 3: Average outcomes by eligibility status 2 years into the program, Bandwidth = 8. Note: The continuous lines in figure present local linear regression estimates of several outcomes on percentage distance to cutoff. Circles in figures represent the mean outcome by cell within intervals of 2 points of distance to cutoff. The kernel used is Epanechnikov.

and (6) include the number of observations used in estimates of columns (3) and (4), and (7) and (8), respectively.³⁵ Columns (4) and (8) show estimates for γ in equation 4 measured 2 years after program enrolment (short run) and 4 years after enrolment (medium run), respectively. Columns (5) and (9) include the years of data used in the estimation.³⁶ Columns (3) and (7) show the results when the outcomes are measured prior to the introduction of CS.

Columns (4) and (8) of table 3 show that the strongest impacts of CS are on the take up of SUF, and of FOSIS (employment) programs for the spouse of the head. Although there are positive impact estimates in several other outcomes, they are not statistically different from zero once we adjust for multiple hypothesis testing. Furthermore, the positive impacts on the take-up of SUF and FOSIS programs are only statistically important in the short run on the whole sample (column 4). Below we show that, for disadvantaged families, these impacts are sustained in the medium run. Separating program impacts by type of family, according to their pre-program conditions, is important according to the logic of intervention (see section 5.5).

It is reasonable that the strongest impacts are on the take-up of subsidies and social services, during the first two years, when home visits are in place. During the visits, the social worker should provide information about the services and subsidies each family is entitled to, how they can benefit the family, and at the same time, help them register for these programs. Thus, CS was able to contribute to its main goal, which bridging the social exclusion of very poor families to the welfare system available to them. It is also natural to find that impacts on employment or housing are smaller, since these require more substantial shifts in individual behavior, availability and use of other social programs and, perhaps, take longer to materialize.

5.3 Balancing Checks

We now assess the balance in the observable characteristics of individuals on each side of the cut-off. To do so, we estimate equation (4) using as dependent variables pre-determined characteristics that should not be affected by the program (see Lee and Lemieux, 2010).³⁷

Columns (3) and (7) of table 3 (labelled 'placebo') show the results when the outcome of interest are pre-determined variables, measured prior to the implementation of CS in 2000 (or 2001 if the family has no information for 2000). There is no statistically significant estimate in

³⁵When we estimate longer-run impact estimates our sample size becomes smaller, because we only have data up to 2009. However, our results for the longer-run impacts are not driven by sample selection, since the two-year effects are similar if we restrict the sample to those families to whom we can estimate both two and four year impacts (results available from the authors).

³⁶Table A.6 in Appendix is a version of table 3, including the t-statistic unadjusted for multiple hypotheses testing.

³⁷The balancing test is a necessary and sufficient condition for validity of strategy. In the case of estimated cutoffs the McCrary test for the continuity of the running variable density function at the threshold may be invalid (Porter and Yu, 2014).

Table 3: ITT estimates and balancing tests for the whole sample.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Years after start	2					4			
	C. Mean	N	Placebo	ITT	Sample	N	Placebo	ITT	Sample
Participation									
SUF	0.648	116,163	0.000 (0.009)	0.023*** (0.007)	2004-2008	66,171	-0.005 (0.012)	0.017 (0.011)	2006-2008
SAP	0.166	129,967	0.007 (0.005)	0.002 (0.006)	2004-2006	19,475	0.029 (0.016)	0.013 (0.014)	2006
Labor Market Programs (FOSIS)									
By head	0.010	192,810		0.004 (0.002)	2004-2007	94,708		-0.001 (0.001)	2006-2007
By spouse	0.016	127,225		0.006** (0.003)	2004-2007	62,408		0.004 (0.002)	2006-2007
Labor market									
Employed (head)	0.717	198,464	0.012 (0.006)	0.009 (0.006)	2004-2008	165,689	0.010 (0.007)	0.001 (0.007)	2006-2009
Self-employed (head)	0.532	198,464	0.004 (0.006)	0.005 (0.006)	2004-2008	165,689	0.008 (0.007)	0.001 (0.007)	2006-2009
Dependent worker (head)	0.185	198,464	0.008 (0.005)	0.005 (0.005)	2004-2008	165,689	0.003 (0.005)	0.000 (0.007)	2006-2009
Formal Worker (head)	0.204			0.009 (0.011)	2007-2008			-0.006 (0.007)	2007-2009
Employed (spouse)	0.148	116,872	-0.007 (0.006)	0.003 (0.006)	2004-2008	73,751	0.001 (0.007)	0.010 (0.010)	2006-2009
Self-employed (spouse)	0.107	116,872	-0.005 (0.005)	0.003 (0.005)	2004-2008	73,751	0.004 (0.006)	0.006 (0.008)	2006-2009
Dependent worker (spouse)	0.041	116,872	-0.002 (0.003)	0.000 (0.004)	2004-2008	73,751	-0.003 (0.004)	0.004 (0.006)	2006-2009
Formal Worker (spouse)	0.078			-0.002 (0.010)	2007-2008			0.005 (0.007)	2007-2009
Housing									
Legal occupation of house	0.529	202,627	-0.004 (0.007)	-0.011 (0.006)	2004-2008	174,269	-0.001 (0.007)	-0.010 (0.007)	2006-2009
Sewage connected	0.370	214,136	0.007 (0.005)	0.005 (0.005)	2004-2006	41,893	0.012 (0.011)	0.012 (0.011)	2006
Water from public network	0.687	202,627	0.006 (0.005)	0.006 (0.005)	2004-2008	174,270	0.005 (0.005)	0.008 (0.005)	2006-2009
Adequate roof	0.403	141,943	0.009 (0.007)	0.005 (0.007)	2004-2006	31,850	-0.003 (0.014)	-0.017 (0.016)	2006
Adequate walls	0.279	141,943	0.009 (0.007)	0.002 (0.006)	2004-2006	31,850	-0.009 (0.015)	-0.016 (0.016)	2006
Heating	0.064	141,943	0.000 (0.003)	0.000 (0.004)	2004-2006	31,850	-0.000 (0.004)	-0.015 (0.009)	2006
Fridge	0.459	141,943	0.005 (0.007)	-0.002 (0.010)	2004-2006	31,850	-0.008 (0.013)	0.018 (0.015)	2006

Note: The table presents the estimated coefficients (and standard errors) on eligibility (measured 2 or 4 years before the outcome) for model 4. Controls excluded from table include quadratic in distance to cutoff, their interaction with eligibility to CS and municipality-year effects. The municipality of residence and distance to cutoff are measured when eligibility is evaluated. "C. Mean" is the control mean (mean of the outcome for the non-eligible at most 4-CAS points above the cutoff).

Robust standard errors are reported in parenthesis clustered at municipality of residence when eligibility is evaluated. * significant at 10%; ** significant at 5%; *** significant at 1% (the critical values for inference are adjusted for multiple hypotheses).

this table after accounting for multiple hypotheses testing, suggesting that individuals located just below and just above each cutoff are similar in terms of observable pre-determined variables. Thus, our empirical strategy is likely to be valid.³⁸

5.4 Instrumental Variables

We show in section 5.1 that eligibility to CS is a strong but imperfect predictor of participation in the program. At the cutoff, eligibility leads to an increase in the probability that a family participates in CS by 12 to 22 percentage points, depending on the year analyzed. We now present estimates for the impacts of program participation estimated by instrumental variables (IV).

Table 4 shows the IV estimates corresponding to the ITT estimates in table 3, for short (column 1) and medium run (column 2) effects of the program. We only present estimates for SUF and take-up of employment programs, where in table 3 we reject the null of no effect, and employment status.

The take-up of SUF by these families is fairly low, at 64.5%. We estimate that the probability that a family takes-up SUF increases by almost 11% two years after enrolment in CS. This is a large impact, but not enough to reach a 100% take-up rate. There are substantial program impacts on the take-up of SUF measured 4 years after enrolment in the program, but they are not statistically different from zero.

The mean participation in employment (FOSIS) programs among non-eligible households is very low (below 2%), both for the head of household and for the spouse. Relative to these values, the magnitudes of program impacts in the short run on the take-up of these programs is substantial: 2.3% for the head, and 3.9% for the spouse. Note that the take-up of employment programs is a lower bound estimate of the impact of the program, since participation in programs that exclusively target individuals in CS families is excluded from the definition and the supply of these programs increased over time since 2004. Impacts in the medium run are smaller and statistically insignificant, most likely due to the fact that the early cohorts were not exposed by the expansion of the supply side of programs.

For all other outcomes there are no statistically important impacts of CS on average (the IV estimates are available upon request). However, these average effects mask important heterogeneity of impact depending on the initial conditions, directly related with the personalized intervention the family and the social worker agree on, and the cohorts of potential entry (with the substantial

³⁸The different panels of figure B.9 in Appendix show this graphically. Although some of the graphs suggest that there may be differences in some variables, they are not statistically different from zero. Furthermore, one of the few outcomes for which we found program impacts was SUF, and for this variable we have perfect balance. The only outcome for which this validation exercise cannot be performed is participation in employment programs from FOSIS prior to 2002, which is not available before 2004.

Table 4: Impact of CS: IV estimates for the whole sample.

	(1)	(2)
Years after start	2	4
Participation		
SUF	0.110*** (0.033)	0.090 (0.058)
Labor market programs - FOSIS (head)	0.023** (0.008)	-0.004 (0.009)
Labor market programs - FOSIS (spouse)	0.039*** (0.012)	0.022 (0.014)
Labor market		
Head Employed	0.051 (0.029)	0.008 (0.037)
Spouse Employed	0.015 (0.029)	0.051 (0.051)

Note: The table presents the estimated coefficients (and standard errors) for the indicator of entry in CS 2 or 4 years before the time at which outcome is measured in model 5. Controls excluded from table include quadratic in distance to cutoff, their interaction with the CS indicator and municipality-year effects. The municipality of residence and distance to cutoff are measured when eligibility is evaluated.

Robust standard errors are reported in parenthesis clustered at municipality of residence when eligibility is evaluated. * significant at 10%; ** significant at 5%; *** significant at 1% (the critical values for inference are adjusted for multiple hypotheses).

reinforcement of the funding of CS), which we will explore in the next section.³⁹

5.5 Differential Impacts Across Groups

We analyze two dimensions of heterogeneity that are instrumental in understanding the pathway of impact. First, we examine whether there are differential impacts for individuals who were in a vulnerable situation before 2002. Second, we examine whether there are differential impacts for individuals entering the program in different years, comparing the effects for those that (potential) enter CS before vs. after the approval of the law that regulates the program in 2004 and expansion of the programs made available to CS participants.⁴⁰

³⁹There could also be heterogeneity in impacts due to unobservables, which we ignore in this paper, and which is much discussed in the literature (e.g., Imbens and Angrist, 1994, Heckman and Vytlačil, 2005).

⁴⁰We also studied two other sources of heterogeneity, but we opted to leave them out of the paper, since they are more speculative. One could argue that the availability of social services varies with the degree of remoteness of

Subsidies The first dimension of heterogeneity looks at whether the conditions were met at the onset of the program, before 2002. In particular, we examine the impacts of CS on SUF separately for families who took-up SUF in either 2000 or 2001 and for those who did not. The reason for doing this is that the take-up of SUF before the existence of CS shows that family members have knowledge of the existence of the subsidy and its availability to the family. Furthermore, they are able to access it. It is possible that CS does not substantially impact SUF take-up for these families, while at the same time it has a larger impact for those families for whom we have no past record of SUF take-up. The latter are more likely to be in a situation of isolation and exclusion from the welfare system.

Table 5 examines the impact of CS on the take-up of SUF for the two groups of families described above and it presents estimates from equations (4) and (5). The table includes two panels each with two sets of rows with the estimates for the short and medium run impacts: panel A presents estimates for those not receiving SUF before 2002, and panel B includes estimates for families who received it before 2002. Columns (1) and (2) show that CS has very high and sustained impacts on the take-up of SUF for those without prior take-up of this subsidy, but a much smaller impact on families who have taken up this subsidy before. The difference between the two groups is large and statistically significant. The p-value for the null hypothesis that the effect for those without SUF prior 2002 is equal to the effect for those that did not receive SUF prior to 2002 is 0.16 and 0 for 2 and 4 years of exposure, respectively. The take-up of SUF increases for the former group of families by 18 to 22% 2 and 4 years after program enrolment, respectively. These coefficients are statistically different from zero even after accounting for multiple hypothesis testing.⁴¹

We also examine whether there are any differences in program impacts between cohorts entering the program before and after 2004. In order to study this issue, we split the sample by cohorts that were differentially exposed to the increase in the supply side: the 2002-2004 cohorts and the 2005-2006 cohorts.

the local of residence. For example, families would incur in a higher transportation cost to access services. Thus, when we allow the effects to vary by urban and rural areas, we cannot reject the null of similar effects across areas in the take-up of subsidies (SUF), employment programs and labor market outcomes. We also allow the effect to vary by whether families live in municipalities where social workers are allocated more or less families (in particular, municipalities where on average social workers are allocated more or less than 50 families). Such sample split can be problematic, since municipalities that allocated more families to each social worker might also be those municipalities with more experienced and able social workers. Also, higher load of social worker correlate strongly with geographic remoteness, so a disaggregation by caseload is similar to a disaggregation by urban and rural areas. When we try such sample split we fail to detect any correlation between the effects we find and the caseload of social workers.

⁴¹There are virtually no impacts of CS on the take-up of SAP, regardless of whether families have taken up this subsidy before or not (see table A.7). We find a negative effect of CS on participation in SAP for those families that received it before 2002. This is a crowd out effect to which we cannot attribute a conclusive cause, but a strong possibility is measurement error in the determination of which families are eligible to SAP in the administrative records.

Table 5: Participation in SUF by initial conditions, for all cohorts pooled together (2002-2006) and separately (2002-2004 and 2005-2006).

	(1)	(2)	(3)	(4)
Years after start	2	4	2	
Cohort			2002-2004	2005-2006
Panel A: Not receiving SUF before 2002				
Eligibility (ITT)	0.034*** (0.013)	0.040** (0.018)	0.018 (0.015)	0.071*** (0.026)
Participation (IV)	0.180*** (0.064)	0.220** (0.096)	0.106 (0.085)	0.318*** (0.116)
Control Mean	0.494	0.541	0.481	0.524
Observations	50,605	28,306	35,277	15,328
P-Value: HA: $\beta_{0506}^{iv} > \beta_{0204}^{iv}$			0.060	
Panel B: Receiving SUF before 2002				
Eligibility (ITT)	0.019** (0.008)	0.002 (0.013)	0.020** (0.009)	0.016 (0.019)
Participation (IV)	0.091*** (0.035)	0.015 (0.067)	0.103** (0.047)	0.055 (0.065)
Control Mean	0.762	0.745	0.765	0.753
Observations	65,558	37,865	48,215	17,343
P-Value: HA: $\beta_{0506}^{iv} > \beta_{0204}^{iv}$			0.736	
P-Value: HA: $\beta_0^{iv} > \beta_1^{iv}$	0.164	0.000	0.496	0.008

Note: See table 3 for a description of specification used in rows (ITT) and see table 4 for the description of specification used in rows (IV). The coefficient estimate in rows (ITT) is the indicator of eligibility, E_{im} , and that in rows (IV) is the indicator of participation in CS, CS_{im} .

The p-values in panels A and B concern the null hypothesis that the effect for those that entered in CS in 2005-2006 equals the effect of those that entered in 2002-2004, $H_0: \beta_{0506}^{iv} = \beta_{0204}^{iv}$, against the alternative that the effect is larger for those entering in the later year, $H_A: \beta_{0506}^{iv} > \beta_{0204}^{iv}$.

The p-value in the last row tests the null hypothesis of the effect for those without SUF prior 2002 equals the effect on those receiving SUF, $H_0: \beta_0^{iv} = \beta_1^{iv}$, against the alternative that the effect is larger for those that did not receive SUF prior to 2002, $H_A: \beta_0^{iv} > \beta_1^{iv}$.

Robust standard errors are reported in parenthesis clustered at municipality of residence when eligibility is evaluated. * significant at 10%; ** significant at 5%; *** significant at 1% (critical values for inference are adjusted for multiple hypotheses testing).

Columns (3) and (4) of table 5 present estimates of the effect of the program two years into CS, by (potential) year of entry.⁴² Panel A shows a large impact on those families who have not taken up SUF before, suggesting that it is for them that the increase in supply is especially important (we reject the null that the estimates for the later cohort in Panel A are equal to those for the early cohort against the alternative hypothesis of a larger effect for the later cohort with a p-value of 0.06). Panel B shows no effect on take-up of SUF for entrants in the period of 2005-2006. Of course, we cannot also rule out the explanation that the program was just better implemented for later cohorts, as CS workers at various levels learned from the early years of implementation. The estimates for the early cohort are similar both for individuals who have taken-up SUF before and for those who have not (for the early cohort the p-value for the null hypothesis that the effect for those without SUF prior 2002 is equal to the effect for those that did not receive SUF prior to 2002 is 0.496).⁴³

Employment Programs We next re-examine the impacts of CS on the take-up of employment (FOSIS) programs. There is no information about the take-up of these programs before 2002, and therefore we cannot do an analysis similar to the one presented for SUF. However, one important question is whether CS induces a larger take-up of FOSIS programs for the population who needed them the most, i.e., for individuals who were not employed before 2002. To study this we divide individuals in the sample into two groups: for the unemployed in either 2000 or 2001 (Panel A) and for the employed (Panel B). Within each family we consider only heads and their spouses.

Table 6 has two sets of two columns: one set for heads (columns 1-2), and one set for spouses (columns 3-4). Panel A shows that CS has very large (relatively to the average value in the non-eligible sample) and statistically strong impacts on the take of FOSIS for individuals who were not employed before 2002. This is true for both heads and spouses, but only in the short run. Panel B shows no statistically significant effects on the participation in training programs among heads or spouses employed before 2002.⁴⁴

Labor Market Outcomes We now study whether CS affects the probabilities of being not employed, self employed, or employed by a third party (what we call wage worker), for individuals who were and were not employed prior to 2002. We focus on heads of household and their spouses. The results are presented in table 7 and we do not find any statistically significant impacts.

We also studied short and medium run program impacts on the employment of the head and

⁴²Since our data for SUF ends in 2008 it is not possible to present medium term estimates of program impacts for the 2005-2006 cohorts.

⁴³We cannot perform an analysis by cohorts to SAP since 2006 is the last year of data we have for this variable.

⁴⁴Table A.8 shows cohort differences for individuals with and without pre-program employment, but there are no statistically significant differences across cohorts.

Table 6: Participation in labor market programs (FOSIS), by initial conditions (cohorts 2002-2006).

Variable	(1)	(2)	(3)	(4)
Years after start	Head		Spouse	
	2	4	2	4
Panel A: Not employed before 2002				
Eligibility (ITT)	0.010*** (0.003)	0.004 (0.003)	0.009*** (0.003)	0.003 (0.003)
Participation (IV)	0.062*** (0.016)	0.024 (0.021)	0.050*** (0.013)	0.016 (0.015)
Control Mean	0.008	0.040	0.015	0.023
Observations	43,384	19,269	113,299	55,705
Panel B: Employed before 2002				
Eligibility (ITT)	0.002 (0.002)	-0.002 (0.002)	-0.014 (0.009)	0.009 (0.007)
Participation (IV)	0.014 (0.009)	-0.010 (0.010)	-0.068 (0.051)	0.071 (0.052)
Control Mean	0.010	0.017	0.028	0.121
Observations	149,426	75,439	13,926	6,703
P-Value: HA: $\beta_0^{iv} > \beta_1^{iv}$	0.000	0.000	0.000	0.808

Note: See table 3 for a description of specification used in rows (ITT) and see table 4 for the description of specification used in rows (IV). The coefficient estimate in rows (ITT) is the indicator of eligibility, E_{im} , and the coefficient estimate in rows (IV) is the indicator of participation in CS, CS_{im} .

The last row presents the p-value for the null hypothesis that the effect for those not employed prior 2002 equals the effect on those employed, $H_0: \beta_0^{iv} = \beta_1^{iv}$, against the alternative that the effect is larger for those not employed prior to 2002, $H_A: \beta_0^{iv} > \beta_1^{iv}$.

Robust standard errors are reported in parenthesis clustered at municipality of residence when eligibility is evaluated. * significant at 10%; ** significant at 5%; *** significant at 1% (the critical values for inference are adjusted for multiple hypotheses testing using the procedure in Algorithms 4.1 and 4.2 of Romano and Wolf, 2005).

Table 7: Impacts on Labor Market Outcomes (cohorts 2002-2006).

	(1)	(2)	(3)	(4)	(5)	(6)
Variable	Employed		Self-Employed		Wage worker	
Years after entry	2	4	2	4	2	4
Panel A: Head						
Panel A1: Not Employed before 2002						
Eligibility (ITT)	-0.002 (0.013)	-0.009 (0.016)	0.010 (0.012)	-0.014 (0.015)	-0.012 (0.007)	0.005 (0.010)
Participation (IV)	-0.005 (0.069)	-0.054 (0.097)	0.058 (0.062)	-0.082 (0.088)	-0.063 (0.035)	0.028 (0.057)
Control Mean	0.235		0.183		0.060	
Observations	43,269	31,395	43,269	31,395	43,269	31,395
Panel A2: Employed before 2002						
Eligibility (ITT)	-0.005 (0.005)	-0.004 (0.006)	-0.001 (0.006)	0.001 (0.008)	0.007 (0.006)	-0.005 (0.008)
Participation (IV)	0.027 (0.025)	-0.022 (0.034)	-0.007 (0.031)	0.006 (0.045)	0.035 (0.030)	-0.028 (0.043)
Control Mean	0.851		0.628		0.219	
Observations	155,195	134,294	155,195	134,294	155,195	134,294
P-Value: HA: $\beta_0^{iv} > \beta_1^{iv}$	0.272	0.524	0.252	0.797	0.960	0.191
Panel B: Spouse						
Panel B1: Not Employment before 2002						
Eligibility (ITT)	-0.004 (0.005)	0.010 (0.010)	0.004 (0.005)	0.006 (0.008)	0.001 (0.003)	0.004 (0.007)
Participation (IV)	0.022 (0.028)	0.057 (0.049)	0.017 (0.024)	0.038 (0.040)	0.004 (0.017)	0.019 (0.035)
Control Mean	0.116		0.082		0.032	
Observations	103,112	63,536	103,112	63,536	103,112	63,536
Panel B2 Not Employment before 2002						
Eligibility (ITT)	0.005 (0.025)	0.007 (0.032)	-0.002 (0.025)	0.003 (0.032)	0.007 (0.016)	0.005 (0.022)
Participation (IV)	0.014 (0.133)	0.021 (0.213)	-0.013 (0.132)	0.009 (0.209)	0.027 (0.086)	0.012 (0.148)
Control Mean	0.424		0.300		0.111	
Observations	13,746	10,201	13,746	10,201	13,746	10,201
P-Value: HA: $\beta_0^{iv} > \beta_1^{iv}$	0.516	0.62	0.412	0.188	0.604	0.680

Note: See table 3 for the specification used in rows (ITT) and table 4 for rows (IV). The coefficient estimate in rows (ITT) is the indicator of eligibility, E_{im} , and the coefficient estimate in rows (IV) is the indicator of participation in CS, CS_{im} . The p-value in the table concerns for the null hypothesis that the effect for those not employed prior 2002 equals the effect on those employed, $H_0: \beta_0^{iv} = \beta_1^{iv}$, against the alternative that the effect is larger for those not employed prior to 2002, HA: $\beta_0^{iv} > \beta_1^{iv}$.

Robust standard errors are reported in parenthesis clustered at municipality of residence when eligibility is evaluated. * significant at 10%; ** significant at 5%; *** significant at 1% (the critical values for inference are adjusted for multiple hypotheses testing using the procedure in Algorithms 4.1 and 4.2 of Romano and Wolf, 2005).

Table 8: Labor market outcomes, by cohorts and initial conditions 2 and 4 years after start.

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Head Employed				Spouse Employed			
	2		4		2		4	
Years after start Cohort	2002-2004	2005-2006	2002-2004	2005-2006	2002-2004	2005-2006	2002-2004	2005-2006
Panel A: All sample								
Eligibility (ITT)	0.008 (0.007)	0.014 (0.011)	-0.004 (0.008)	0.016 (0.013)	-0.004 (0.006)	0.037 (0.017)	0.002 (0.011)	0.040 (0.022)
Participation (IV)	0.046 (0.039)	0.061 (0.048)	-0.026 (0.051)	0.077 (0.061)	-0.022 (0.034)	0.166 (0.079)	0.010 (0.064)	0.180 (0.097)
Control Mean	0.720	0.709	0.737	0.707	0.119	0.277	0.242	0.273
P-Value: HA: $\beta_{0506}^{iv} > \beta_{0204}^{iv}$	0.408		0.100		0.016		0.068	
Panel B: Not employed before 2002								
Eligibility (ITT)	0.006 (0.015)	-0.026 (0.028)	-0.011 (0.017)	-0.005 (0.033)	-0.004 (0.006)	0.044** (0.019)	0.002 (0.011)	0.045 (0.023)
Participation (IV)	0.039 (0.086)	-0.127 (0.136)	-0.080 (0.119)	-0.025 (0.157)	-0.021 (0.032)	0.198** (0.084)	0.009 (0.061)	0.198 (0.099)
Control Mean	0.225	0.296	0.300	0.306	0.0875	0.236	0.202	0.236
P-Value: HA: $\beta_{0506}^{iv} > \beta_{0204}^{iv}$	0.836		0.376		0.020		0.060	
Panel C: Employed before 2002								
Eligibility (ITT)	0.002 (0.006)	0.013 (0.011)	-0.010 (0.007)	0.015 (0.015)	0.007 (0.028)	-0.004 (0.057)	0.008 (0.037)	0.006 (0.066)
Participation (IV)	0.009 (0.036)	0.058 (0.048)	-0.065 (0.046)	0.071 (0.069)	0.044 (0.173)	-0.023 (0.272)	0.074 (0.323)	0.032 (0.318)
Control Mean	0.863	0.808	0.839	0.799	0.379	0.506	0.513	0.488
P-Value: HA: $\beta_{0506}^{iv} > \beta_{0204}^{iv}$	0.208		0.060		0.556		0.552	
P-Value: HA: $\beta_0^{iv} > \beta_1^{iv}$	0.400	0.920	0.580	0.724	0.628	0.208	0.564	0.296

Note: See table 3 for a description of specification used in rows (ITT) and table 4 for rows (IV). The coefficient estimate in rows (ITT) is the indicator of eligibility, E_{im} , and the coefficient estimate in rows (IV) is the indicator of participation in CS, CS_{im} . The p-value in the table concerns the null hypothesis that the effect for those that entered in CS in the years of 2005-2006 equals the effect of those that entered between 200-2004, $H_0: \beta_{0506}^{iv} = \beta_{0204}^{iv}$, against the alternative that the effect is larger for those entering in the later year, $H_A: \beta_{0506}^{iv} > \beta_{0204}^{iv}$. The last row presents the p-value for the null hypothesis that the effect for those not employed prior 2002 equals the effect on those employed, $H_0: \beta_0^{iv} = \beta_1^{iv}$, against the alternative that the effect is larger for those not employed prior to 2002, $H_A: \beta_0^{iv} > \beta_1^{iv}$. Robust standard errors are reported in parenthesis clustered at municipality of residence when eligibility is evaluated. * significant at 10%; ** significant at 5%; *** significant at 1% (the critical values for inference are adjusted for multiple hypotheses testing).

spouse by cohort of entry (see table 8).⁴⁵ The only group for which we find a statistically important impact on employment (after accounting for multiple hypotheses testing) are spouses (who are females in 98% of the cases), who were not employed before 2002, and who (potentially) entered the program after 2004. We reject the null that the effects on early and later cohorts are the same (p-value is 0.02). This suggests that CS promotes the employment of married women. With an average participation of 30%, female labor force participation in Chile is low by Latin American standards, overall and among the poorest sections of the population. In this context, CS coupled with the support of employment programs could constitute an important avenue to improve female labor market participation. The expansion of access to child care might have enhanced these improvements, although there are no reasons to expect that the access to public day care centers exhibits a discontinuity at the effective cutoffs we estimate (Medrano, 2009).

Other Outcomes We also study program impacts on four other sets of outcomes: housing, education, health and variables associated with behaviors. The effects we find in most of these outcomes are not statistically significant (even without adjusting inference for multiple hypothesis testing).

There are seven variables related to housing conditions (collected from the CAS), related to connections to the water and sewage networks, ownership of the house, and quality of the walls and ceilings (see table A.9 in Appendix A). We also analyze nine variables (collected from the FPS) concerning school enrolment of children, health coverage of children and adults in the household, and participation in employment centers. The variables where we find a positive impact of participation in CS indicates that all adults aged 65 or above in the family have completed health check-ups (see table A.10 in Appendix A).

5.6 Additional Results and Sensitivity Analysis

In this last section we discuss how robust our results are to changes in the functional form used to model the running variable in our RD and IV estimators, to changes in the window of data we use around each discontinuity threshold, and to explicitly accounting for some dynamics of program participation in our RD estimator. All results discussed here refer to reduced form estimates taken two years after (potential) program entry.

Panel A of Table 9 shows robustness to the choice of functional form of running variable. We present ITT estimates for several specifications: basic (column 1), using cubic in distance to cutoff as running (column 2), the specifications of columns 1 and 2, but without interacting eligibility with distance to cutoff (columns 3 and 4), and using quadratic of CAS as running variable (column

⁴⁵We consider the overall sample (panel A), and also divide it depending on whether the individual was not employed before 2002 (panel B) or was employed (panel C) of table 8.

5). There are hardly any changes in our main results. Panel B of Table 9 shows the sensitivity of our results to using different windows of data around the discontinuity. We use six possible windows of data around the cutoff: 15-points, 20-points (our basic sample), 25-points, 30-point and 50-points. Our results are robust to the choice of window of data around the discontinuity points.

We also estimated models using different sets of fixed effects for the place of residence at potential entry (see Table A.11 in Appendix). Besides municipality-year effects, we also included separately municipality and year fixed effects, separately neighborhood (which is the location unit immediately below municipalities) and year fixed effects, and, finally, neighborhood-year effects. We find similar results regardless of whether we control for interactive, or whether we include only additive municipality and year effects, which suggests that municipality specific shocks are not likely to be correlated with how CS is rolled out across years. Estimates are also similar if we include neighborhood fixed effects.

In our main set of estimates we restrict the sample to those families who were present in the CAS system prior to the introduction of CS (in 2000 or 2001). This sample restriction is important because we need pre-program data for balancing checks, and to look at heterogeneous effects by pre-program conditions. When we consider the larger sample that results from relaxing this restriction, our results do not change substantially (see table A.12 in Appendix).

We also estimate the effects separately for the sample of families living in municipalities where the size of the estimate of ψ in equation (2) is high and for those where it is low. Our results are driven by the first set of municipalities.⁴⁶

In table A.14 in Appendix we show that our results are robust to an alternative method to obtain the eligibility cutoff (we use a split-sample approach suggested in Card et al., 2008).

6 Conclusion

Banerjee and Duflo (2007) and Duflo (2012) emphasize that, because those in extreme poverty spend so much time and effort with the most basic daily problems, they may have difficulty in making rational decisions on many important issues of their day to day lives. Therefore, what seems like unjustifiable chaos in their lives, and inability to get organized at the most basic levels (such as, for example, applying for social programs readily available to them), may just be a

⁴⁶We consider that a municipality has discontinuity in estimation of equation (2) if the estimate for ψ at the value of CAS-score that maximizes the R^2 of (2) is at least 0.1. Table A.13 in Appendix shows that our results are driven by the sample of families living in municipalities where the size of $\hat{\psi}$ at the cutoff score is greater than 0.1. Figure B.10 in Appendix shows values of CAS that determine the eligibility to CS in municipalities with a high and low discontinuity estimation of equation (2) is similar, however, the mass of families in municipalities with a discontinuity in estimation of equation (2) at least 0.1 is to the left to the mass of families in municipalities with low discontinuity.

Table 9: ITT Estimates: robustness to functional form and trimming around cutoff.

	(1)	(2)	(3)	(4)	(5)
Panel A: Functional Form	Basic	Distance Cubic	Distance: no interaction Quadratic	Cubic	CAS Quadratic
SUF	0.023*** (0.007)	0.013 (0.010)	0.022* (0.005)	0.023*** (0.006)	0.022*** (0.005)
SUF - no SUF before 2002	0.034*** (0.013)	0.030* (0.017)	0.029 (0.009)	0.032*** (0.011)	0.033*** (0.009)
FOSIS participation (head)	0.004 (0.002)	0.004* (0.002)	0.007*** (0.001)	0.005*** (0.001)	0.007*** (0.001)
FOSIS participation (head) - not empl. before 2002	0.010*** (0.003)	0.006 (0.004)	0.012*** (0.002)	0.011*** (0.003)	0.012*** (0.002)
FOSIS participation (spouse)	0.006* (0.003)	0.001 (0.003)	0.013*** (0.002)	0.009*** (0.002)	0.014*** (0.002)
FOSIS participation (spouse) - not empl. before 2002	0.009*** (0.003)	0.002 (0.003)	0.014*** (0.002)	0.011*** (0.002)	0.015*** (0.002)
Panel B: Trimming around cutoff (CAS points)	15	20	25	30	50
SUF	0.017** (0.008)	0.023*** (0.007)	0.014** (0.007)	0.017** (0.006)	0.031*** (0.006)
Observations	87,992	116,163	127,899	136,801	157,062
SUF - no SUF before 2002	0.027 (0.015)	0.034*** (0.013)	0.026** (0.012)	0.026** (0.011)	0.040*** (0.010)
Observations	37,900	50,605	55,544	59,302	68,425
FOSIS participation (head)	0.004* (0.002)	0.004 (0.002)	0.005* (0.002)	0.005*** (0.001)	0.007*** (0.001)
Observations	145,355	192,810	211,869	226,595	259,597
FOSIS part. (head) - not empl. before 2002	0.008 (0.003)	0.010*** (0.003)	0.011** (0.003)	0.011*** (0.003)	0.013*** (0.002)
Observations	32,346	43,384	47,631	51,087	58,576
FOSIS participation (spouse)	0.002 (0.003)	0.006** (0.003)	0.009** (0.002)	0.011*** (0.002)	0.012*** (0.002)
Observations	96,126	127,225	140,120	149,874	171,802
FOSIS part. (spouse) - not empl. before 2002	0.004 (0.003)	0.009*** (0.003)	0.011** (0.002)	0.012*** (0.002)	0.013*** (0.002)
Observations	85,783	113,299	124,834	133,566	153,159

Note: Panel A of the table presents the coefficient estimates (and standard errors) on eligibility from model 4. Controls excluded from column (1) include quadratic in distance to cutoff, their interaction with eligibility to CS and municipality-year effects; controls excluded from column (2) include a cubic in distance to cutoff and its interaction with eligibility to CS and municipality-year effects. Columns (3) and (4) have estimates with the same models as in columns (1) and (2), except that eligibility to CS is not interacted with distance to cutoff. The municipality is the municipality of residence when eligibility is evaluated. Column (5) presents the marginal effect on eligibility from estimating the following model

$$Y_{imk} = \phi + \gamma E_{im} + \zeta E_{im} CAS_{im} + \eta E_{im} CAS_{im}^2 + \rho CAS_{im} + \theta CAS_{im}^2 + u_{imk}.$$

Panel B presents the coefficient estimates (and standard errors) on eligibility from model 4 trimming the sample differently around the cutoff. Controls excluded include quadratic in distance to cutoff, their interaction with eligibility to CS and municipality-year effects.

Robust standard errors are reported in parenthesis clustered at municipality of residence when eligibility is evaluated. * significant at 10%; ** significant at 5%; *** significant at 1% (the critical values for inference are adjusted for multiple hypotheses testing using the procedure in Algorithms 4.1 and 4.2 of Romano and Wolf, 2005).

consequence of the circumstances they live in.

CS is a program that attempts to help the extreme poor make better basic decisions in their lives, not only through the provision of information, but also through personalized planning strategies designed by trained social workers who visit them for a period of two years. Participating households are guaranteed access to monetary transfers to provide income support in the short run. At the same time, there is a strong effort to make available to the poor all the social programs that can help their situation sustain their exit from poverty in the medium run.

CS is, in its conception, a very ambitious program. In its implementation, it prioritizes the establishment of stronger links between those who are in extreme poverty and the social welfare system that is designed to serve them. And it is in this dimension that the program is most successful. CS reached in extreme poverty through its targeting instrument and through its personalized counseling helped them activate the demand for monetary subsidies and social services, especially for those families who were most alienated from the system to start with. For those families who had not taken up SUF before CS came to existence, and who enrolled after 2004, there was an increase in SUF take-up by more than 30 percentage points, from a basis of only 52%. Analogously, the take-up of employment programs was mainly driven by individuals whose employment was harder to activate, namely spouses who were unemployed or outside the labor force at the onset of the program.

The impacts of CS on a broader set of outcomes such as employment or housing are much more limited, and generally not statistically different from zero on average.

One mechanism we explore in the analysis to explain the lack of average effects are the differences in supply side availability. Our results suggest the demand component of CS is only productive when the corresponding supply of social services is enhanced and reorganized: the employment effects for the spouses were detected only for those cohorts exposed to an expanded supply side. The housing programs, in contrast, were highly rationed throughout the intervention period of our analysis. The lessons learned from the experience of CS are relevant for many middle countries trying to linking up income support programs to interventions that strengthen the productive capacity of the poorest and more vulnerable households. The supply side response came as a result of the diagnostics and improved understanding of the needs of the target population during the two years of psychosocial support: many of the existing programs were not tailored to the profiles and skills of the most vulnerable and/or were not large enough to cover the demand. Supply side availability and access, however, may not be enough. A more intensive version of CS may be needed to achieve the required transformation, whereby the psychosocial support (or other forms of identification of the needs of the poorest) is combined with a stronger focus on the scope, composition and quality of programs targeted to this population.

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Appendix
*Tackling Social Exclusion: Evidence from
Chile*

A Tables

Table A.1: List of Minimum Conditions to be met by families.

		Applies to % fams	Fulfilled at Entry
	Identification		
I1	All family members registered in the Civil Registry.	N.A.	N.A.
I2	All members of family have an ID card.	N.A.	N.A.
I3	The family has CAS updated at the municipality of residence.	N.A.	N.A.
I4	All men over 18 have military situation sorted.	N.A.	N.A.
I5	All adult members of the family have regularized their bureaucracy, as appropriate.	N.A.	N.A.
I6	Individuals with a disability should have the disability certified by COMPIN (<i>Comisión Médica, Preventiva e Invalidez</i>) and registered in the National Disability Center.	N.A.	N.A.
	Health		
H1	Family service registered in the Primary Health Care.	100%	92%
H2	Pregnant women have their health checks updated.	30%	55%
H3	Children under 6 have their vaccinations updated.	64%	92%
H4	Children under age 6 have their health checks updated.	64%	91%
H5	Women 35 years and older have the Pap test updated.	85%	64%
H6	Women who use birth control are under medical supervision.	73%	76%
H7	Elderly are under medical supervision.	43%	71%
H8	All members of the family who have a chronic illness are under medical supervision.	56%	74%
H9	Family members with disabilities that can be rehabilitated participating in a rehabilitation program.	32%	46%
H10	Family members are informed on health and self-care.	100%	74%
	Education		
E1	Preschoolers attend a nursery school program.	51%	74%
E2	If mother works and there are no adults to take care of children, these should be in some form of child care.	45%	81%
E3	Children up to 15 years are attending an educational establishment.	76%	94%
E4	Children who attend preschool, primary or secondary, benefit from assistance programs appropriate school.	79%	86%
E5	Children over age 12 are literate.	68%	95%
E6	Children with disabilities able to study are incorporated into the educational system, regular/special.	32%	56%
E7	That there is an adult responsible for the child's education and that is in regular contact with the school.	82%	95%
E8	Adults have a responsible attitude towards education, recognizing the value of formal education.	87%	94%
E9	That adults are literate.	99%	86%
	Family Dynamics		
F1	Daily conversation about topics such as habits, times and places for recreation.	100%	86%
F2	The family has adequate mechanisms to deal with conflicts.	100%	80%
F3	That there are clear rules of coexistence within the family.	100%	85%
F4	Equitable distribution of household tasks (regardless of the sex and according to the age).	100%	85%
F5	Family knows about community resources and development programs.	100%	80%
F6	People involved in domestic violence are incorporated into a program of support.	45%	52%
F7	Families who have children in the protection system somewhere visit them regularly.	35%	39%
F8	Families with young members in the correctional system should support him/her.	34%	36%
	Housing		
C1	Family has its housing situation clarified regarding tenure of house and site in which they live.	100%	89%
C2	If the family wants to apply for housing, it should be doing it.	78%	40%
C3	Access to clean water.	100%	90%
C4	An adequate power system.	100%	83%
C5	They have a system of proper sewage disposal.	100%	73%
C6	That house is not raining, not flooded and is well sealed.	100%	36%
C7	That housing has at least two habitable rooms.	100%	67%
C8	That each family member has his bed with basic equipment (sheets, blankets, pillows).	100%	45%
C9	Basic equipment to feed the members (pots, pans, cutlery for all family members).	100%	75%
C10	They must have a proper system of garbage disposal.	100%	91%
C11	That the home environment is free from pollution.	100%	85%
C12	That the family has access to the subsidy payment of potable water consumption, if applicable.	57%	53%
	Labor Market		
L1	At least one adult family member works on a regular basis and have a stable salary.	99%	40%
L2	No child under 15 years drop out of school to work.	88%	97%
L3	That people who are unemployed are registered in the Municipal Information Office (OMIL).	82%	42%
	Income		
G1	That the members of families entitled to SUF have it (at least are applying to it).	90%	79%
G2	That family members entitled to Family Allowance (<i>Asignación Familiar</i>) have it.	68%	76%
G3	That family members entitled to PASIS (welfare pension) have it (at least are applying to it).	72%	71%
G4	The family has income above the poverty line.	100%	27%
G5	The family has a budget organized according to their resources and priority needs.	100%	64%

Table A.2: Labor market programs to which families have access.

Programs exclusively for CS families		
Job Placement Employment (dependent work)	FOSIS	Equip individuals so that they can take a job through: job training courses, services for placement in companies, and training and technical support after placement in the workplace.
Hiring bonus	SENCE	It subsidizes up to 50% of a minimum monthly wage per worker from CS families, for a period of up to six months. Contribution for optional training for companies contracting individuals for two or more months and for the costs of transportation.
Program for Training and Employment	CONAF	Training and employment program with actions in activities preferably related with agro-forestry or the development of local production.
Hiring bonus for youth	SENCE	For beneficiaries of CS between 18 and 29 years in a job. It subsidizes the hiring of workers contributing with 50% of the minimum wage for a period of between 1-4 months, renewable for 4 months. In addition, there is funding for job training.
Self-Employment Employment (independent work)	FOSIS	Contribute to develop an independent economic activity. Includes job training, remedial or upgrade skills; training and technical support for business plans, training and technical support for the marketing of goods and services.
Micro-enterprise	FOSIS	Support and financing for micro firms (fund management capital and support in the process of acquisition of assets, inputs and services).
Micro-enterprise for indigenous in urban areas	CONADI	Technical assistance, training and training support, under a self-managed participatory on issues related to production activities that generate their subsistence. The goal is to finance productive initiatives for indigenous beneficiaries of CS.
Support Program for Subsistence Production	FOSIS	To increase disposable income of rural families, through savings generated by food production. Includes: access to simple user-friendly technology simple; training in the use, management and repair of technologies; education to improve eating habits (basic information on food preparation and nutrition).
Employability Job Skills Job skills for women Youth Employment Support	SENCE PRODEMU FOSIS	Includes job readiness workshops and motivational work for users requiring employability skills. For participants in OMIL. Training program for women that includes developing job skills, both technical and occupational training. To help unemployed between 18-24 to improve their employability through a specific job placement plan and participation in public and private employment programs.
Program of preferential access to CS families		
Job Placement Employment (dependent work)	FOSIS	See Employment (dependent work).
Self-Employment Employment (independent work) Support for Economic Activities Program for Strengthening of Microenterprise Initiatives	FOSIS FOSIS FOSIS	See Employment (independent work). Includes: financing productive investment, providing specialized services (consulting and technical assistance, etc.), access to credit. The goal is to contribute to the consolidation of small units that were supported by the Support for Economic Activities.
Employability Job Skills Program Social Enterprise Support Program	FOSIS FOSIS	Oriented towards leveling studies and social training. To improve beneficiaries' employability, through certification of their capabilities. Support to initiatives that offer community service. Includes workshops and courses, which are certified by technical training centers, universities and other institutions. Each participant receives a financial contribution to purchase materials/tools that facilitate the delivery of service.

There are 5 institutions offering several employment programs to poor and indigent families in Chile: FOSIS, SENCE, CONAF, CONADI and PROMEDU. FOSIS stands for Fondo de Solidaridad e Inversión Social – Fund for Solidarity and Social Investment, which implements several programs in the areas of entrepreneurship, employment and social empowerment. SENCE (Servicio Nacional de Capacitación y Empleo – National Training and Employment Service) which is responsible for supervision and to establish job training programs. CONAF (Corporación Nacional Forestal – National Forest Corporation), which administers the forest policies. CONADI (Corporación Nacional de Desarrollo Indígena – National Indigenous Development Corporation) that promotes, coordinates and implements development plans that promote the development of indigenous peoples. Finally, PRODEMU (Fundación para la Promoción y Desarrollo de la Mujer – Foundation for the Promotion and Development of Women) helps vulnerable women develop competencies and skills that promote personal development and social and labor market integration.

Table A.3: Families contacted by the Puente program annually.

Year	Contacted	Not Participating	Participating	Interrupted
2002	43892	2149	38273	3470
2003	55015	2754	48154	4107
2004	52963	2433	47162	3368
2005	55407	2170	50701	2536
2006	51296	3112	46727	1457
Total	258573	12618	231017	14938
Total %	100.00%	4.90%	89.30%	5.80%
Total %	100.00%	4.90%	95.10%	

Note: Each year about 50,000 families were invited to participate in the system. Of these, on average, 4.9% did not participate because they refused or because it was not possible to locate the family. The rest, 95.1% started working with social workers. 5.5% of families contacted interrupted the process, either by decision of the family support, of the family or both. The rest, 89%, has participated regularly in the system. The program interruption occurs preferentially at 3-4 months of incorporation. Source: Raczynski, 2008.

Table A.4: Selection of families to CS.

	(1)	(2)	(3)	(4)	(5)
	Mean 2002	Within Municipality R2	Coeff/SE	Within Neighborhood R2	Coeff/SE
SAP	0.06	0.0474	-0.0034 (0.0061)	0.0897	-0.0116** (0.0055)
SUF	0.43	0.0713	0.1206*** (0.0041)	0.1197	0.1132*** (0.0039)
Housing					
Adequate walls	0.74	0.0504	-0.0660*** (0.0049)	0.0919	-0.0629*** (0.0035)
Adequate ceil	0.65	0.0523	-0.0785*** (0.0047)	0.0933	-0.0732*** (0.0033)
Legal occupation of house	0.29	0.0486	-0.0406*** (0.0040)	0.0906	-0.0425*** (0.0034)
Water from public network	0.74	0.0473	0.0064 (0.0082)	0.0892	-0.0063 (0.0051)
Sewage connected	0.25	0.0483	0.0055*** (0.0007)	0.0904	0.0061*** (0.0007)
Fridge	0.20	0.0473	0.0041 (0.0046)	0.0892	0.0028 (0.0042)
Heating	0.00	0.0474	-0.1021*** (0.0105)	0.0894	-0.0987*** (0.0100)
Electricity meter	0.36	0.0475	0.0074*** (0.0020)	0.0896	0.0111*** (0.0018)
CAS and Employment					
CAS	469.75	0.0707	-0.0041*** (0.0002)	0.1123	-0.0042*** (0.0002)
Head is employed	0.66	0.0505	0.0597*** (0.0036)	0.0918	0.0541*** (0.0033)
Spouse is employed	0.15	0.0585	0.0290*** (0.0041)	0.1127	0.0269*** (0.0038)
Imputed income	0.85	0.0530	0.0749*** (0.0032)	0.0974	0.0695*** (0.0033)
Demographics					
Age of head	44.21	0.0624	-0.0041*** (0.0001)	0.1020	-0.0038*** (0.0001)
Single headed	0.46	0.0557	-0.0925*** (0.0041)	0.0967	-0.0885*** (0.0039)
Male head	0.64	0.0488	-0.0418*** (0.0027)	0.0908	-0.0422*** (0.0025)
Years of schooling	4.78	0.0474	0.0017** (0.0007)	0.0893	0.0014** (0.0007)
Presence of children	0.66	0.0869	0.2094*** (0.0050)	0.1251	0.2024*** (0.0049)
Family Size	3.57	0.0864	0.0539*** (0.0012)	0.1257	0.0530*** (0.0011)
Family belongs to dominant ethnicity in neighborhood	0.87	0.0507	-0.0156** (0.0063)	0.1154	-0.0176*** (0.0059)
Rural	0.66	0.0474	-0.0188** (0.0088)	0.0892	-0.0006 (0.0110)
Minutes family takes to nearest health center	31.46	0.0543	0.0004*** (0.0001)	0.1065	0.0003*** (0.0001)

Note: The table includes univariate correlations of selected family and their neighborhood characteristics measured the first a family is observed in CAS between 2002 and 2005. Only families that are eligible to CS according to the official cutoff are included in the table. Columns 2-3 present correlations within municipality; columns 4-5 present correlations within neighborhood. Robust standard errors in parentheses, clustered by municipality of residence. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A.5: Definition of variables used.

Variables		Data available CAS	FPS	Associated minim cond	Sample
Participation					
SUF	1 if at any family member receives SUF. Conditions on presence of age eligible children in family. The sample used for the impacts on SUF conditions on the presence of children in family before 2001, since poor families with children are the target of this subsidy. An additional requirement for the eligibility to SUF is that the family is not receiving Asignacion Familiar, which is assigned to children whose parents have Social Security. We do not observe this requirement in our data, but tabulations from CASEN 2003 show that 87% of CS participants do not receive Asignacion Familiar. 1 if receives SAP. Conditions on supply of water from public network before 2002. ⁴⁷ Individual (head or spouse) participates in at least of FOSIS program (which are not exclusive for CHS families). In particular, we construct an indicator that takes value 1 if the individual participated in Empleo Extra, Apoyo a Actividades Económicas (AAE), Programa Fortalecimiento de Iniciativas Microempresariales (FIM), Competencias Laborales (PNCL) or Programa Apoyo a Emprendimientos Sociales (PES) (see table A.2).	2000-2006	2007-2008	G1	$t + 2, t + 4, t + 6$
SAP		2000-2006	external source	2004-2007.	$t + 2, t + 4$ $t + 2, t + 4$
FOSIS					
Labor market and Income					
Not employed	1 if the individual is retired, if he/she did not have any gainful activity in the three months prior to the survey or if he/she is a first time job seeker. An individual is defined as not employed if he/she is a first time job seeker, if it is not performing any gainful activity, if he/she has been unemployed for at least 3 months or it is retired. Only defined for employed individuals. Includes family workers that do not receive wage, independent workers, small agricultural producer, median or large agricultural or urban entrepreneurs. Family workers that do not receive wage, employer, independent worker, domestic workers. Only defined for employed individuals. Include urban and rural dependent workers and workers in the public or private sector. Individuals employed in the private and public sector or armed forces.	2000-2006	2007-2009	L1	$t + 2, t + 4, t + 6$
Self-employed		2000-2006			$t + 2, t + 4, t + 6$
Dependent		2000-2006	2007-2009		$t + 2, t + 4, t + 6$
Housing					
Legal occupation of house	Owns the place (fully paid and with or without overdue debts) or is a tenant (without overdue rents). The alternative is to simply occupy the place of current, with or without a possible threat of eviction. The family owns the place where she lives (fully paid and with or without overdue debts), or is a tenant (without overdue rents), uses for free (ie, used as counterpart to pay for services, owned by a family member, granted usufruct). The alternative is occupation irregular (ie, user who does not own it, do not pay for use and has no permission or consent of the owner for use). Owns the place (fully paid and with or without overdue debts).	2000-2006	2007-2009	C1	$t + 2, t + 4, t + 6$
Owner of house (if legal occupation)		2000-2006	2007-2009		$t + 2, t + 4$
Sewage connected	Sewage connected to network (either the house has or not is exclusive use). As opposed to toilets connected to septic tank, sanitary latrine, black hole (with exclusive use or not) or the housing does not have access to any sewage disposal system.	2000-2006		C5	$t + 2, t + 4$
Water from public network	The water comes from public or private drinking water (includes, in rural areas, the systems Rural Drinking Water). As opposed to well, pump, river. 1 if from public network with own meter, shared meter or without meter. As opposed to well or pump, river, another source (eg, from truck, lake).	2000-2006	2007-2009	C3	$t + 2, t + 4, t + 6$

Definition of variables used (cont.).

Variables		Data available CAS	FPS	Associated minim cond	Sample
Housing					
Adequate roof	The roof is made of material such shingle, tile or slate, with inner lining. As opposed to zinc or slate, without inner lining, phonolite, straw, broom sedge, reed, cane, based on elements such as cardboard, cans, bags.	2000-2006		C6	$t + 2, t + 4$
Adequate roof or mixed	The roof is made of material such shingle, tile or slate, with inner lining, or the ceil is composed of different materials, but at least part of it includes tiles with inner lining. As opposed to zinc or slate, without inner lining, phonolite, straw, broom sedge, reed, cane, based on elements such as cardboard, cans, bags.	2000-2006		C6	$t + 2, t + 4$
Adequate walls	The walls are made of brick/concrete blocks, masonry stone (with inner lining). As opposed to have all walls but at least one wall of the house is built with the previous materials mud thatch, drywall; to partition unlined; waste (as cardboard, cans, bags).	2000-2006		C6	$t + 2, t + 4$
Adequate walls or mixed	The walls are made of brick/concrete blocks, masonry stone (with inner lining); mud; or all but at least one wall of the house is built with the previous materials. As opposed to mud thatch, drywall; to partition unlined; waste (as cardboard, cans, bags).	2000-2006		C6	$t + 2, t + 4$
Water Heating	1 if the family owns boiler, which can be powered by gas, electricity, solar energy, kerosene or wood.	2000-2006		C6	$t + 2, t + 4$
Fridge	1 if family owns a refrigerator in good condition and that it is functioning.	2000-2006		C8	$t + 2, t + 4$
Other outcomes					
all children enrolled in school	Indicators of enrolment in school for all children in family in a given age group enrolled in school (6-11, 12-14, 15-18, 19-24). The first three groups are chosen according to the schedule of the Subvencion Escolar (school subsidy). All indigent children in grades 7-8 of basic level or in the four years of the medium level are eligible to Subvencion Escolar (regardless of passing grade or not).		2007-2009	E3	$t + 2, t + 4, t + 6$
all children in preschool age in preschool	Indicator of enrollment.		2007-2009	E1	$t + 2, t + 4, t + 6$
all children with controls by family (<8y)	Indicator for health check-ups.		2007-2009	H4	$t + 2, t + 4, t + 6$
all elderly with controls by family (≥ 65)	Indicator for health check-ups.		2007-2009	H7	$t + 2, t + 4, t + 6$
Someone in family had problems w/ alcohol/drugs			2007-2009		$t + 2, t + 4, t + 6$
Any training program	Any adult 18-65 in family attended a training program.		2007-2009		$t + 2, t + 4, t + 6$
OMIL	At least of the unemployed members of the family is registered at OMIL		2007-2009	L3	$t + 2, t + 4, t + 6$

Table A.6: ITT estimates and balancing tests for the whole sample.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Years after start	C. Mean	N	Placebo	2 ITT	t-stat	Sample	N	Placebo	4 ITT	t-stat	Sample
Participation											
SUF	0.648	116,163	0.000 (0.009)	0.023*** (0.007)	3.286	2004-2008	66,171	-0.005 (0.012)	0.017 (0.011)	1.545	2006-2008
SAP	0.166	129,967	0.007 (0.005)	0.002 (0.006)	0.333	2004-2006	19,475	0.029 (0.016)	0.013 (0.014)	0.929	2006
Labor Market Programs (FOSIS)											
By head	0.010	192,810		0.004 (0.002)	2.000	2004-2007	94,708		-0.001 (0.001)	-1.000	2006-2007
By spouse	0.016	127,225		0.006** (0.003)	2.000	2004-2007	62,408		0.004 (0.002)	2.000	2006-2007
Labor market Head											
Employed	0.717	198,464	0.012 (0.006)	0.009 (0.006)	1.500	2004-2008	165,689	0.010 (0.007)	0.001 (0.007)	0.143	2006-2009
Self-employed	0.532	198,464	0.004 (0.006)	0.005 (0.006)	0.833	2004-2008	165,689	0.008 (0.007)	0.001 (0.007)	0.143	2006-2009
Dependent worker	0.185	198,464	0.008 (0.005)	0.005 (0.005)	1.000	2004-2008	165,689	0.003 (0.005)	0.000 (0.007)	0.000	2006-2009
Formal Worker	0.204			0.009 (0.011)	0.818	2007-2008			-0.006 (0.007)	-0.857	2007-2009
Spouse Employed	0.148	116,872	-0.007 (0.006)	0.003 (0.006)	0.500	2004-2008	73,751	0.001 (0.007)	0.010 (0.010)	1.000	2006-2009
Self-employed	0.107	116,872	-0.005 (0.005)	0.003 (0.005)	0.600	2004-2008	73,751	0.004 (0.006)	0.006 (0.008)	0.750	2006-2009
Dependent worker	0.041	116,872	-0.002 (0.003)	0.000 (0.004)	0.000	2004-2008	73,751	-0.003 (0.004)	0.004 (0.006)	0.667	2006-2009
Formal Worker	0.078			-0.002 (0.010)	-0.200	2007-2008			0.005 (0.007)	0.714	2007-2009
Housing											
Legal occupation of house	0.529	202,627	-0.004 (0.007)	-0.011 (0.006)	-1.833	2004-2008	174,269	-0.001 (0.007)	-0.010 (0.007)	-1.429	2006-2009
Sewage connected	0.370	214,136	0.007 (0.005)	0.005 (0.005)	1.000	2004-2006	41,893	0.012 (0.011)	0.012 (0.011)	1.091	2006
Water from public network	0.687	202,627	0.006 (0.005)	0.006 (0.005)	1.200	2004-2008	174,270	0.005 (0.005)	0.008 (0.005)	1.600	2006-2009
Adequate roof	0.403	141,943	0.009 (0.007)	0.005 (0.007)	0.714	2004-2006	31,850	-0.003 (0.014)	-0.017 (0.016)	-1.063	2006
Adequate walls	0.279	141,943	0.009 (0.007)	0.002 (0.006)	0.333	2004-2006	31,850	-0.009 (0.015)	-0.016 (0.016)	-1.000	2006
Heating	0.064	141,943	0.000 (0.003)	0.000 (0.004)	0.000	2004-2006	31,850	-0.000 (0.004)	-0.015 (0.009)	-1.667	2006
Fridge	0.459	141,943	0.005 (0.007)	-0.002 (0.010)	-0.200	2004-2006	31,850	-0.008 (0.013)	0.018 (0.015)	1.200	2006

Note: The table presents the estimated coefficients (and standard errors) on eligibility (measured 2 or 4 years before the outcome) for model 4. Controls excluded from table include quadratic in distance to cutoff, their interaction with eligibility to CS and municipality-year effects. The municipality of residence and distance to cutoff are measured when eligibility is evaluated. "C. Mean" is the control mean (mean of the outcome for the non-eligible at most 4-CAS points above the cutoff).

Robust standard errors are reported in parenthesis clustered at municipality of residence when eligibility is evaluated. * significant at 10%; ** significant at 5%; *** significant at 1% (the critical values for inference are adjusted for multiple hypotheses - see Romano and Wolf, 2005).

Table A.7: Take-up of SAP, by initial conditions (cohorts 2002-2006).

	(1)	(2)
Years after start	2	4
Panel A: Not receiving SAP before 2002		
Eligibility (ITT)	0.009 (0.006)	-0.006 (0.011)
Participation (IV)	0.047 (0.032)	-0.044 (0.080)
Control Mean		0.094
Panel B: Receiving SAP before 2002		
Eligibility (ITT)	-0.064*** (0.022)	0.033 (0.070)
Participation (IV)	-0.301** (0.113)	0.261 (0.803)
Control Mean		0.639
P-Value: $H_A: \beta_0^{iv} > \beta_1^{iv}$.364	.598

Note: See table 3 for a description of specification used in rows named (ITT) and see table 4 for the description of specification used in rows named (IV). The coefficient estimate in rows (ITT) refers to the indicator of eligibility, E_{im} , whereas the coefficient estimate in rows (IV) refers to the indicator of participation in CS, CS_{im} . "C. Mean" in the mean of the outcome for those at most 4-CAS points above the cutoff.

The last row presents the p-value for the null hypothesis that the effect for those without SAP prior 2002 equals the effect on those receiving SAP, $H_0: \beta_0^{iv} = \beta_1^{iv}$, against the alternative that the effect is larger for those that did not receive SAP prior to 2002, $H_A: \beta_0^{iv} > \beta_1^{iv}$.

Robust standard errors are reported in parenthesis clustered at municipality of residence when eligibility is evaluated. * significant at 10%; ** significant at 5%; *** significant at 1% (the critical values for inference are adjusted for multiple hypotheses testing; see Romano and Wolf, 2005).

Table A.8: Participation in labor market programs (FOSIS), by cohorts and initial conditions 2 years after start.

	(1)	(2)	(3)	(4)
	Head		Spouse	
Cohort	2002-2004	2005-2006	2002-2004	2005-2006
Panel A: All sample				
Eligibility (ITT)	0.004* (0.002)	0.002 (0.003)	0.006 (0.003)	0.009 (0.005)
Participation (IV)	0.026** (0.011)	0.010 (0.012)	0.033** (0.017)	0.040 (0.021)
Observations	141,926	50,879	94,952	32,273
Control Mean	0.010	0.008	0.019	0.010
P-Value: HA: $\beta_{0506}^{iv} > \beta_{0204}^{iv}$	0.497		0.520	
Panel B: Not employed before 2002				
Eligibility (ITT)	0.012** (0.004)	0.007 (0.007)	0.008** (0.003)	0.012** (0.005)
Participation (IV)	0.073*** (0.022)	0.035 (0.036)	0.043** (0.017)	0.054** (0.023)
Observations	32,153	11,177	84,570	28,729
Control Mean	0.008	0.009	0.017	0.008
P-Value: HA: $\beta_{0506}^{iv} > \beta_{0204}^{iv}$	0.500		0.528	
Panel C: Employed before 2002				
Eligibility (ITT)	0.002 (0.002)	0.001 (0.003)	-0.012 (0.011)	-0.019 (0.013)
Participation (IV)	0.015 (0.013)	0.006 (0.012)	-0.077 (0.072)	-0.095 (0.069)
Observations	109,730	39,688	10,382	3,544
Control Mean	0.011	0.007	0.028	0.028
P-Value: HA: $\beta_{0506}^{iv} > \beta_{0204}^{iv}$	0.564		0.516	
P-Value: HA: $\beta_0^{iv} > \beta_1^{iv}$	0.512	0.508	0.468	0.480

Note: See table 3 for the specification used in rows (ITT) and see table 4 for the description of specification used in rows (IV). The sample used in columns (1) and (3) includes the years of 2004 to 2006, whereas the sample used in columns (2) and (4) includes only 2007 information.

The p-value in the table concerns the null hypothesis that the effect for those that entered in CS in the years of 2005-2006 equals the effect of those that entered between 200-2004, $H_0: \beta_{0506}^{iv} = \beta_{0204}^{iv}$, against the alternative that the effect is larger for those entering in the later year, $H_A: \beta_{0506}^{iv} > \beta_{0204}^{iv}$. The last row presents the p-value for the null hypothesis that the effect for those not employed prior 2002 equals the effect on those employed, $H_0: \beta_0^{iv} = \beta_1^{iv}$, against the alternative that the effect is larger for those not employed prior to 2002, $H_A: \beta_0^{iv} > \beta_1^{iv}$.

Robust standard errors are reported in parenthesis clustered at municipality of residence when eligibility is evaluated. * significant at 10%; ** significant at 5%; *** significant at 1% (the critical values for inference are adjusted for multiple hypotheses testing; see Romano and Wolf, 2005).

Table A.9: Impact of CS: ITT estimates for quality of housing.

	(1)	(2)	(3)	(4)	(5)
	Basic	Before 2002		Area	
		No	Yes	Urban	Rural
Panel A: Water Connection					
A.1: Water from Public Network	0.006 (0.005)	0.007 (0.008)	-0.001 (0.003)	-0.003 (0.003)	0.004 (0.009)
Control Mean	0.687	0.147	0.962	0.976	0.298
A.2: Water fetched to the house	-0.008* (0.004)	-0.001 (0.003)	-0.021 (0.014)	-0.001 (0.002)	-0.010 (0.009)
Control Mean	0.192	0.0793	0.645	0.0167	0.428
Panel B: Tenency					
House owner	-0.011 (0.006)	-0.009 (0.008)	-0.011 (0.008)	-0.008 (0.008)	-0.017 (0.010)
Control Mean	0.529	0.270	0.828	0.532	0.526
Panel C: Sewage Connection					
Sewage connected to network	0.005 (0.005)	0.002 (0.005)	0.003 (0.009)	0.006 (0.008)	-0.006 (0.004)
Control Mean	0.370	0.241	0.881	0.604	0.0404
Panel D: Quality of the walls					
D.1: Adequate walls	0.002 (0.006)	0.000 (0.007)	-0.004 (0.014)	0.012 (0.008)	-0.011 (0.010)
Control Mean	0.279	0.180	0.560	0.277	0.283
D.2: Adequate walls or mixed	0.001 (0.005)	0.005 (0.018)	0.000 (0.004)	-0.001 (0.006)	0.001 (0.007)
Control Mean	0.893	0.624	0.947	0.916	0.862
Panel E: Quality of the ceiling					
E.1: Adequate ceiling	0.005 (0.007)	0.007 (0.008)	-0.005 (0.011)	0.015 (0.010)	-0.009 (0.010)
Control Mean	0.403	0.253	0.668	0.451	0.341
E.2: Adequate ceiling or mixed	0.006 (0.006)	0.025 (0.015)	-0.004 (0.005)	0.009 (0.007)	0.003 (0.009)
Control Mean	0.828	0.540	0.922	0.848	0.802
Panel F: Electricity					
House with electricity	0.006 (0.007)	-0.000 (0.010)	0.006 (0.009)	0.013 (0.010)	-0.002 (0.011)
Control Mean	0.570	0.342	0.806	0.554	0.591
Panel G: House has at least 2 habitable rooms					
House owner	0.006 (0.004)	0.004 (0.026)	0.003 (0.003)	0.014 (0.007)	-0.004 (0.003)
Control Mean	0.943	0.681	0.968	0.915	0.979

Note: The table presents the coefficient estimates (and standard errors) on eligibility from model 4. Controls excluded from table include quadratic in distance to cutoff, their interaction with eligibility to CS and municipality-year effects. The municipality is the municipality of residence when eligibility is evaluated. "C. Mean" is the mean of the outcome for those at most 4-CAS points above the cutoff.

Robust standard errors are reported in parenthesis clustered at municipality of residence when eligibility is evaluated. * significant at 10%; ** significant at 5%; *** significant at 1% (the critical values for inference are adjusted for multiple hypotheses testing using the procedure in Algorithms 4.1 and 4.2 of Romano and Wolf, 2005).

Table A.10: Impact of CS: ITT and IV estimates for the whole sample (behaviors).

Years after start	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	C. Mean	N	ITT	IV	N	ITT	IV
All children in family with health check-ups (<8y)	0.992	16,657	0.001 (0.005)	0.004 (0.017)	38,266	-0.006** (0.003)	-0.027* (0.014)
All elderly in family with health check-ups (≥65)	0.640	15,901	0.043* (0.023)	0.302* (0.166)	36,151	0.025 (0.017)	0.183 (0.131)
Unemployed individuals enrolled in OMIL	0.311	3,848	0.005 (0.044)	0.033 (0.158)	9,268	0.022 (0.032)	0.114 (0.146)
All children in preschool age in preschool	0.484	5,891	0.003 (0.039)	-0.002 (0.147)	13,801	-0.000 (0.028)	-0.005 (0.138)
All children 6-11 enrolled in school	0.958	20,493	0.010 (0.009)	0.036 (0.032)	47,358	0.001 (0.007)	0.002 (0.030)
All children 12-14 enrolled in school	0.980	13,729	0.006 (0.006)	0.022 (0.023)	32,436	0.001 (0.004)	0.002 (0.018)
All children 15-18 enrolled in school	0.845	17,776	-0.006 (0.016)	-0.029 (0.055)	43,136	0.002 (0.009)	0.006 (0.043)
At least one indiv 19-24 in college	0.263	15,969	0.022 (0.022)	0.085 (0.090)	40,230	0.002 (0.014)	0.011 (0.062)
Someone in family had problems w/ alcohol/drugs	0.0342	60,751	0.007 (0.005)	0.032 (0.021)	142,745	0.004 (0.003)	0.017 (0.015)

Note: See table 3 for a description of specification used in columns (3) and (6) and see table 4 for the description of specification used in columns (4) and (7). "C. Mean" in the mean of the outcome for those at most 4-CAS points above the cutoff. Robust standard errors are reported in parenthesis clustered at municipality of residence when eligibility is evaluated. * significant at 10%; ** significant at 5%; *** significant at 1% (the critical values for inference are adjusted for multiple hypotheses testing using the procedure in Algorithms 4.1 and 4.2 of Romano and Wolf, 2005).

Table A.11: Impact of CS: Choice of Fixed Effects.

	(1)	(2)	(3)	(4)
Fixed effects	Basic	Municip Year	Neighb Year	Neighb-year
SUF	0.023*** (0.007)	0.023** (0.007)	0.026*** (0.008)	0.029*** (0.008)
Labor market progr. - FOSIS (head)	0.004 (0.002)	0.004 (0.002)	0.003 (0.002)	0.003 (0.002)
Labor market progr. - FOSIS (spouse)	0.006* (0.003)	0.006** (0.002)	0.005 (0.003)	0.006 (0.003)

Note: The table presents the coefficient estimates on eligibility from model 4 controlling for different location fixed effects when eligibility to CS is assessed. Column (1) is our basic specification, which controls for municipality-year effects. Column (2) controls separately for municipality and year fixed effects; column (3) includes separately for neighborhood and year fixed effects, and, column (4) controls for neighborhood-year fixed effects. Robust standard errors are reported in parenthesis clustered at municipality of residence when eligibility is evaluated. * significant at 10%; ** significant at 5%; *** significant at 1% (the critical values for inference are adjusted for multiple hypotheses testing).

Table A.12: Impact of CS not conditioning on presence in data prior to 2002.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years after start	2		2		4		4	
	Basic N	ITT	Not conditioning N	ITT	Basic N	ITT	Not conditioning N	ITT
SUF	116,163	0.023*** (0.007)	208,365	0.023*** (0.006)	66,171	0.017 (0.011)	115,940	0.026*** (0.008)
Labor market progr. - FOSIS (head)	192,810	0.004 (0.002)	304,725	0.005*** (0.001)	94,708	-0.001 (0.001)	137,998	0.002 (0.001)
Labor market progr. - FOSIS (spouse)	127,225	0.006** (0.003)	195,273	0.006*** (0.002)	62,408	0.004 (0.002)	90,802	0.002 (0.002)

Note: The table presents the coefficient estimates on eligibility from model 4. Controls excluded: quadratic in distance to cutoff, their interaction with eligibility to CS and municipality-year effects. The municipality is the municipality of residence when eligibility is evaluated. Robust standard errors are reported in parenthesis clustered at municipality of residence when eligibility is evaluated. * significant at 10%; ** significant at 5%; *** significant at 1% (the critical values for inference are adjusted for multiple hypotheses testing).

Table A.13: ITT Estimates: by size of discontinuity at the value of CAS-score that maximizes the R^2 of (2).

	(1)	(2)	(3)	(4)
	First Stage	SUF	Participation in FOSIS programs Head	Spouse
Panel A: Municipalities with discontinuity				
Eligibility (ITT)	0.302*** (0.010)	0.061*** (0.006)	0.009*** (0.001)	0.017*** (0.002)
Observations	95,494	95,494	157,712	103,141
Control Mean	0.242	0.669	0.014	0.027
Panel B: Municipalities without discontinuity				
Eligibility (ITT)	0.062*** (0.010)	0.027* (0.016)	0.004* (0.002)	0.008** (0.004)
Observations	20,669	20,669	35,098	24,084
Control Mean	0.063	0.656	0.008	0.011

Note: The table presents estimates for coefficient on eligibility from model 4. The effects are measured two (potential) years after entry in CS. Controls excluded are: quadratic in distance to cutoff, their interaction with eligibility to CS and municipality-year effects. The two panels in the table include estimates separately for those municipalities with (Panel A) and without (Panel B) discontinuity in estimation of equation 2. We consider that a municipality has discontinuity in estimation of equation (2) if the estimate for ψ at the value of CAS-score that maximizes the R^2 of (2) is at least 0.1.

Robust standard errors are reported in parenthesis clustered at municipality of residence when eligibility is evaluated. * significant at 10%; ** significant at 5%; *** significant at 1% (the critical values for inference are not adjusted for multiple hypotheses testing).

Table A.14: ITT Estimates: Cutoff obtained through split-sample.

	(1)	(2)	(3)
	SUF	Participation in FOSIS programs Head	Spouse
Eligibility (ITT)	0.030** (0.013)	0.002 (0.003)	0.008* (0.004)
Participation (IV)	0.131*** (0.050)	0.010 (0.013)	0.043** (0.018)
Observations	47,264	79,127	51,683

Note: The table presents estimates for the coefficients on eligibility and participation from models 4 and 5. The effects are measured two years after (potential) entry in CS. Controls excluded are: quadratic in distance to cutoff, their interaction with eligibility to CS and municipality-year effects. The estimates presented in this table are obtained in the following way. First, the *effective cutoff* for eligibility for each year between 2002 and 2006 and for each municipality is estimated as in equation 2. However, instead of using all families in a given year in a municipality with a valid CAS, only a random sample of 2/3 of families is used. For each municipality-year the cutoff is similar to that obtained using the whole sample (and it is available from the authors). Then, we use these cutoffs and estimate models 4 and 5 (which correspond to those presented in tables 3 and 4, respectively) but restricting the sample to those families not used to estimate the municipality-year cutoff relevant to assess the impacts of CS two years after (potential) entry. Robust standard errors are reported in parenthesis clustered at municipality of residence when eligibility is evaluated. * significant at 10%; ** significant at 5%; *** significant at 1% (the critical values for inference are not adjusted for multiple hypotheses testing).

B Figures

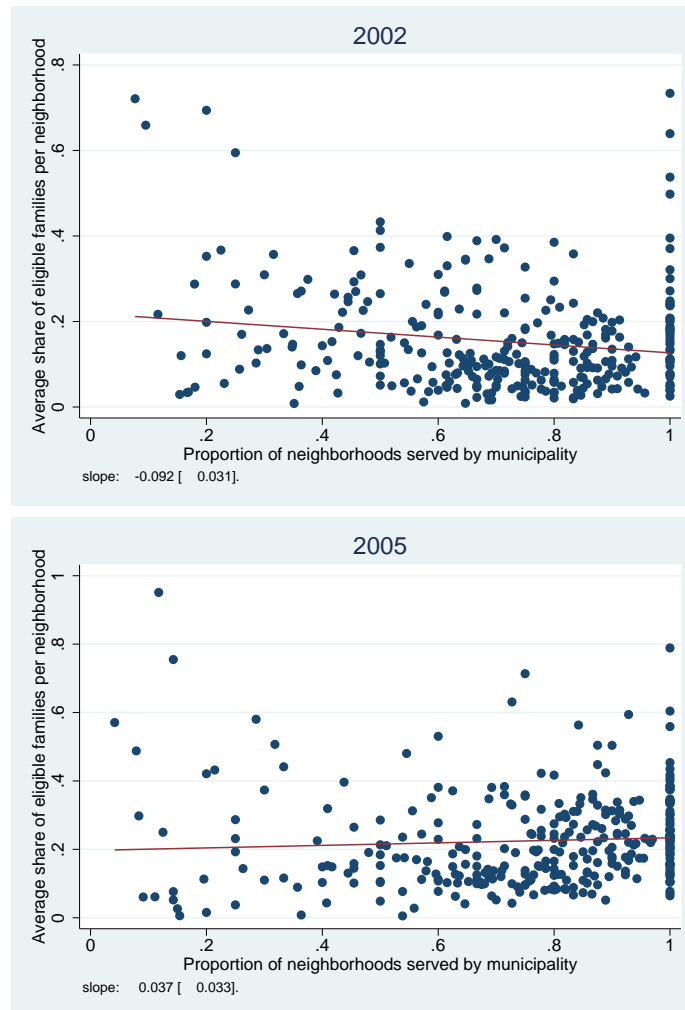


Figure B.1: Strategy of enrolment within municipalities in 2002 and 2005).
Note: The graphs present the relation between the average share of eligible families per neighborhood (per municipality) and the proportion of neighborhoods served in a municipality, in 2002 and in 2005. Each dot in the figures corresponds to a municipality. The line is the regression fit of the average share of eligible families per neighborhood (per municipality) on the proportion of neighborhoods served in a municipality. The coefficient estimates and standard errors are presented in the bottom of each graph.

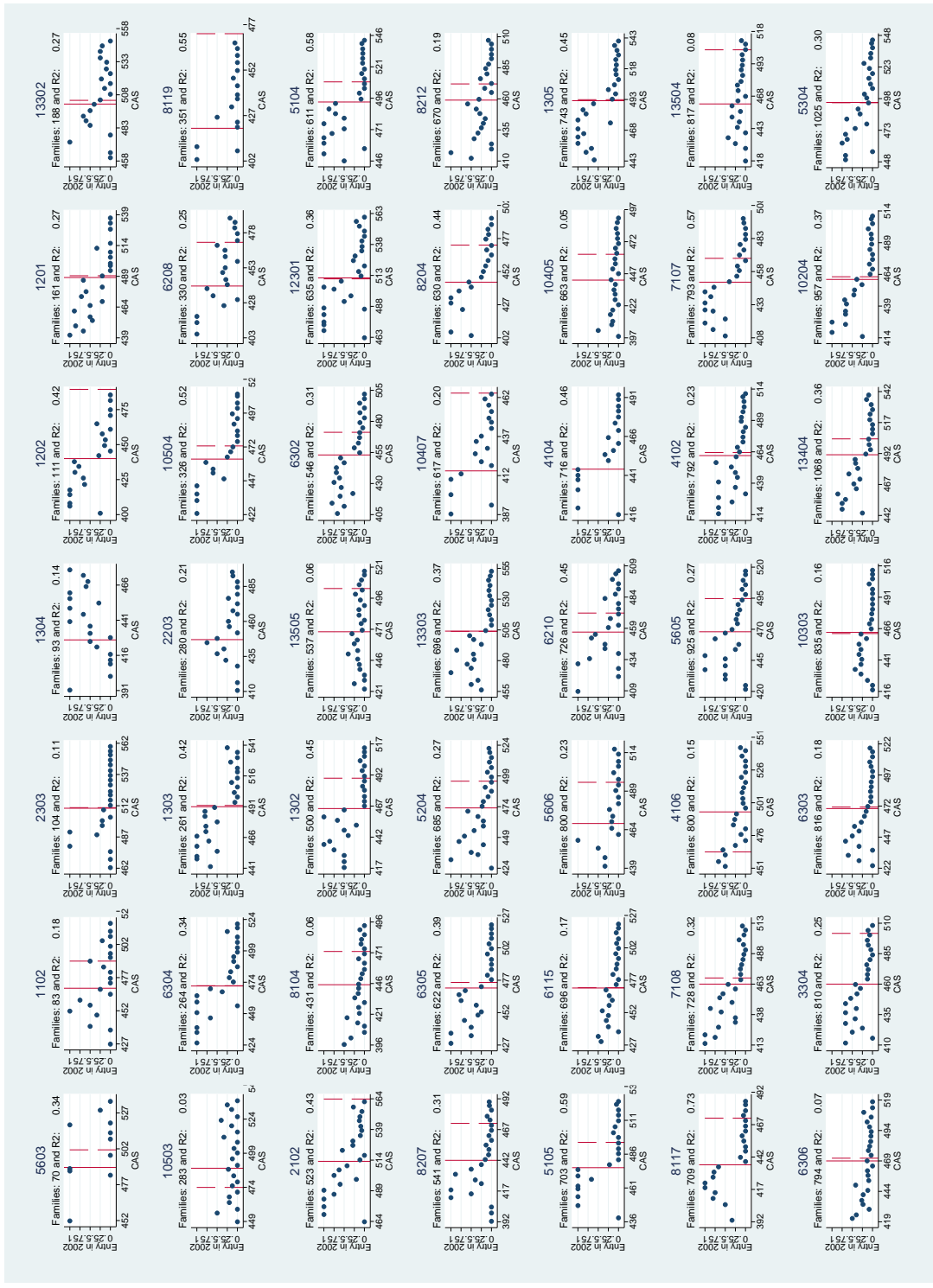
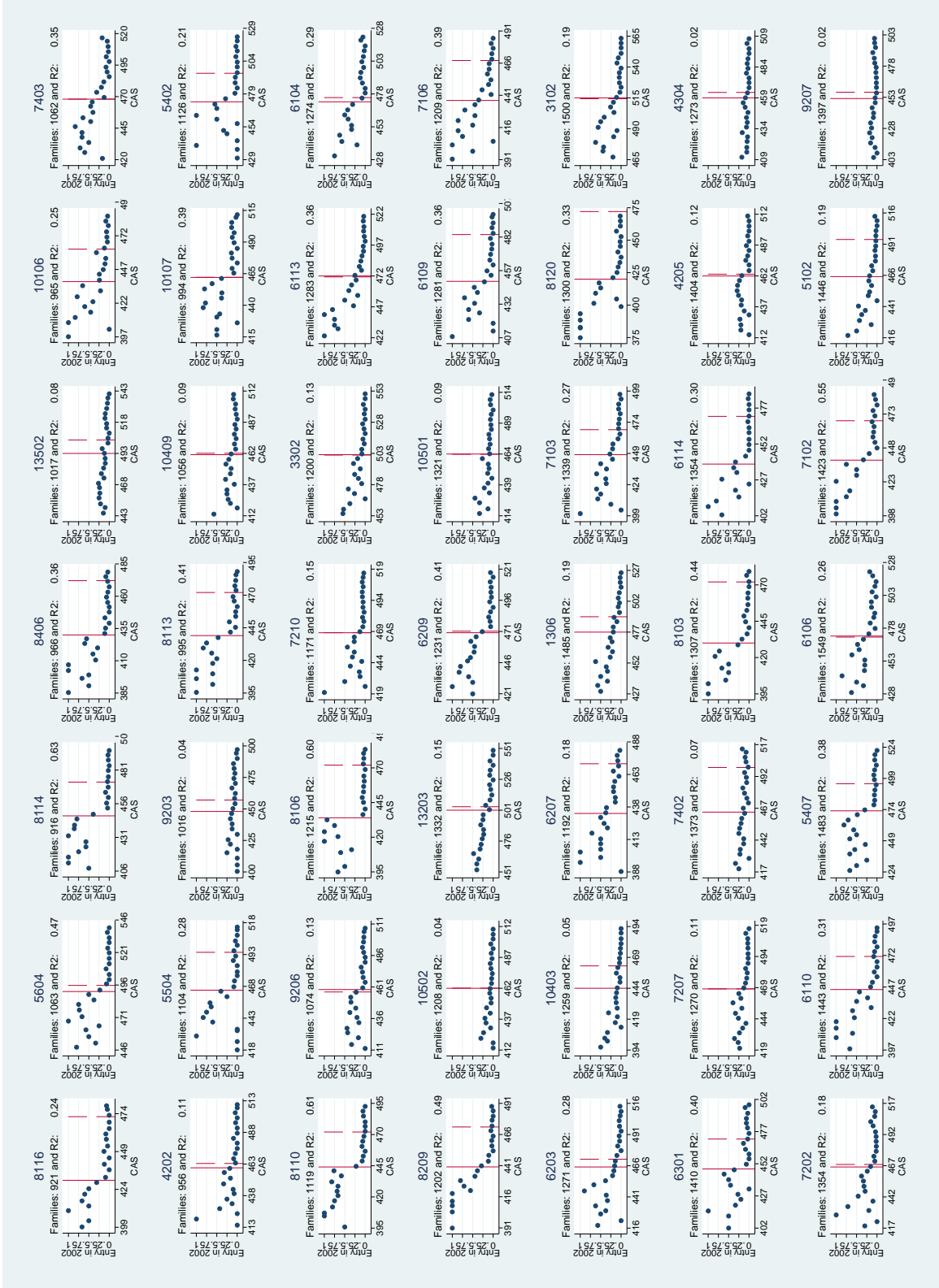
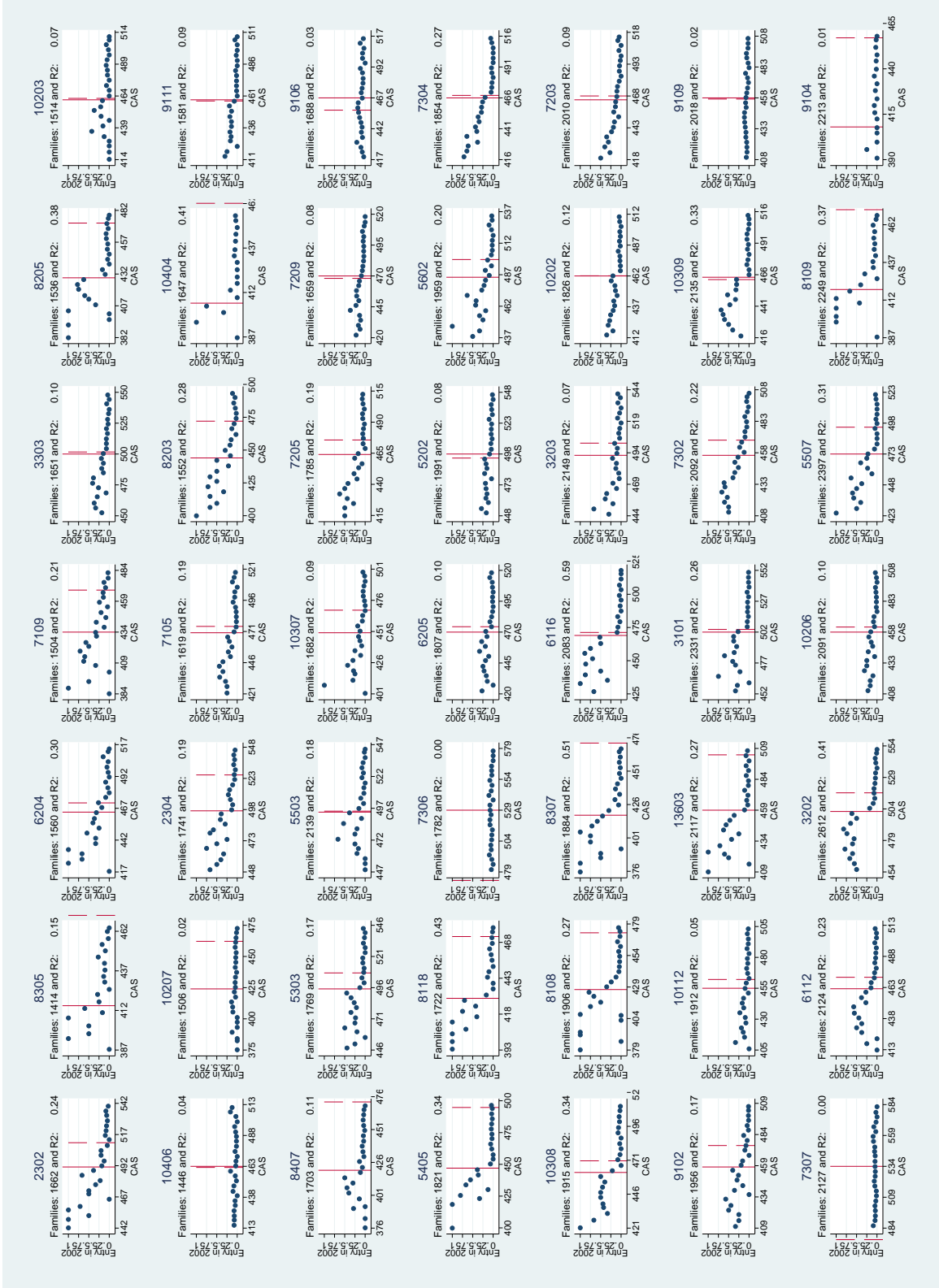


Figure B.2: Participation in CS and *effective cutoff* in illustrative municipalities (2002).

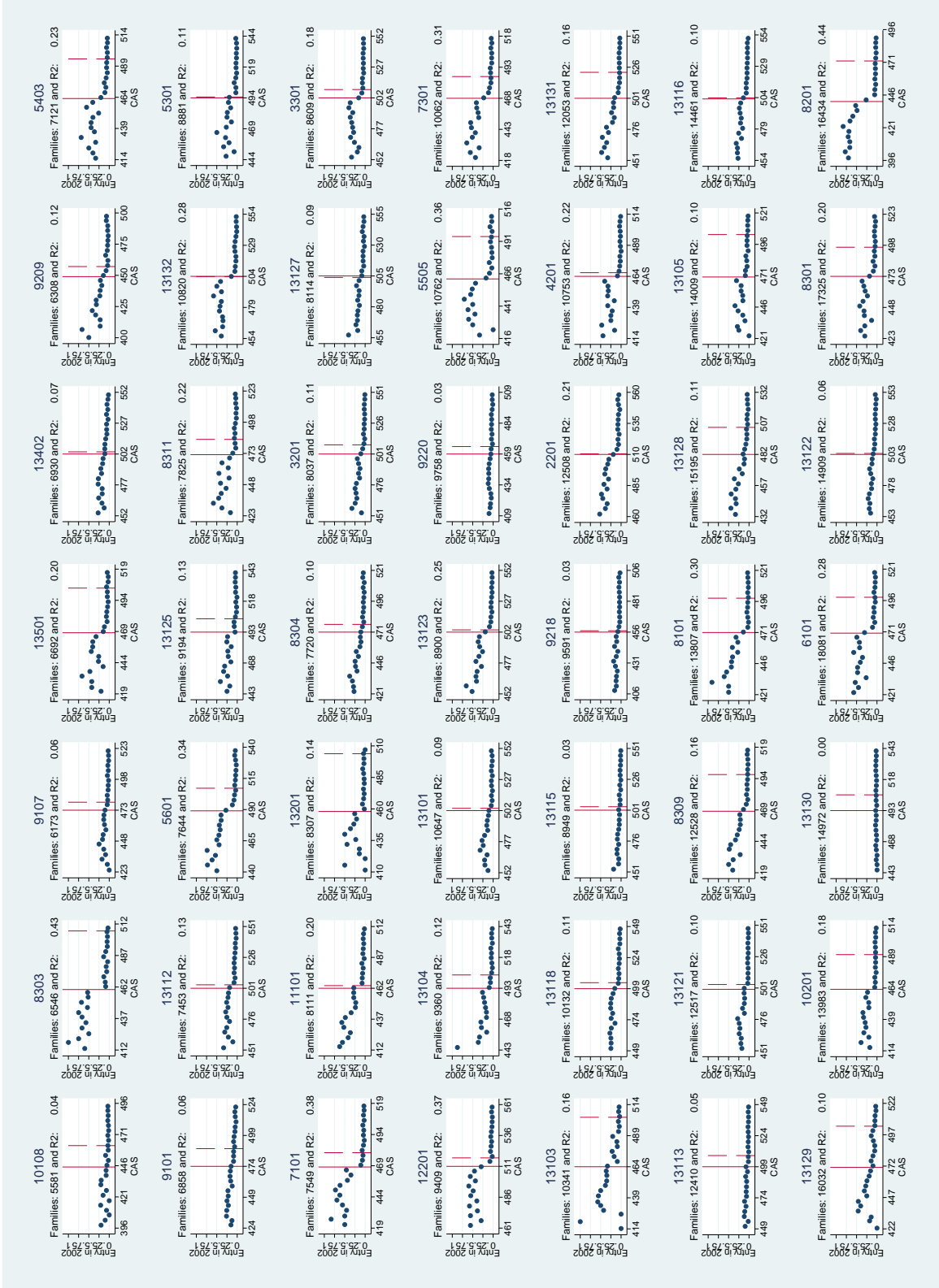
Note: The dots on the graphs are the proportion of families in municipality entering in CS in each year (see y-axis) by intervals of CAS score of 4 points in distribution of CAS in 2002 in each municipality. The solid vertical line represents the point identified as *effective cutoff* and the dashed line is the *official cutoff*. Where only one line is shown, the two coincide. The graphs in the figure are ordered by increase size of municipality of residence in 2002. The R^2 in the top of each graph is the *effective cutoff*, so that only families at most 40-points apart from it are depicted (the CAS score varies between 380 and 770 points).



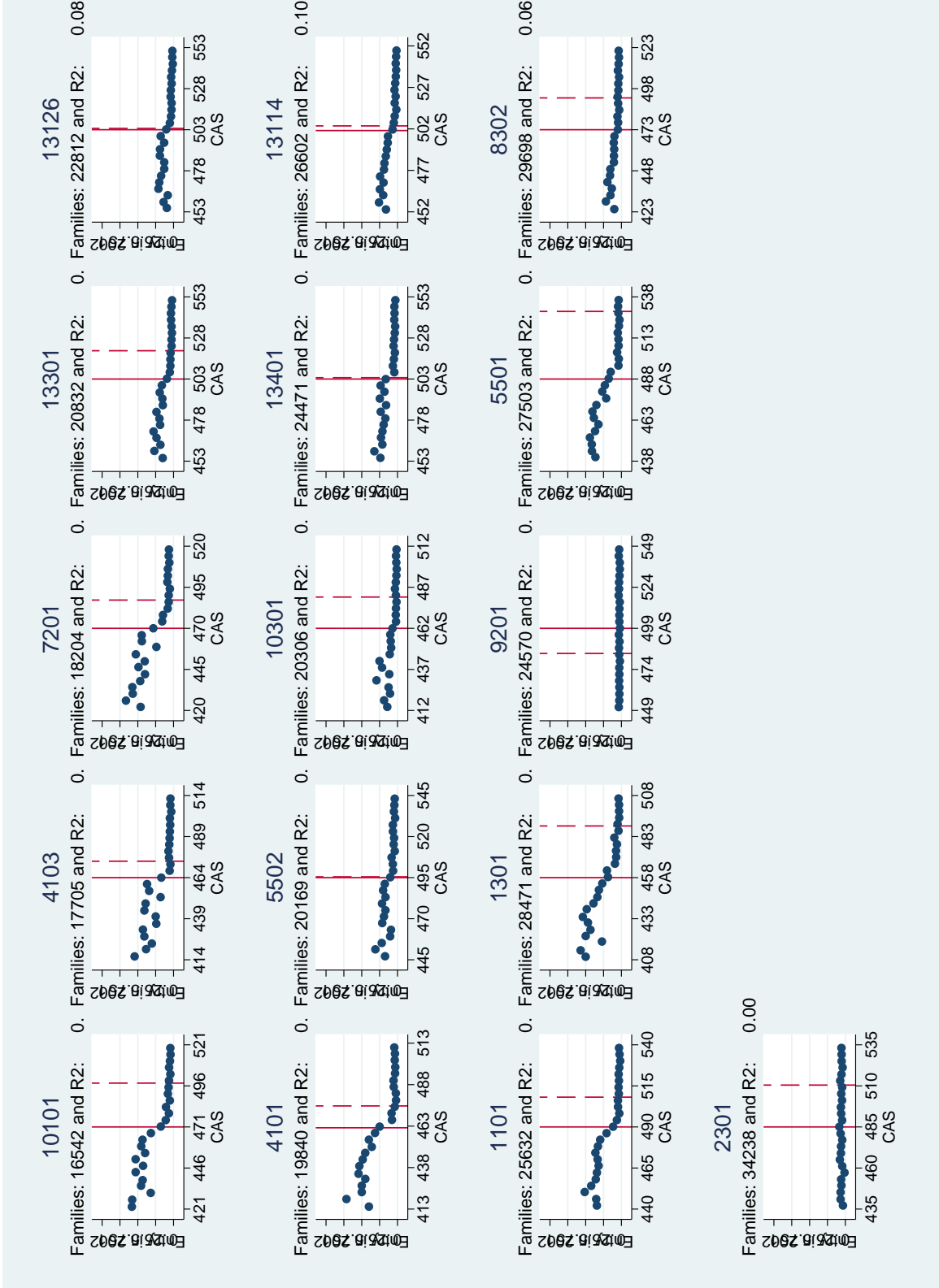
Participation in CS and effective cutoff and official cutoff in illustrative municipalities (cont.).



Participation in CS and effective cutoff and official cutoff in illustrative municipalities (cont.).



Participation in CS and effective cutoff and official cutoff in illustrative municipalities (cont.).



Participation in CS and effective cutoff and official cutoff in illustrative municipalities (cont.).

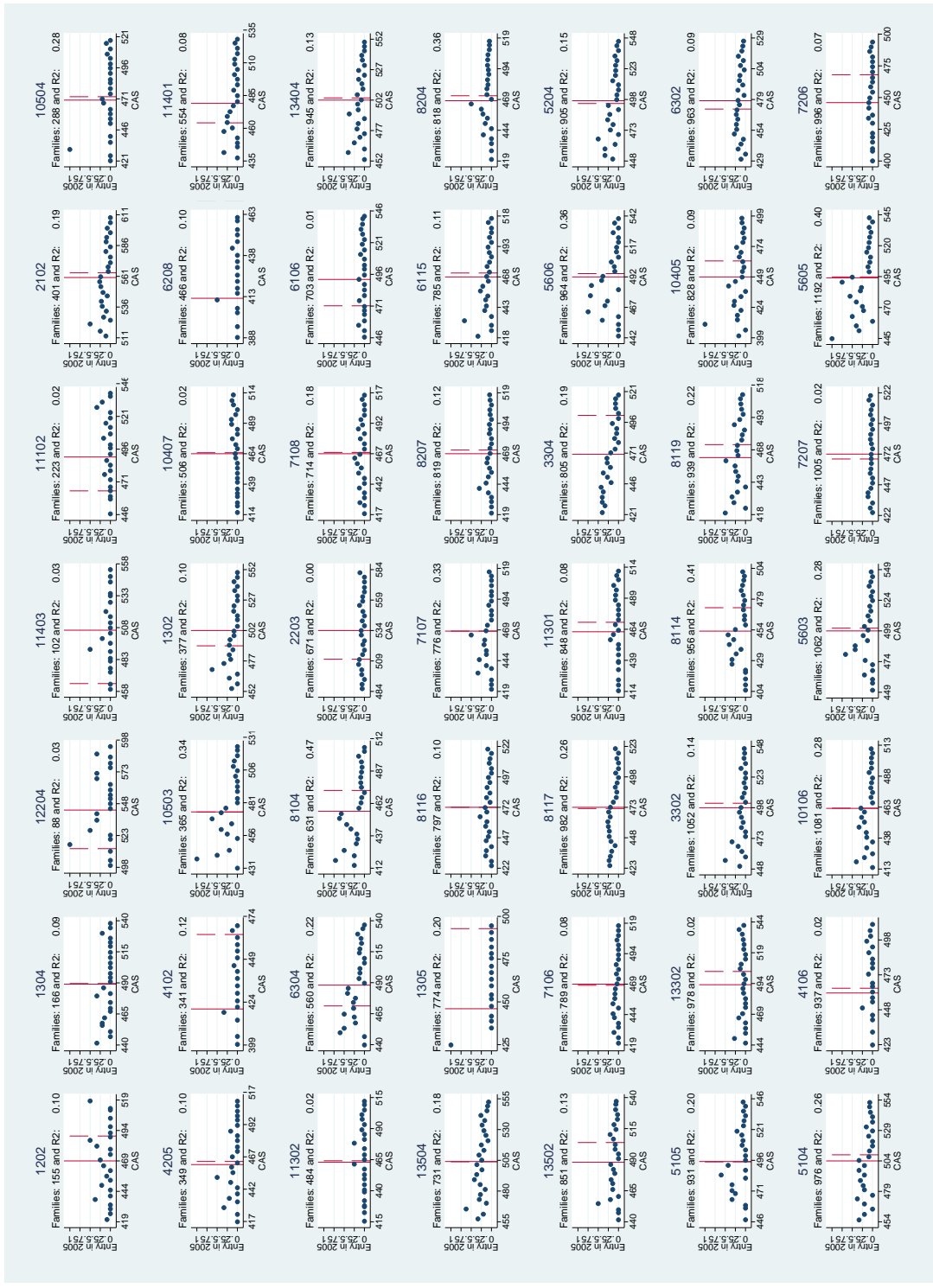
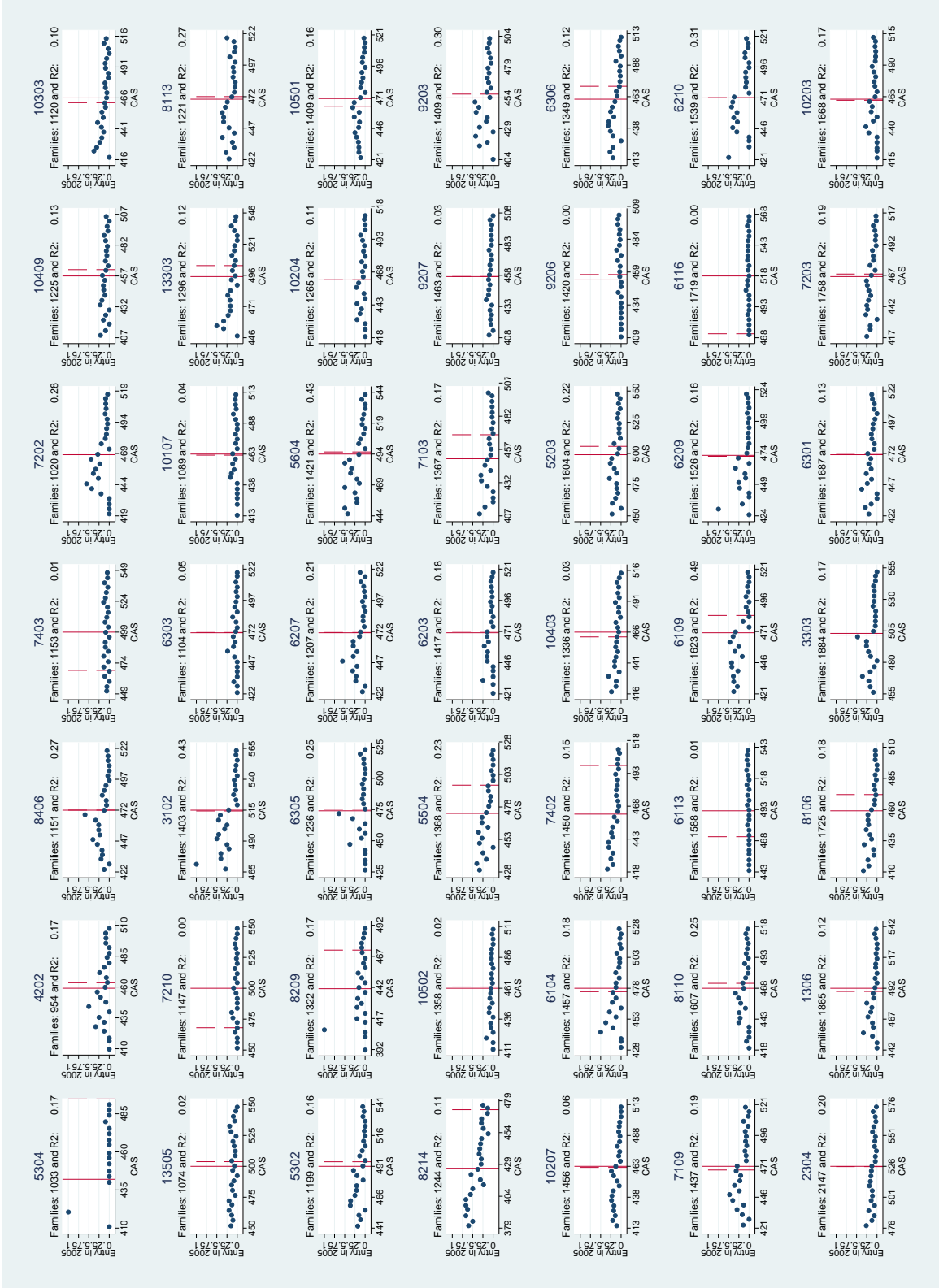
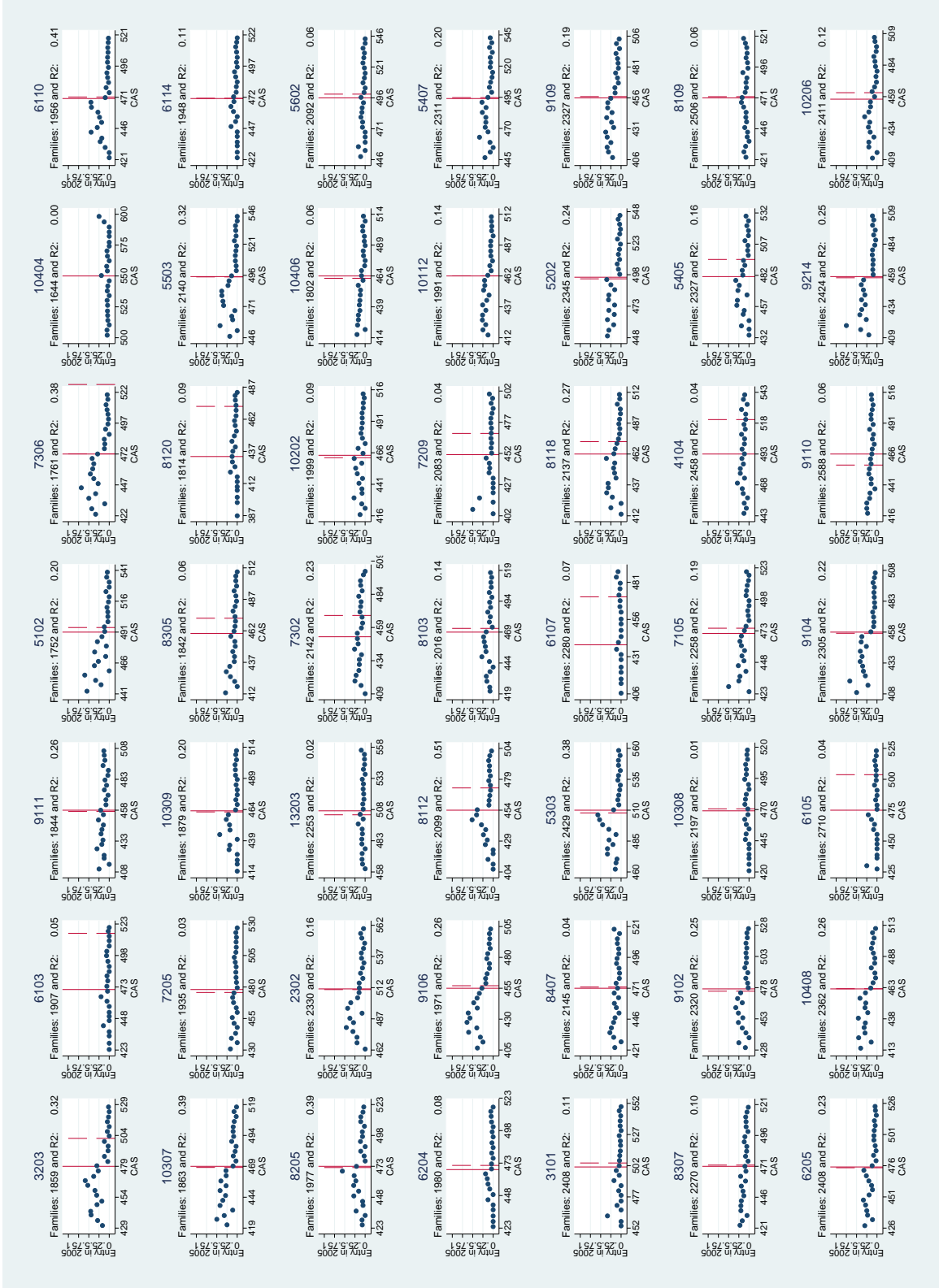


Figure B.3: Participation in CS and *effective cutoff* in illustrative municipalities (2005).

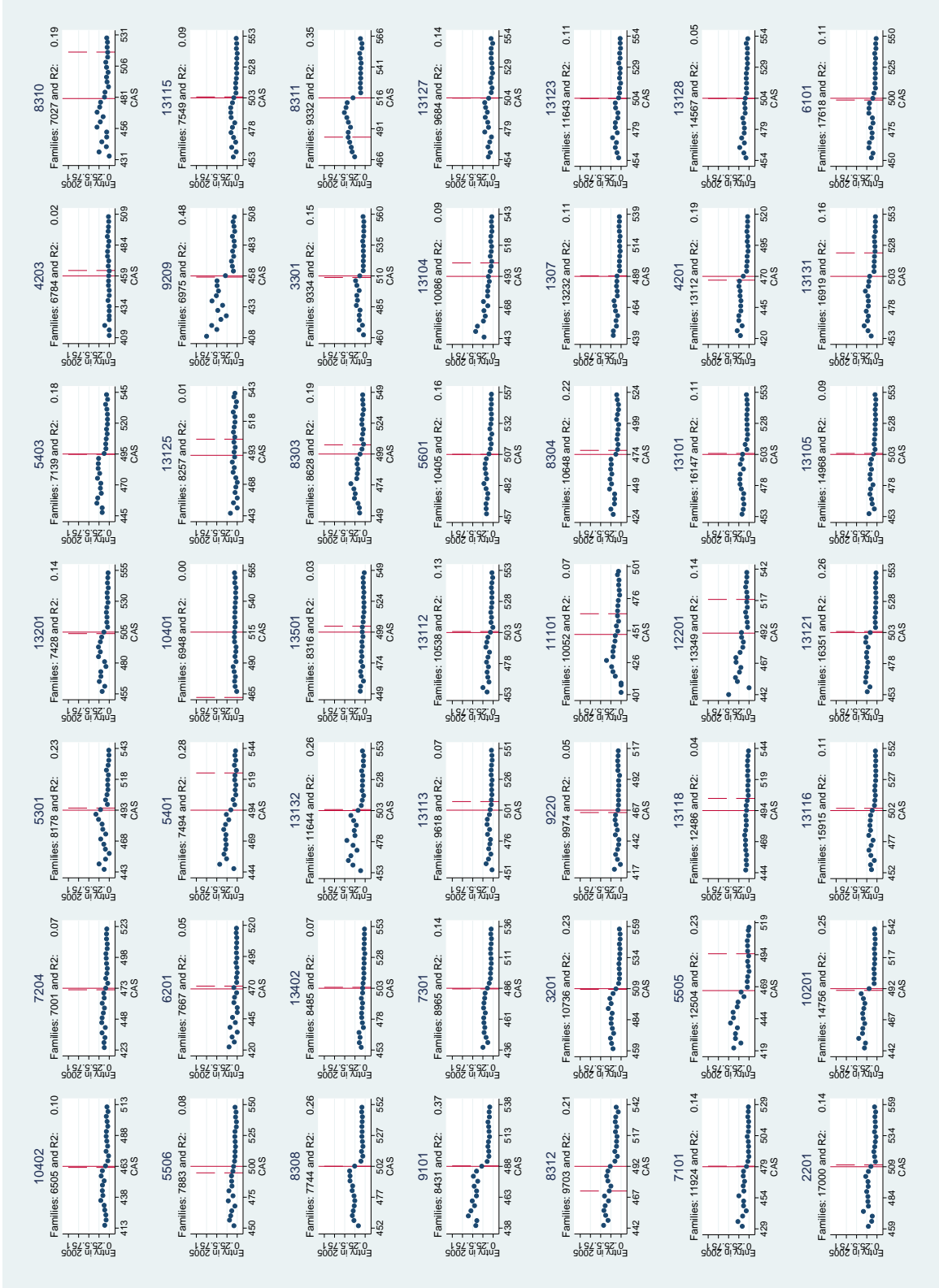
Note: The dots on the graphs are the proportion of families in municipality entering in CS in each year (see y-axis) by intervals of CAS score of 4 points in distribution of CAS in 2002 in each municipality. The solid vertical line represents the point identified as *effective cutoff* and the dashed line is the *official cutoff*. Where only one line is shown, the two coincide. The graphs in the figure are ordered by increase size of municipality of residence in 2002. The R^2 in the top of each graph is the *effective cutoff*, so that only families at most 40-points apart from it are depicted (the CAS score varies between 380 and 770 points).



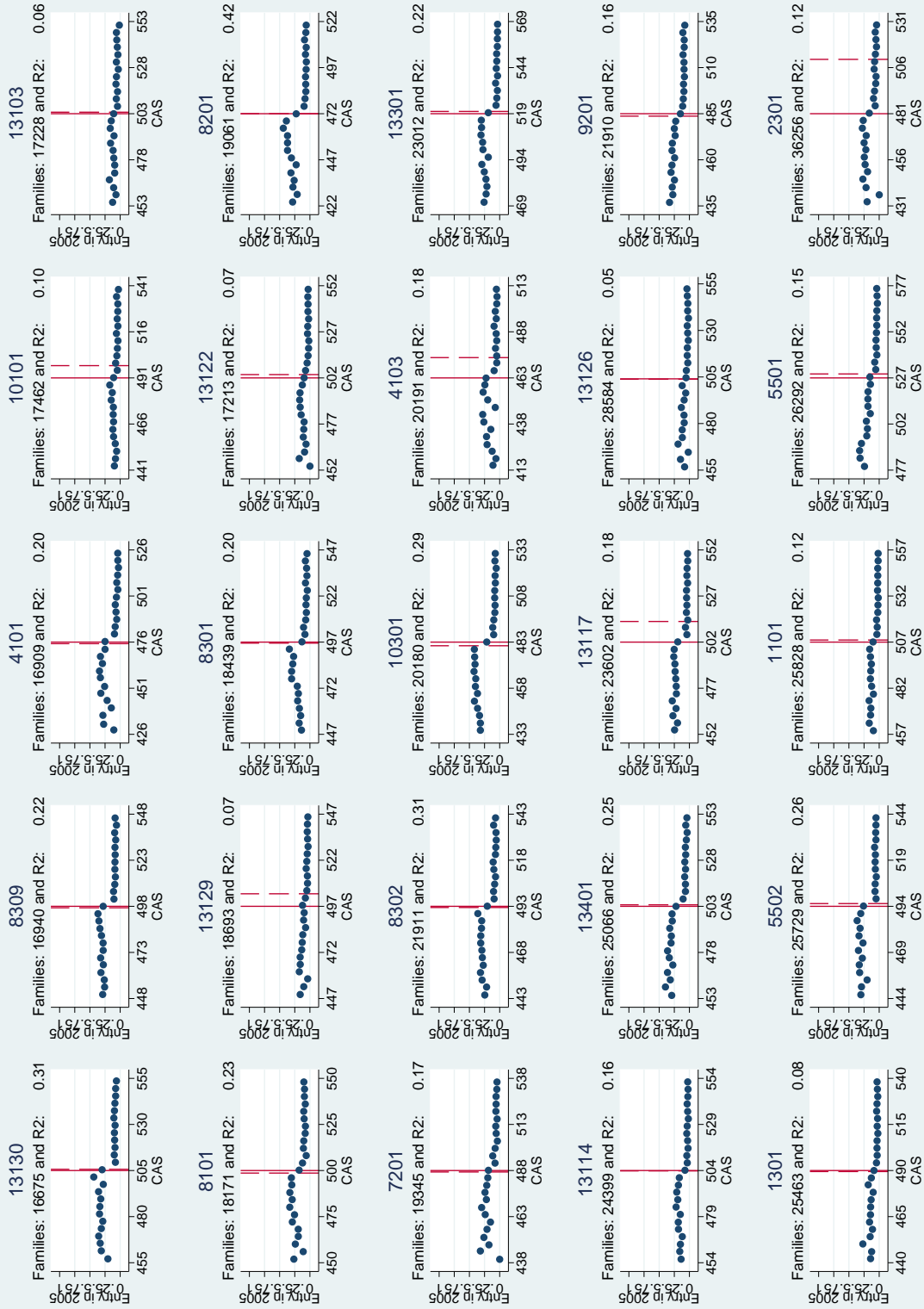
Participation in CS and effective cutoff and official cutoff in illustrative municipalities (cont.).



Participation in CS and effective cutoff and official cutoff in illustrative municipalities (cont.).



Participation in CS and effective municipalities (cont.).



Participation in CS and effective cutoff and official cutoff in illustrative municipalities (cont.).

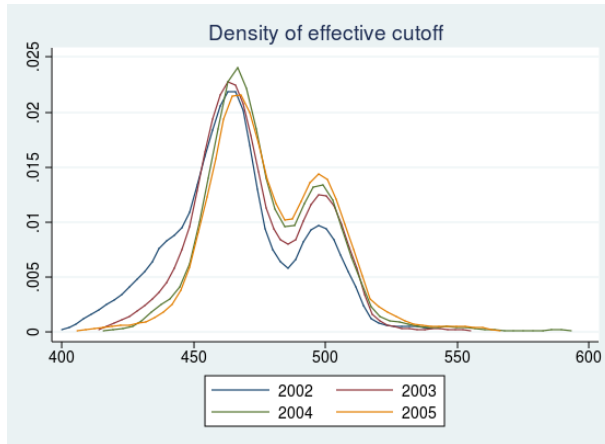


Figure B.4: Distribution of effective cutoffs

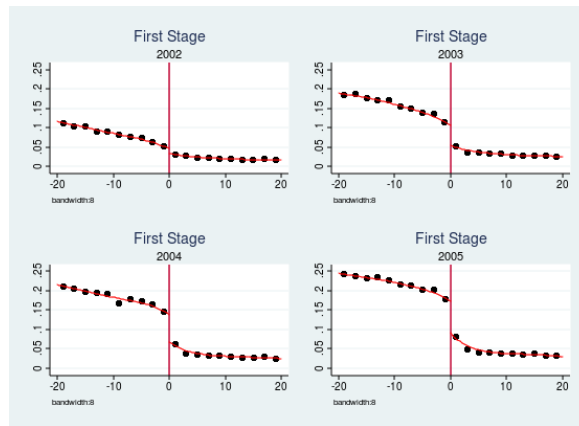


Figure B.5: Participation in CS among eligible and non-eligible: Eligibility defined by the official cutoff.

Note: The continuous lines are local linear regression estimates of an indicator for entry in CS in the year indicated on the top of each panel on distance to cutoff in that year. The bandwidth is set to 8. Circles in figures represent the mean outcome by cell within intervals of 2-points of distance to cutoff. The kernel used is Epanechnikov.

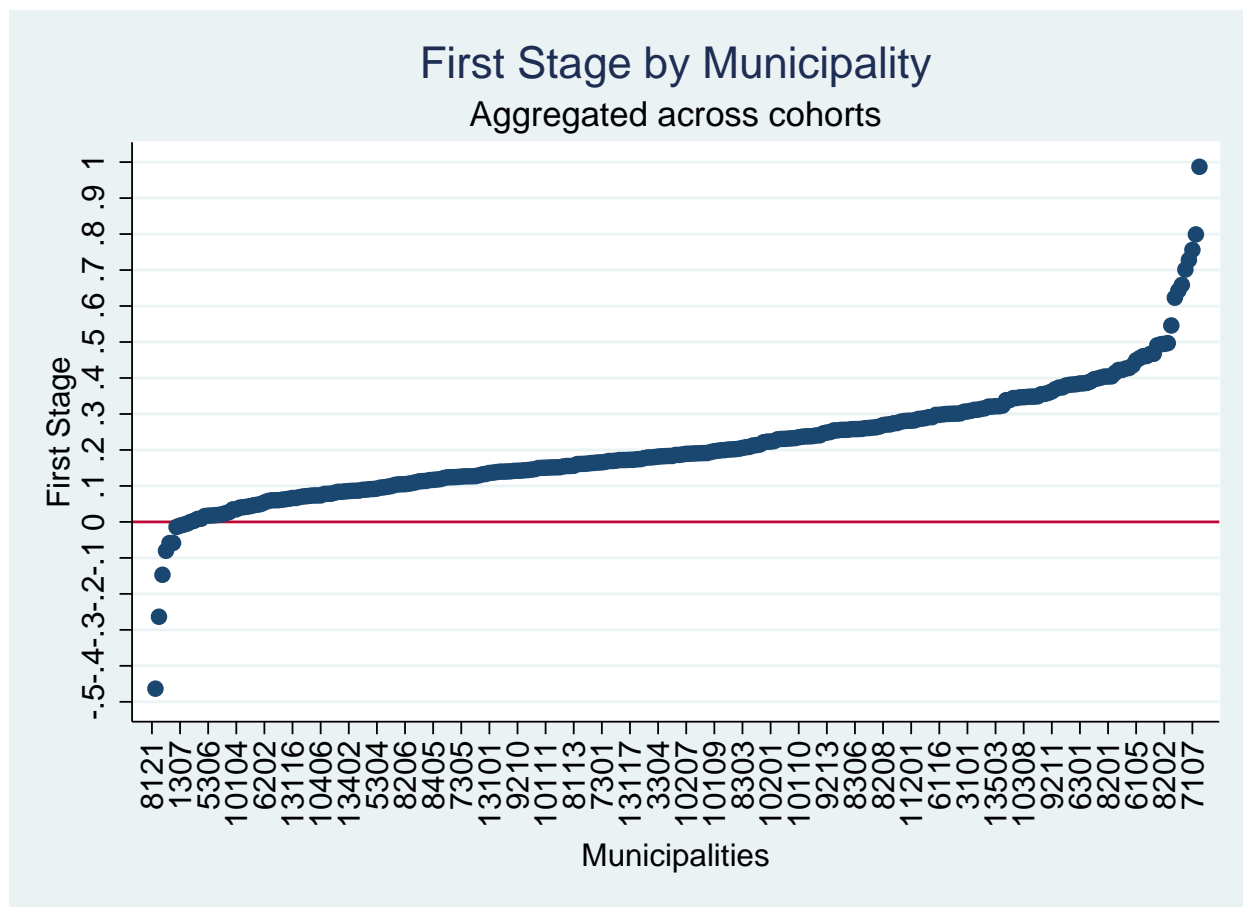


Figure B.6: First Stage Estimates by Municipality.

Note: Each circle in the figure presents estimates for γ from the following first stage equation $CS_{imk} = \phi + \gamma E_{im} + f(CAS_{im} - \overline{CAS}_m) + u_{imk}$ estimated separately for each municipality. All cohorts (2002-2006) are pooled together.

The codes in the x-axis refer to the municipalities, which are ordered by size of $\hat{\gamma}$. The codes correspond to the Chilean official coding and are the same codes using in figures B.2 and B.3. Not all municipalities are labelled for presentation purposes.

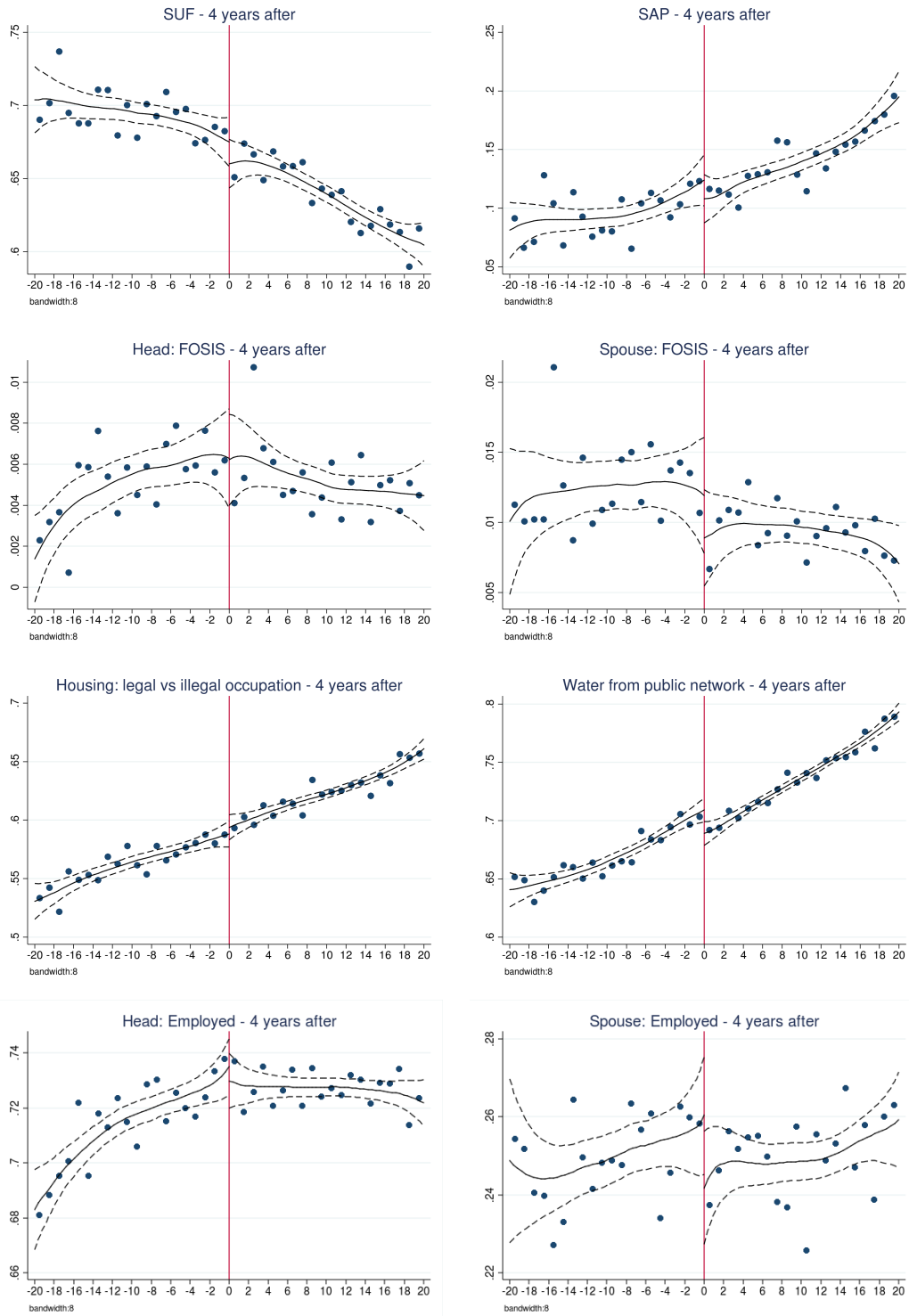


Figure B.7: Average outcomes by eligibility status 4 years into the program, Bandwidth = 8. Note: The continuous lines in figure present local linear regression estimates of several outcomes on percentage distance to cutoff. Circles in figures represent the mean outcome by cell within intervals of 2 points of distance to cutoff. The kernel used is Epanechnikov.

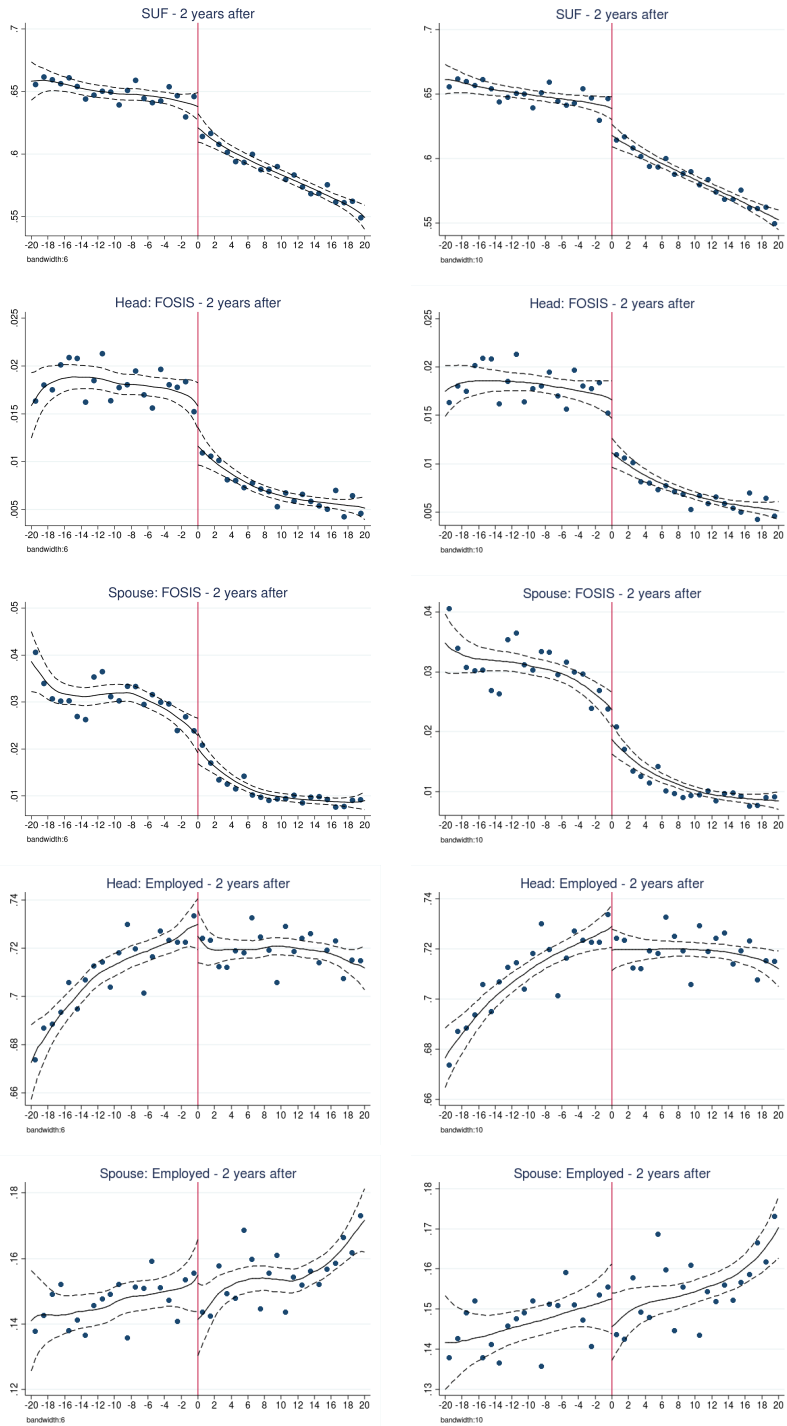


Figure B.8: Average outcomes by eligibility status and 2 years into the program. The figures on the right hand side use a bandwidth of 6-points, whereas the figures on the left hand side use a bandwidth of 10-points.

Note: The continuous lines in figure present local linear regression estimates of several outcomes on percentage distance to cutoff. Circles in figures represent the mean outcome by cell within intervals of 2 points of distance to cutoff. The kernel used is Epanechnikov.

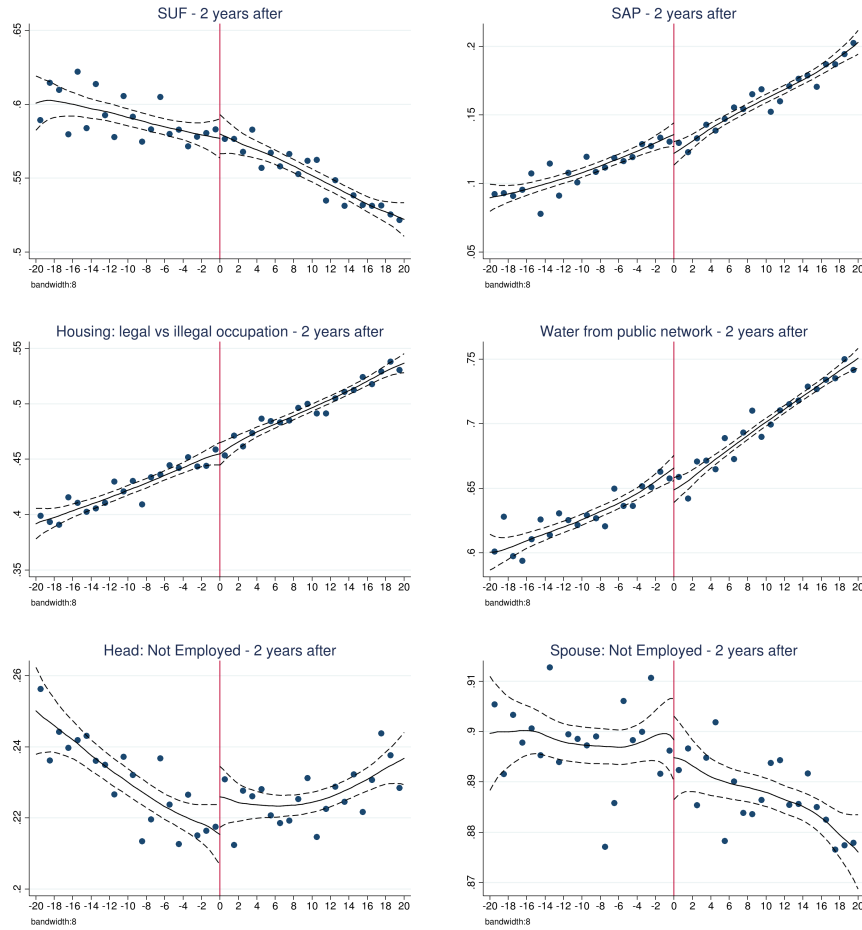


Figure B.9: Balancing checks: Pre-2002 outcomes by eligibility status, Bandwidth = 8.
 Note: The continuous lines in figure present local linear regression estimates of several outcomes on percentage distance to cutoff. Circles in figures represent the mean outcome by cell within intervals of 2 points of distance to cutoff. The kernel used is Epanechnikov.
 Next to the outcome used we include the designation of the same used, that is, this figures uses the sample used in the estimation of potential exposure to Chile Solidario for 2 years.

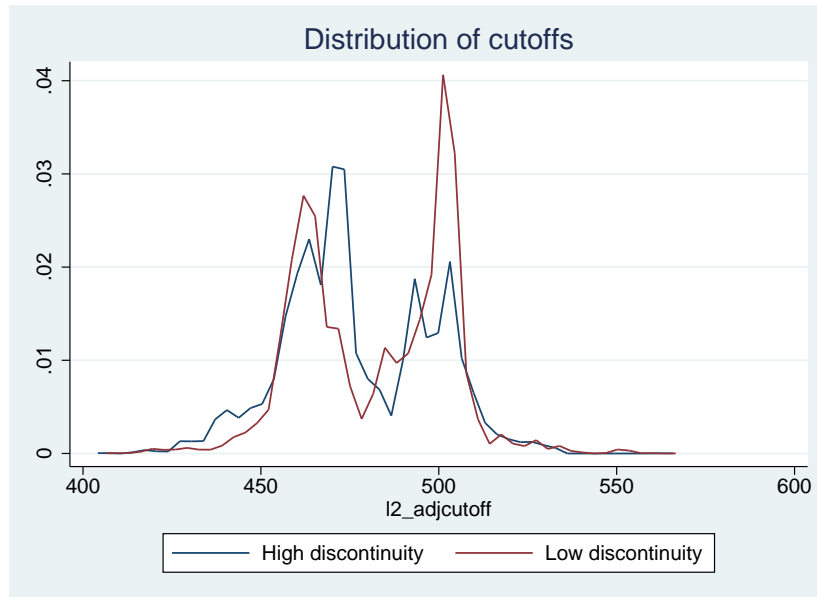


Figure B.10: Distribution of effective cutoffs

Note: The figures presents the distribution of cutoff for those municipalities with high and without (or with low) discontinuity in estimation of equation 2. We consider that a municipality has high discontinuity in estimation of equation (2) if the estimate for ψ at the value of CAS-score that maximizes the R^2 of (2) is at least 0.1. All cohorts (2002-2006) are pooled together.

C Dynamic RD and repeated observations

One potential problem of equation (5) is that it ignores the evolution of the effective cutoffs within each municipality which followed the program roll out, and which means that families just ineligible in t may become eligible $t + 1$ (equation (1) is static, while selection into CS is sequential). As a result, our static IV estimates could be too small, because they ignore the fact that, over time, an increasing fraction of ineligible individuals is able to participate in CS. At the same time, it is also true that over time, the fraction of eligible individuals enrolled in the program may also change, which is again ignored when we take the static RD estimates at different points in time.

Therefore, we adapt the standard RD procedure to our setting, following Cellini, Ferreira and Rothstein, 2010. We have a panel in which individuals who do not receive CS in a given year may receive it in subsequent years. We then use a version of their procedure to test whether this subsequent entry invalidates our static approach. Let β^k be the impact on some outcome Y of having first enrolled in CS k years ago. To simplify, take the first cohort of participants in the program, 2002. We can estimate β^1 from:

$$Y_{2003im} = \alpha + \beta^1 CS_{2002im} + f(CAS_{2002im} - \overline{CAS}_{2002m}) + \varepsilon_{2003im}$$

where we instrument CS_{2002im} with E_{2002im} . Similarly, for those that could have started the program in 2002 we estimate the effects in 2004 (2 years after entry)

$$Y_{2004im} = \alpha + \theta^2 CS_{2002im} + f(CAS_{2002im} - \overline{CAS}_{2002m}) + \varepsilon_{2004im} \quad (6)$$

again instrumenting CS_{2002im} with E_{2002im} , but in this case $\theta^2 \neq \beta^2$, because some individuals for whom $CS_{2002im} = 0$ may have $CS_{2003im} = 1$. In other words, θ^2 measures a weighted average of 2-years and 1-year impacts, since some of the families around the 2002-cutoff will enroll in CS in 2003.

To see this suppose there are three time periods: $t = 1, 2, 3$. Then:

$$\begin{aligned} Y_1 &= \alpha + \beta^1 CS_1 + \varepsilon_1 \\ Y_2 &= \alpha + \beta^2 CS_1 + \beta^1 CS_2 + \varepsilon_2 \\ Y_3 &= \alpha + \beta^3 CS_1 + \beta^2 CS_2 + \beta^1 CS_3 + \varepsilon_3. \end{aligned} \quad (7)$$

Then, the β^1 , the impact of being in the program for 1 year, is given by

$$\begin{aligned} E(Y_1|E_1 = 1) - E(Y_1|E_1 = 0) &= \beta^1 [E(CS_1|E_1 = 1) - E(CS_1|E_1 = 0)] \Leftrightarrow \\ \frac{E(Y_1|E_1 = 1) - E(Y_1|E_1 = 0)}{E(CS_1|E_1 = 1) - E(CS_1|E_1 = 0)} &= \beta^1 = \theta^1 \end{aligned} \quad (8)$$

Assuming constant effects across cohorts and individuals, the impact of being in the program for

2 years, β^2 , is given by

$$\begin{aligned}
& E(Y_2|E_1 = 1) - E(Y_2|E_1 = 0) = \\
& \beta^2[E(CS_1|E_1 = 1) - E(CS_1|E_1 = 0)] + \beta^1[E(CS_2|E_1 = 1) - E(CS_2|E_1 = 0)] \Leftrightarrow \\
& \frac{E(Y_2|E_1 = 1) - E(Y_2|E_1 = 0)}{E(CS_1|E_1 = 1) - E(CS_1|E_1 = 0)} = \beta^2 + \beta^1 \frac{E(CS_2|E_1 = 1) - E(CS_2|E_1 = 0)}{E(CS_1|E_1 = 1) - E(CS_1|E_1 = 0)} \Leftrightarrow \\
& \theta^2 = \beta^2 + \beta^1 \pi^1 \tag{9}
\end{aligned}$$

where we estimate β_1 from equation (8) and π^1 is a ratio of two first stage estimates: (i) the coefficient on eligibility in $t = 1$, E_1 , from a regression of an indicator of entry in CS in $t = 2$, CS_2 , on eligibility in $t = 1$, E_1 , conditional on not having started CS in $t = 1$, $CS_1 = 0$ (controlling for a function of CAS in $t = 1$, which we omit above to simplify notation) and (ii) the coefficient on eligibility in $t = 1$, E_1 , from a regression of an indicator of entry in CS in $t = 1$, CS_1 , on eligibility in $t = 1$, E_1 (controlling for a function of CAS in $t = 1$, which we omit above to simplify notation).

The impact of being in the program for 3 years, β^3 , is given by

$$\begin{aligned}
& E(Y_3|E_1 = 1) - E(Y_3|E_1 = 0) = \beta^3 [E(CS_1|E_1 = 1) - E(CS_1|E_1 = 0)] \\
& + \beta^2 [E(CS_2|E_1 = 1) - E(CS_2|E_1 = 0)] + \beta^1 [E(CS_3|E_1 = 1) - E(CS_3|E_1 = 0)]
\end{aligned}$$

Thus,

$$\begin{aligned}
& \frac{E(Y_3|E_1 = 1) - E(Y_3|E_1 = 0)}{E(CS_1|E_1 = 1) - E(CS_1|E_1 = 0)} = \beta^3 + \beta^2 \frac{E(CS_2|E_1 = 1) - E(CS_2|E_1 = 0)}{E(CS_1|E_1 = 1) - E(CS_1|E_1 = 0)} + \\
& \beta^1 \frac{E(CS_3|E_1 = 1) - E(CS_3|E_1 = 0)}{E(CS_1|E_1 = 1) - E(CS_1|E_1 = 0)} \\
& \theta^3 = \beta^3 + \beta^2 \pi^1 + \beta^1 \pi^2.
\end{aligned}$$

In the derivation above we assumed that θ^k does not depend on $t - k$ (i.e., β^k does not depend on year of entry into CS nor do the π terms).

Table C.1 shows that most π s are very small in magnitude, therefore the estimates of the main results are largely unaffected by the dynamic entry into the program around the threshold. The coefficients of β s (adjusted) for the take-up of public subsidies are all within 95% of the θ s (not adjusted) in table C.2.

Table C.1: Delayed Entry in CS: Estimates of entry in CS in subsequent years around a cutoff of a given year.

	(1)	(2)	(3)	(4)	(5)
Cutoff in $t-k$	$t-2$	$t-3$	$t-4$	$t-5$	$t-6$
Dependent Variable			Panel A		
$CS_{t-(k-1)}$	0.139*** (0.010)	0.138*** (0.008)	0.148*** (0.010)	0.099*** (0.012)	0.072*** (0.017)
Observations	92,738	96,127	79,610	68,414	22,909
Control Mean	0.201	0.200	0.171	0.181	0.186
			Panel B		
$CS_{t-(k-2)}$		0.066*** (0.008)	0.077*** (0.009)	0.063*** (0.009)	0.038*** (0.013)
Observations		47,368	48,078	48,360	16,159
Control Mean		0.162	0.159	0.141	0.143
			Panel C		
$CS_{t-(k-3)}$			0.047*** (0.012)	0.046*** (0.010)	0.050*** (0.014)
Observations			23,271	34,464	14,654
Control Mean			0.150	0.143	0.131
			Panel D		
$CS_{t-(k-4)}$				0.022** (0.011)	0.064*** (0.022)
Observations				12,694	4,230
Control Mean				0.142	0.114

Note: The table presents estimates of the indicator of entry in CS included in each row on eligibility (and the function of distance to cutoff) measured at the period indicated in each column. The coefficient estimate presented in column (1) in the first row results from regressing an indicator of entry in CS the previous year to which the outcome is measured, CS_{t-1} , on eligibility taken 2 years before, E_{t-2} (as well as distance to cutoff and municipality of residence fixed effects), conditional on not having entry in CS in $t-2$, $CS_{t-2} = 0$. The sample used in estimation is the sample used to estimate the effects on SUF. The estimates for other outcomes are similar and are available from the authors.

Robust standard errors are reported in parenthesis clustered at municipality of residence when eligibility is evaluated. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table C.2: Dynamic Regression Discontinuity Estimates: 2SLS adjusted and unadjusted estimates for the whole sample.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Years after start	2		4		6				
RDD	Not adjusted	Adjusted	Sample	Not adjusted	Adjusted	Sample	Not adjusted	Adjusted	Sample
SUF	0.110*** (0.033)	0.096*** (0.033)	2004-2008	0.090 (0.058)	0.083* (0.047)	2006-2008	0.369*** (0.139)	0.281** (0.124)	2008
Labor market									
Employed (head)	0.051* (0.029)	0.022 (0.033)	2004-2008	0.008 (0.037)	0.066*** (0.020)	2006-2009	-0.110* (0.062)	-0.083 (0.079)	2008-2009
Employed (spouse)	0.015 (0.029)	0.016 (0.026)	2004-2008	0.051 (0.051)	0.023 (0.057)	2006-2009	-0.022 (0.086)	-0.020 (0.080)	2008-2009

Note: The table presents the estimated coefficients (and standard errors) for the indicator of entry in CS 2, 4 and 6 years before the time at which outcome is measured in model 5. Controls excluded from table include quadratic in distance to cutoff, their interaction with the CS indicator and municipality-year effects. The municipality of residence and distance to cutoff are measured when eligibility is evaluated.

Robust standard errors are reported in parenthesis clustered at municipality of residence when eligibility is evaluated. * significant at 10%; ** significant at 5%; *** significant at 1%.

D Data

D.1 The Ficha CAS and the CAS score

The ficha CAS is used to compute the CAS score (index of unsatisfied basic needs), and it is used as an instrument for targeting most social programs in Chile since 1980. This register covers around 30% of the Chilean population and it includes 50 variables grouped into 9 categories. The index is used to determine eligibility several programs, some of them use CAS score to rank the applicants and serve those in more need, whereas other programs use CAS as one of the variables to be considered when determining eligibility status.

The CAS is a continuous index that results from a weighted average of underlying variables. The variables that enter the score have different weights and are concerned to four main areas: housing conditions (wall, floor, ceiling, overcrowding, water access, sewage, shower), property type, education of family members, occupation, income, and ownership of durables (fridge, boiler, tv). Housing and education of the head of family or spouse represent almost half of the weight of the index (see Larrañaga, 2005).

The Ficha considers the family as the unit of reference, which is defined as a group of persons that live together, whether or they not are relatives, and who share some kind of income and auto-recognize themselves as a family. The unit of application of this survey is the household, so each time someone or a family applies for a Ficha, the entire household will be surveyed. The questionnaire is filled by the head of family, and only under his/her authorization other member may fill the questionnaire.

The Ficha is valid for a period of two years, as long as families do not change their address or composition. The survey is filled at family's house and to attest the credibility of information provided 20% of all valid surveys are randomly re-interviewed by a supervisor and all surveys with invalid entries are revised and if necessary households are re-interview.

This data does not intend to represent the Chilean population. An individual or family that intends to apply for a social program will do it at the office supplying the program or at the municipality. It is also possible that the local authority takes the initiative to survey a family to learn about its vulnerability. Since all the information is centrally managed, it is difficult to game the system by obtaining scores in more than one municipality, and using the most favorable one.

The Ficha CAS is relatively cheap to administrate costing about US\$8.65 per household and this cost is borne by the municipalities. About 30 percent of Chilean households undergo interviews, which is reasonable given that the target group for the subsidy programs is the poorest 20 percent. In 1996, administrative costs represented a mere 1.2 percent of the benefits distributed using the CAS system (see Clert and Wodon, 2002).

To understand the dynamics of entry in the data, we present in table D.1 a cross tab between the number of families in each wave against the first year the family has a valid survey. In each year between 2000 and 2005 there are around 1.5-1.8 millions families with a valid score (in 2006 there are only 0.7 million of families, since this was the year of transition to FPS). About 70% of the families with CAS valid in 2002 already had a valid score in either 2000 or 2001. Between 2000 and 2006 about 80% of the families requested the survey twice. This shows that there is some persistency of families in the system.

Table D.1: Entry in CAS and FPS system.

year	2000	2001	2002	2003	First survey		2006	2007	2008	2009	Total
					2004	2005					
2000	911451	0	0	0	0	0	0	0	0	0	911451
2001	591928	1007589	0	0	0	0	0	0	0	0	1599517
2002	411449	761596	492069	0	0	0	0	0	0	0	1665114
2003	407838	478048	430662	363590	0	0	0	0	0	0	1680138
2004	390184	529435	175638	307440	375248	0	0	0	0	0	1777945
2005	371145	508127	212268	120912	320014	354134	0	0	0	0	1886600
2006	116321	159658	66801	51642	52090	27969	239109	0	0	0	713590
2007	301902	404250	155137	107885	107335	96040	57938	474205	0	0	1704692
2008	437427	502430	219427	154967	154910	142719	88950	413452	622539	0	2736821
2009	469306	527809	238250	169572	168932	156477	98665	394302	586865	404427	3214605
Total	4408951	4878942	1990252	1276008	1178529	777339	484662	1281959	1209404	404427	17890473

Note: Number of households with a valid CAS/FPS score in each year vs. the year they are first observed in the system.

D.2 The Ficha de Proteccion Social

In 2007 the instrument to select families into the program was replaced by the Ficha de Proteccion Social. This new targeting instrument assesses the vulnerability to of households to short and long run shocks. This is a significant change from the CAS, which weighed heavily on assets and durables ownership. The FPS considers the needs of different members in the household according to equivalence scales. The unit of reference is the family defined as a household, that is, individuals that live together and share family expenditures. Whereas the CAS (2000-2006) score is valid for 2 years, the FPS-score (2007-2009) is updated monthly.

As Ficha CAS, FPS has information on each family's member date of birth, education, income and labor market participation, house ownership and its conditions. FPS contains variables related with use of health facilities, school attendance by children, disability status of members and alcohol and drugs use of family members.

D.3 Constructing the administrative panel (Consolidado CAS and FPS)

The data we use is a panel formed using Ficha CAS and FPS that includes individuals surveyed between March 1998 and May 2008. We performed the following checks to each cross section of the data:

- We drop repeated observations in 2000, 2001 and 2007, which correspond to least to two identical rows of data.;
- We recode the individual identifier, RUT (Rol Unico Tributario) or RUN (Rol Unico Nacional)⁴⁸, to missing if it is too small (1000 or less) and flag observations with the same

⁴⁸The national identification number in Chile is the RUT (Rol Unico Tributario); sometimes it is called RUN (Rol Unico Nacional). It is used as a national identification number, tax payer number, social insurance number, passport number, driver's license number, for employment, etc., and it allows us to merge the several administrative data sets used in the paper. Since year 2004 every born baby has a RUT number; before it was assigned at the moment of applying to get the ID card. Each individual in the data set is identified by a unique combination of RUT and *digito verificador*. The *digito verificador* is either a letter or number that is assigned to each RUT by an algorithm that ensures the authenticity of RUT.

identifier⁴⁹. We verify whether individuals have valid identifier, this is important because is the combination RUT-*digito verificador* that allows us to merge the several waves of CAS Consolidado, FPS and these with data from other sources. We consider that an individual possess a valid RUT if it fulfils several requirements: (i) if it is larger than 50,000, (ii) if the *digito verificador* is correctly assigned, and (iii) if it is not missing. Individuals with invalid or missing RUT tend to have lower income, less years of education, to be in families with lower CAS and in larger families, are less likely to be head of family and to be younger than 18;

- We check if two individuals with the same combination RUT-*digito verificador* are the same person. Two individuals surveyed in the same year with the same RUT, *digito verificador*, gender, date of birth, region, province and municipality of residence, number of survey, relationship to head of family, name and surname and CAS are considered the same person, so we keep only one observation per year;
- As CAS index is assigned to the family, we dropped families with CAS varying within family;
- We found a few observations of heads of family whose parents or grandparents are younger than the head (on average 1500 out of 6 millions individuals per wave), which we flag but do not exclude from data given the small proportion of cases.

All income related variables are top coded at the 99th percentile and all income values are deflated to May 2008 using the monthly CPI (Banco Central de Chile, 2008). We have some concerns regarding the quality of income data in 2006: for 179394 observations (35% out of 506051 non-missing observations) the period of income reported is 0, which is an unassigned code.

D.4 Register of CS Participants: The Puente Data Set

We use data on all families that were ever invited to participate in Chile Solidario between 2002 (when the program was implemented) and May 2009. Among other information, this data includes the exact date of entry of each family in CS. Additionally, for each family we have the Chilean National identification number (the RUN) of the individual who receives the cash transfers associated to CS (the *Bono Chile Solidario* and the *Bono de Egreso*). We use the RUN to link the data on participation with the other administrative data we use (Ficha CAS 2000-2006, FPS 2007-2009 and the register of participants in employment programs offered by FOSIS between 2004 and 2007).

References

- [1] Clert, Carine and Quentin Wodon, 2002, The Targeting of Government Programs in Chile: A Quantitative and Qualitative Assessment, MPRA Paper No. 15414, posted 26. May 2009 00:05 UTC, Online at <http://mpra.ub.uni-muenchen.de/15414/>.

⁴⁹An individual without documents can be identified by a missing RUT and a digito 1 in Ficha CAS or an entry of RUT equal to date of birth in FPS. Foreign individuals have RUN 1 in FPS.