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ABSTRACT

Tackling Social Exclusion: Evidence from Chile*

We study an innovative welfare program in Chile which 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. We find strong and lasting impacts of the program on the take up of subsidies and employment services. These impacts are important only for families who had little access to the welfare system prior to the intervention.

JEL Classification: C26, I38, J08

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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 simultaneously tackled multiple dimensions of deprivation in the lives of these families through frequent and personal contact with them, and the coordination of different government agencies providing various 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, or *Brasil Sem Miséria* in Brazil).

This paper studies short and medium run impacts of CS on those who participated in it between 2002 and 2006. Our main results focus on three sets of outcomes: the take-up of subsidies, and participation in training and employment programs; the labor force participation and employment of heads of household and their spouses; and housing conditions.[1] We find that CS participants increase their take-up of a family allowance for poor children (the Subsidio Unico Familiar, hereafter SUF) by 11%, relatively to an average take-up of 65% among comparable non-participants. There is also some 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 general improvements in employment outcomes. Finally, we find no evidence of important impacts on housing conditions of participant families.

Program impacts on the take-up of subsidies are especially large for families not accessing services before CS was implemented (the primary target group), and for families enrolling in CS only after 2004, when the expansion of the supply of social services was effectively implemented. For example, the increase in the uptake of SUF is 22% for families who did not take up these subsidies before 2002, and it is visible at least for 4 years after the family first enrolled in CS. If we further condition on having enrolled in CS after 2004, the impact on the take-up of SUF rises to 32%.[2] Similarly, there is a 20% increase in the employment rate of females (spouses of the

[1]The choice of outcomes is dictated in part by the fact that we use administrative data in our evaluation, which has a more limited set of outcomes than, say, 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.

[2]Since 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.

household head), if they were not employed before 2002, and if their family enroled in CS after 2004.

In order 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 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 mainly because of supply constraints. CS was rolled out for a period of 5 years and about 20% of the 250,000 target families were enrolled each year, giving priority to the poorest (more than 95% of all eligible families invited to participate in CS accept the invitation).

We estimate that the average impact of eligibility for CS on participation in the program 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 up to 6 years after entry into the program.

Galasso (2006) conducts the first evaluation of this program. 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 results is consistent with the findings in this paper, whereas the administrative data we use does not allow us to analyze either health or education outcomes in detail. However, the survey she uses 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. 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 exactly the same administrative dataset as us, although in the latter paper they also complement it with the household survey originally used in Galasso (2006). The questions they ask are analogous to ours, but the evaluation methodology is quite different. They rely on a mix of differences in differences and matching, and find small impacts of the program across a variety of dimensions. They also focus on more limited set of cohorts than we do.

The RD estimator we use here provides an important alternative to evaluation results based on the matching or differences in differences estimators implemented in the papers discussed above. Relatively to the matching estimator, it accounts for selection into the program based on unobservable variables. Relatively to a differences in differences estimator, it does not rely on a common trends assumption between treatment and control groups, which may be problematic if the poorest households in Chile, which are also the first to be served by the program, are on a different trend than those not served by the program, which could well be the case. The standard criticism of an RD type estimator such as the one we implement is that it is only able to identify program impacts for the population of households in the neighborhood of the discontinuity threshold, which may or may not be an interesting parameter to look at, depending on the application. However, in our setting there is a very large number thresholds, which vary across time and space, which means that we are able to identify program impacts in the neighborhood of a large number of discontinuity points. Therefore, while it is still true that we will not be able to estimate program impacts for very poor and very rich people, who are never in the neighborhood of any of the discontinuities considered here, our estimates should be valid for a large set of households in the middle of these two groups. 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. However, their focus is very different: their outcome of interest is test scores of children, which can be obtained from school records and merged with program records. In addition, they study only the first cohort of participants. This could be a limitation since the program is likely to have become much more effective for later cohorts, as we explain below. In their paper, they report not only the average impact of the program from the RD estimator, but also how these impacts vary across different discontinuity thresholds.

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 success in 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 increase in access to monetary subsidies and services for households previously disconnected from the welfare system is not only important because families are able to supplement their income through them, but also because they become more linked to the social protection system. The intense psychosocial support through home visits is 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.

However, even a program as innovative and intensive 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 target population is difficult to work in terms of skills, and physical capital and psychological endowments. The 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). A more recent literature has also focused on psychological constraints (Mullainathan and Shafir, 2013).

The empirical evidence on interventions that combine capital and skill enhancement in developing countries is small but growing rapidly. Integrated programs for poor adolescents (Bandiera et al., 2012), or for the extreme poor (Bandiera et al., 2013, and Duflo and Banerjee 2013), can have transformational effects on their employment and income trajectories when they are of very high quality. In most countries, as in Chile, such programs are responsibility of the welfare system. Therefore, even if the frequent home visits provided by a program such as CS help diagnose the needs of each family, and stimulate their demand for social services, the extent to which their lives can improve depends on the quality of the programs that are made available to them, and the extent to which these programs are tailored to their needs.[3]

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. The mechanism of targeting and the structure of CS changed substantially in 2006, therefore our study focuses on the first five cohorts of entrants (2002-2006). The program is unique in that it recognizes that the provision of financial resources is not a sufficient condition for alleviating extreme poverty, since families in extreme poverty face multiple other constraints, starting with their inability to use the available welfare services. 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. We now provide more detail on the most important aspects 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, who make the link between families and the network of public and private services available to families. Each year, a social worker is responsible for 50 families on average (SD 25).

The home visiting period has 2 phases: the initial 6-8 months is a period of intensive work between families and counselor and the final 16-18 months are a follow-up period. During these

[3]Colombia introduced in 2007 a program similar to CS (*Juntos*), but which unfortunately suffered from a number of implementation and data 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.

home visits the social worker and families agree to fulfil 53 minimum conditions (see table A.1). The direct cost of home visits (including the cost of the visit itself, and the training of social worker, and supervision) amounts on average to USD$263 per family over the two years of in which these visits take place.

Guaranteed access to monetary subsidies Participating families receive a monthly cash transfer (called *Bono Chile Solidario*) during the first 24 months, with the size of the transfer ranging between USD$8 and USD$21 per month (decreasing over time).[4] For the subsequent three years, families receive the *Bono de Engreso* (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 conditional 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 CCT.[5]

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 Unico 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.

Preferential access to social services and the reorganization of the supply side Participating families have preferential access to a whole array of social services locally available in municipality of residence. Employment and training programs are of particular interest due to their potential effects on individuals' labor market outcomes. These programs fall into three categories: (i) job placement programs for wage employment, mainly job training programs and wage subsidies (ii) self-employment programs and support to micro-enterprizes, through a combination of technical assistance and seed funding for inputs and startup capital, and (iii) employability programs, which range from adult education equivalency and training focused on soft-skills. The employment and training programs available are described in table A.2 in the Appendix A.

Although families started receiving visits from social workers in 2002, the first cohorts of beneficiaries had to rely on the existing supply of services available in each municipality. Municipalities and local services providers were simply asked to improve the coordination of different programs serving the target population. After 2004 (when the law governing CS was passed), there was an improvement in the quantity and quality of the supply of such auxiliary services. The programs

[4]The 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.

[5]For example, depending on the family structure, the transfer from Mexican Oportunidades may exceed $150 per month, and the Bolsa Familia monthly transfer in Brazil varies between $40-$60 per family.

(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.[6] Additionally, the budget share allocated to the provision of programs to CS beneficiaries increased sixfold between 2003 and 2007 (Mideplan, 2009). As a result, the coverage of the potential demand[7] for employment programs among CS beneficiaries increased from 24% in 2004 to 100% in 2007. To understand the impact of the supply adjustment on the effectiveness of CS we disaggregate our analysis by cohorts of entry, depending on whether families entered the program before or after 2004.

Selection of families, Coverage and Cost The targeting instrument used to select families to CS was the Ficha CAS between 2002 and 2006 (see section 4 for details about Ficha CAS). From this instrument it is possible to construct the CAS score, according to which families are deemed eligible or not. The program was assigned geographically in proportion of the percentage of the population in extreme poverty in each municipality (P_c, estimated from the 2000 national household survey CASEN). 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_c.

In order to be eligible, a family needs to have a CAS score. Therefore, in the initial stages of CS, there was an effort to register indigent families with the CAS system. However, the new registration occurred only in a few isolated instances (see Larrañaga and Contreras, 2010). Due to capacity constraints not all eligible families were invited in the first year of operation. Thus, the initial plan was that 25% of all eligible families should be enrolled in CS in each year between 2002 and 2005, starting with families with lowest CAS values within the municipality. This sequencing implied that the cutoffs P_c were not binding in the first few of years of implementation of the program. Instead, within each municipality there was an effective threshold that varied across years. In section 3 we explain how this feature of the rollout of the program is used to identify its effects. Once invited, a family could reject or accept to participate. Acceptance was almost universal: out of all invited families only 4.7% did not participate (see table A.3 in the Appendix A).

3 Empirical Strategy

Our goal is to estimate β from the following equation:

$$Y_i = \alpha + \beta CS_i + f(X_i) + \varepsilon_i \tag{1}$$

[6] See table A.2 in the Appendix A.

[7] Potential demand is defined as the number of households who have not met the corresponding minimum condition at entry to the program.

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 $f(.)$), 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, the estimation of this equation by ordinary least squares (OLS) is problematic. Families who participate in CS are systematically different from those who do not in terms of their observable and unobservable characteristics.

On one end, participants in CS are indigent, and therefore they are on average much poorer than those who do not participate in CS. On the other end, not all indigent families participate in CS, and there may be differences between participants and non-participants among the indigents. Families who live in more remote areas may be harder to visit, making them less likely to be invited to 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 address these problems we use a regression discontinuity design, exploring 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.

A family is eligible for CS if its CAS score falls below a given cutoff, which varies across municipalities and time (as we explain below):

$$E_{imt} = 1[CAS_{imt} \leq \overline{CAS}_{mt}]$$

where E_{imt} is an indicator which takes value 1 if family i living in municipality m in year t is eligible for CS, CAS_{imt} is family i's CAS score, and $\overline{CAS}_{mt}$ is the CS eligibility cutoff in municipality m at time t. In each municipality and time period, we compare outcomes of families just below (just eligible) and just above (just ineligible) their respective cutoffs (see, for example, Hahn et al., 2001; Imbens and Lemieux, 2008; Lee and Lemieux, 2009).

An official set of CAS-cutoffs determining which families are eligible to the program was developed by the Ministry of Planning based on the income distribution in the municipality in 2000. As described in section 2, this cutoff was a function of the proportion of families who were found to be extreme poor in the national household survey carried out in 2000. We call this the *official* cutoff. However, because of capacity constraints, CS was implemented gradually, targeting the poorest families first (those with the lowest CAS scores within each municipality), and then moving up in the CAS distribution. Thus, the first families to be served had a CAS score substantially below the *official* municipality cutoffs, which were not binding in the first years of the program.

In practice, the way the program was rolled out between 2002 and 2006 was roughly the following. Once the annual funding for CS was set for each municipality, the number of beneficiaries was defined for that year. Given that priority should be given to those families with the lowest CAS scores, a local cutoff could then be implicitly defined, as the CAS score below which the number of eligible families was equal to the number of potential beneficiaries fixed for the year.[8]

[8]In practice there was some slippage because some individuals with low CAS scores were often very hard to

We call these the *effective* cutoffs (which are cohort and municipality specific). Over the years, the *effective* cutoffs converged to the *official* cutoffs.

Since these *effective* cutoffs are not observed, we follow a simple procedure proposed by Chay et al. (2005) to estimate them for each municipality and year of potential entry. This method searches across potential cutoff values for the one that best fits the participation data. The first step of this procedure is to construct different indicators of eligibility corresponding to different cutoff values for each municipality. Then, in each municipality, we regress participation in CS on eligibility. There is a separate regression for each potential cutoff value, defining a different potential eligibility variable. Finally, we define the *effective* cutoff for each municipality-year as the one that maximizes the fit of the regression.[9]

Figure B.1 in the Appendix B 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. We show in section 5 that the effective cutoffs are much stronger determinants of participation than the official cutoffs. 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 also very different across years. It drops from 16 points in 2002, to 2.9 points in 2005.[10]

Eligibility and Participation Many eligible families never enrol in CS. Table A.4 in Appendix A presents some of the main correlates of participation in CS, using only the sample of families who were eligible according to the official cutoff when they were first observed in the Ficha CAS (standard errors are clustered at the municipality level).

Families who are selected to CS are more likely to be connected to the welfare system, namely through the take-up of SUF. Within municipality, families who are selected to CS are less likely to have adequate walls or ceilings in their homes,[11] less likely to be legal occupants of their home, and more likely to have a connection to the sewage network. As expected, 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 (perhaps indicating a strong motivation to improve their life conditions, which also leads them to enrol in CS). 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.

reach, so it was difficult to set a schedule of regular home visits as required by CS.

[9]Figure B.10 plots the average participation by vingtiles of the distribution of CAS of each municipality in 2002. Although this figure includes only 25 municipalities in Chile the distributions presented are representative of what happened in the rest of the country. From this figure it is evident that the participation in CS is concentrated in the first two vingtiles of the CAS, with a sharp decline in participation thereafter.

[10]Figure B.2 plots the density of CAS-scores among the eligible families in the sample used in the regressions for families exposed to 2 years of CS, that is, those with CAS at most 20-points apart from the cutoff. The vertical lines in the figure are the 10th and 90th percentiles of the *effective* cutoffs. This figure shows the range of CAS over which we estimate the effects and the density of eligible families.

[11]See table A.5 for the definition of variables used.

There are also some ineligible families who are able to benefit from CS. This means that the mapping from eligibility to participation in CS is not perfect. We address this problem by presenting instrumental variables estimates of the program computed as described in expression (2) (for very small ε):

$$\frac{\lim_{\varepsilon \longrightarrow 0^+} \Pr\left(Y_i = 1 | CAS_{imt} = \overline{CAS}_{mt} - \varepsilon\right) - \lim_{\varepsilon \longrightarrow 0^+} \Pr\left(Y_i = 1 | CAS_{imt} = \overline{CAS}_{mt} + \varepsilon\right)}{\lim_{\varepsilon \longrightarrow 0^+} \Pr\left(CS_{imt} = 1 | CAS_{imt} = \overline{CAS}_{mt} - \varepsilon\right) - \lim_{\varepsilon \longrightarrow 0^+} \Pr\left(CS_{imt} = 1 | CAS_{imt} = \overline{CAS}_{mt} + \varepsilon\right)}. \quad (2)$$

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. All our comparisons of families in each side of the cutoff are done within municipality and time period. Our models include municipality-year effects, which absorb municipality-year shocks which may affect the outcome, independently of eligibility (for example, shocks in the local supply of social services, or shocks to the local labor market).

Once a family enrols in CS, it remains in the program for 5 years, 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. Therefore, for each year in which we measure eligibility we remove from the cohort of potential program entrants all families who are already enrolled in CS (because they are not affected by the eligibility cutoff in that year). This means that our estimates are valid for a sample which is changing over time (which could be an important issue if program impacts vary substantially across families).

In regression discontinuity designs it is standard practice to restrict the sample to those families whose CAS is near the cutoff for the program, since 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 (nonparametric) 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:

$$Y_{imk} = \phi + \gamma E_{im} + f(CAS_{im} - \overline{CAS}_m) + u_{imk} \quad (3)$$

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 $\left(CAS_{im} - \overline{CAS}_m\right)$, which can be different in either side of the cutoff, but we also present a robustness analysis using other parametric functions of distance to cutoff.

Then, we 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 $f(CAS_{im} - \overline{CAS}_m)$. In the second stage we estimate:

$$Y_{imk} = \alpha + \theta C\widehat{S_{imt-k}} + g(CAS_{im} - \overline{CAS}_m) + \varepsilon_{imk} \tag{4}$$

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. 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).

One potential problem of equation (4) 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 $t + 1$. 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. In 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 results presented in section 5 show that the estimates produced in a static model are similar to those in the dynamic model.

Specification checks 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.

Then, 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. Estimates are also similar if we include neighborhood fixed effects (neighborhoods are defined within 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), for whom we have pre-determined outcomes. Most of these robustness checks are included in Appendix A, but we refer to the most important ones in the main text.

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 people participating 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), so our panel spans data from March 1998 to December 2009, covering over 14 million individuals, corresponding to nearly 60 million observations (see more 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 important to construct eligibility for CS.[12]

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 FOSIS[13] between 2004 and 2007.

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).[14] 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. Given that the introduction of the new targeting mechanism was associated with new eligibility rules to CS

[12]The 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.

[13]FOSIS 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.

[14]See table D.1 in Appendix D for the 13 variables entering the CAS score.

in this paper we do not focus on the effects for families that entered in CS in 2007 or after. 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.[15]

Construction of sample The final sample 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 (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. Of 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.

4.1 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

[15]Relatively to the CAS, the FPS is a more comprehensive instrument and it includes much more detailed information than the CAS on the labor market situation of each family member, and health and education. Information about durables and housing was mostly dropped from the FPS.

To understand the dynamics of entry in the data, we present in table D.2 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.

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 (41% among eligible vs. 57%; 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.

4.2 Definition of Cohorts

The empirical strategy we lay out is clear in a static setting. However, our data is such that we observe families over several years. Each family can potentially enrol in CS in several years, or never at all.

Therefore, we define cohorts of potential entrants each year in the following way. Since 2002 is 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 the 2003 cohort, we consider every family in the CAS database in that year, but who has not enrolled in CS in any prior year. Past participants in CS should not be considered potential entrants into the program, and therefore we remove them from the sample. There are past CS participants on both sides of the set of CAS cutoff points in 2003.

If there were never new entrants into the CAS database, the families in the 2003 cohort would be a subset of families in the 2002 cohort. There are however entrants into the CAS database each year. Nevertheless there is still a large overlap in the set of families belonging to each cohort.

This does not mean that the overlap between the families in each cohort used in the estimation sample is equally large. The reason is that the cutoffs increase every year during the first 4 years of the program. Since we only take families with CAS scores contained within windows of 20 points on either side of the cutoff, it is possible that, for very large changes in cutoffs, there are substantial differences in the set of families used in the estimation of program impacts for each cohort (see Section 5.7).

When defining cohorts in subsequent years we proceed in an analogous way. In other words, we take all families who are in the CAS database in that year, and delete from the sample all those who have participated in CS in a prior year.

We estimate models with and without pooling all cohorts. When we pool them, we restrict the coefficients of the model to be the same across all cohorts.

5 Results

5.1 Simple Statistics from Program Data

Before turning to our main empirical results, we use program data to document to what extent participants in CS are able to fulfil the minimum conditions discussed in section 2, during their time in the program. This data is produced by the social workers, who during their periodic visits to CS families, record whether each of the 53 minimum conditions is or not fulfilled.

Although we did not gain access to all the records produced in each visit by each social worker, we were able to access four snapshots of this register: December 2003, September 2004, September 2005, and August 2006. For each family in the program in each of these dates, we have information about the fulfillment of 47 out of the 53 minimum conditions. We also have the diagnostic performed in the first visit, which shows the extent to which the family fulfills these conditions at the time it enters the program (more details on this data are provided in the Appendix D).

Since different families have been in CS for different amounts of time in each of the four snapshots of data available to us, we can reorganize this data to compute the proportion of families fulfilling each of the minimum conditions when they first enter the program, and how this proportion evolves as families spend more time in CS. We plot this in figure B.3 in Appendix B. We consider measurements taken up to 36 months after entry into the program. In order to compare the progress on these outcomes comparable with our main RD analysis we restrict the sample to families whose CAS at entry in CS was at most 20-points apart from the cutoff.[16] There is wide variation in the extent to which these minimum conditions are satisfied at entry (see also table A.1 in Appendix A). However, 36 months after program entry, it is striking how the proportion of households satisfying every one of them is very close to 1. This data suggests therefore, that CS is quite successful in improving the lives of its participants.

However, it is difficult from the raw data to separate the role of aggregate trends, differences across cohorts, and exposure to the program. Therefore, using this same sample, we estimate the following equation:

$$Y_{fmkt} = \alpha + \sum_{t=0}^{36} \theta_t T_{fmkt} + f(CAS_{fmk}) + \eta_f + \pi_k + \tau_{month} + \zeta_m + \nu_{fmkt} \qquad (5)$$

where Y_{fmkt} is an indicator which is equal to 1 if a given minimum condition is fulfilled for family f, residing in municipality m when entered CS, k is the year of entry (2002 to 2006) and $month$ is the month when the Puente data we have access to was recorded (August, September or December). η_f is a family fixed effect. We also include indicators for the number of months since entry in Chile Solidario (T_{fmkt}), indicators for month of Puente survey (τ_{month}), cohort of entry (π_k) and

[16]Note that we observe the CAS-score of families at entry in CS.

municipality of residence (ζ_m), and a cubic on CAS at entry ($f(CAS_{fmk})$)[17]. We run one for these regressions for each minimum condition.

Figure 1 plots our estimates of θ_t for 11 of the 47 minimum conditions for which we have data on. These conditions were chosen since they closely resemble some of our main outcome variables from Ficha CAS and FPS, namely take-up of public subsidies, employment and housing (see table A.5 in Appendix A; figure B.4 in Appendix B includes estimates for the 47 minimum conditions). Identification of these parameters comes only from within family variation, although we assume that any underlying trends in the economy are common to all cohorts. The plots of figure 1 are suggestive about when to expect the largest changes in the the minimum conditions according to the period of exposure to the program. Regularization of housing tenure and access to clean water are minimum conditions that take the longest to be improved. Take-up of SUF and enrolment in the public health system exhibit the largest changes during the two years of psychosocial support with the health workers.

Although these results provide an interesting first image of the impacts of CS in the lives of the households, there are two important limitations to this data. First, this dataset only includes participants. It is still possible to look at the progression of each family within the CS program, and to remove other trends in the economy from this progression by exploring the fact that different families start in different time periods. But this can only be done under the assumption that these trends are common to all entry cohorts, which may not be quite true.

A perhaps more serious problem concerns the way this data is recorded. Whenever a minimum condition is fulfilled at a given point in time, the corresponding indicator becomes 1 in that period, and in all subsequent periods. Although we cannot verify this independently, it is plausible that even if a family fulfills a given minimum condition at a given point in time (for example, *at least one household member works regularly*), it may not be able to fulfill it in the future. However, in the data we never see such reversals. Therefore, the most accurate way to interpret each of our outcomes variables is not as an indicator of whether the minimum condition is satisfied at that point, but as an indicator of whether the minimum condition was ever satisfied at any date between the entry into CS and the current date, even if it is no longer satisfied at present. If that is the case, we are likely to overstate the positive impacts of the program by looking at this data.

We now turn to an alternative method to estimate program impacts (discussed in section 3), and also an alternative dataset (discussed in section 4). We are not able to study nearly as many outcomes, but we are able to provide robust estimates of the impact of CS on a smaller set of variables.

[17]The municipality fixed effects and controls in CAS at entry are subsumed by the family fixed effects.

5.2 Eligibility and Participation in CS

We start by showing how eligibility for CS predicts participation in the program. Panel A of Figure 2 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. These are small bins in terms of the distribution of CAS (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 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.

Table 2 complements these figures, by showing estimates of equation (3), 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.

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 discontinuities around the *official* cutoff are statistically significant, but much smaller in magnitude. Panel B of Figure 2 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.

5.3 Intent-to-Treat Estimates of Program Impacts

It is useful to start with simple intent-to-treat (ITT) estimates, because they 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 show 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 a restricted sample). In section 4.2 we allow the effects to vary by year of entry into the program. All variable definitions are given in table A.1 in the appendix.

The different panels in figures 3 and 4 show estimates of the relationship between outcomes and the distance to the municipality and cohort specific cutoffs, $(CAS_{im} - \overline{CAS}_m)$. 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 enrolment (figure B.5 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 $(CAS_{ijmt} - \overline{CAS}_{mt} \leq 0)$ and ineligible $(CAS_{ijmt} - \overline{CAS}_{mt} > 0)$ families.[18] The figures also include 95% confidence intervals.

Figure 3 suggests that there is an increase in the take-up of SUF and SAP at the eligibility cutoff. We show below that the regression estimates are statistically significant for SUF,[19] even though there is overlap in the pointwise confidence intervals on each side of the discontinuity. The two middle panels in the figure concern the take-up of FOSIS (the employment programs), 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.[20] In the regressions below we can reject the null hypothesis that these impacts are equal to zero. Finally, the bottom panels concern the probability of legal ownership of the house, and access to public water networks. There is no statistically significant difference in these outcomes between families on either side of the discontinuity point.

Figure 4 concerns the employment status of head and spouse two years after entry in the program. We consider three mutually exclusive labor market states: not employed, self-employed, or wage worker. The panels on the left concern employment outcomes of the head of the household,

[18] We use a bandwidth equal to 8. We have also tried using bandwidths equal to 6 and 10, which resulted in fairly similar figures, see figures B.6 and B.7 in the appendix, respectively.

[19] 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.

[20] We exclude participation in those programs which were especially created to serve members of families in CS. See table A.2 in Appendix for the exact programs included.

while those on the right concern the spouse. Although there are some differences in a few outcomes at the discontinuity point, none of them is statistically different from zero.

Table 3 presents estimates of γ in equation (3) for different outcomes (where, as above, $f(CAS_{im} - \overline{CAS}_m)$ is a quadratic polynomial in its argument, with different coefficients on each side of the cutoff. We include municipality-year specific fixed effects in all regressions. There are five columns in table 3. The first one shows the mean of the variable being considered for the sample of just ineligible families (with CAS scores at most 4 points above the cutoff). The second one shows ITT estimates measured 2 years after program enrolment (short run), corresponding to the results which were just presented in graphs. The fourth column consists of ITT estimates measured 4 years after enrolment (medium run). Columns (3) and (5) include the exact years of data used in the estimation. When we estimate longer-run impact estimates our sample size becomes smaller, because we only have data up to 2009. 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).[21]

Table 3 shows 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. Below we show that, for particularly disadvantaged families, some of these impacts are sustained even in the medium run. Separating program impacts by type of family, according to their pre-program conditions, is very important, as shown in section 5.6.

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. These are readily available to CS families, who are poor, and therefore eligible to receive them. In the first few visits to CS families, the social worker should be able to provide information about the programs and subsidies each family is entitled to, how they can benefit the family, and at the same time, help them register for these programs. In that sense, CS was able to significantly contribute to its main goal, which was an approximation 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 and use of other social programs.[22]

[21]The critical values are adjusted for a two sided test. Throughout the paper we use 250 bootstrap replications to obtain the adjusted critical values.

[22]Additionally, the lack of effects on housing may reflect the strong constraints in the provision on support on this dimension (see Mideplan, 2010).

5.4 Balancing Checks

In studies using regression discontinuity design it is standard to assess the extent to which there is balance in the observable characteristics of individuals in each side of the cutoff. Substantial imbalances may indicate that individuals were able to manipulate their score to be in the most favorable side of the discontinuity. Thus, we estimate equation (3) using as dependent variables pre-determined characteristics that should not be affected by the program (see Lee and Lemieux, 2010).

Table 4 shows the results for different variables (using the same specification as in table 3). The table has 5 columns: one showing the mean of each variable for just ineligible families (column 1), columns 2 and 4 include the size of the sample used (which is the same for each outcome in the tables 3 and 4), column 3 presents the estimates with the sample used in the short run regressions, and column 5 uses the sample of the medium run regressions. All the outcome variables are measured either in 2000 (or 2001 if the family has no information for 2000), before CS was ever implemented.

There is no statistically significant estimate in this table after accounting for multiple hypothesis testing, suggesting that individuals located just below and just above each cutoff are similar in terms of observable pre-determined variables. Therefore, our empirical strategy is likely to be valid.[23]

5.5 Instrumental Variables

We show in section 5.2 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. Therefore, as discussed in section 3, the impacts of program participation are estimated by instrumental variables (IV).

Table 5 shows the IV estimates corresponding to the ITT estimates in table 3. Again, inference is adjusted for multiple hypothesis testing using the procedure in Romano and Wolf (2005). Table 5 includes IV estimates for short (columns 1 and 2) and medium run (columns 3 and 4) effects of the program.

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.[24] There are substantial program

[23]The different panels of figure B.8 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.

[24]As in any RD estimator, we are only able to identify program impacts for those families located near the

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 very large: 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 the share of vacancies of such programs that were exclusively targeted to individuals in CS families increase over time since 2004. Impacts in the medium run are smaller and statistically insignificant.

For all other outcomes there are no statistically important impacts of CS on average. However, these average effects mask important heterogeneity of impact depending on the initial conditions and the cohorts of potential entry, which we study in the next section.[25]

5.6 Differential Impacts Across Groups

In this section we show that there are substantial differences in program impacts across different types of families. We consider two dimensions of heterogeneity. First, we examine whether there are differential impacts for individuals who were in a vulnerable situation before participating in the program. 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.

Subsidies We start by dividing families according to whether they have any recorded access to subsidies 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.[26] The reason for doing this is that the take-up of SUF before the existence of the program 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 large 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

cutoffs. However, we have a fairly large range of cutoffs (as shown in figure B.1 in Appendix), the only families we are unable to span are extremely poor, with CAS values below 400 (the lowest cutoff in our data). This corresponds to 0.12% of families in the CAS registers.

[25]There 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 Vytlacil, 2005).

[26]The 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. Kleven and Kopczuk, 2011, model imperfect take-up as a response to program complexity and administrative hassle.

from the welfare system.

Table 6 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 (3) and (4). The table includes two panels each with three 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. CS has very high and sustained impacts on the take-up of SUF for those without prior take-up of this subsidy, but it has virtually no impact on families who have taken up this subsidy before. The difference between the two groups is large and statistically significant. 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. At the same time, there are virtually no impacts of CS on the take-up of SAP, regardless of whether families have taken up this subsidy before or not.[27]

It is also important to examine whether there are any differences in program impacts between cohorts entering the program before and after 2004. This is because 2004 marks the date after which the supply component of CS was effectively implemented, leading to an increase in the availability of social services and subsidies for CS families. Differences in program impacts across cohorts could result from differences that they encountered in the supply of social services. In order to study this issue, we divide the sample into two additional groups: the 2002-2004 cohorts, and the 2005-2006 cohorts (where cohorts are defined as was described in section 4.2).

Table 7 presents estimates of the effect of the program two years into CS, and it includes two columns, one for the ITT estimates other for the IV estimates. We allow program impacts to vary with the (potential) year of entry.[28] There are three panels. Panel A considers the whole sample, Panel B takes only families who have not taken-up SUF neither in 2000 nor in 2001, and Panel C includes families who have taken-up SUF prior to 2002. Starting with the first panel, the IV estimates of program impacts for the later cohorts are almost twice as large as the estimates for the early cohorts. Panel B shows that this is mainly driven by 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 B 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). Of course, we cannot 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 (see the p-value in the bottom of column 1).[29]

[27] See table A.6. 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.

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

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

Employment Programs We next re-examine the impacts of CS on the take-up of employment (FOSIS) programs. Unfortunately, we do not have information about the take-up of these programs before 2002, and therefore cannot do an analysis similar to the one presented for SUF. However, one interesting 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. In order to study this question 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 8 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 (after accounting for multiple hypothesis testing) 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.[30]

Labor Market Outcomes We now turn to the labor market outcomes of both heads and their spouses. We ask 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. Table 9 shows no statistically significant impacts in any of these variables, and for any of the groups in Panels A (for heads) and B (for spouses).

Table 10 presents program impacts on the employment of the head (left) and spouse (right) by cohort of entry. We 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). We consider short and medium run impacts for both sets of cohorts. The only group for which we find any statistically important impacts on employment (after accounting for multiple hypothesis testing) are spouses (who females in 98% of the cases), who were not employed before 2002, and who have entered the program after 2004. The effects on the later cohorts are statistically different than those in early cohorts (p-value is 0.02).

Therefore, this suggests that CS impacts the employment of a particular demographic group. With an average participation of 30%, female labor force participation in Chile is low even by Latin American standards. In this context, CS coupled with the support of employment programs could constitute an important avenue to improve female labor market participation. Preferential access to child care might have also explained this improvement, but there are no studies to support a causal link between access to public day care centers and labor force participation in Chile (Medrano, 2009).

[30] Table A.7 shows cohort differences for individuals with and without pre-program employment, but there are no statistically significant differences across cohorts.

Other Outcomes We considered two other sets of outcomes: housing and variables associated with behaviors. Since the effects we find in most of these outcomes are not statistically significant (even without adjusting inference for multiple hypothesis testing), we opted to include the results in Appendix A.

There are seven outcome 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). There are also nine additional 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 only one of these variables where we find a positive impact of participation in CS indicates whether all adults aged 65 or above in the family have completed health check-ups (see table A.10).

5.7 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. Before we turn to that, we also discuss whether our estimates of the impact of the program vary according to the level of the threshold affecting each set of households. This could be especially interesting if the impacts of CS vary substantially across families at different levels of poverty. All results discussed here refer to reduced for estimates taken two years after (potential) program entry.

Heterogeneity by baseline CAS We allowed the effects of CS to vary with CAS to understand which families drive the effects found on employment of head and participation in SUF (see figure B.9 in Appendix). We estimate a version of model 3 where we use CAS as running variable instead of distance to cutoff. The left hand side of figure B.9 in Appendix shows that the increase on the take-up of SUF is mainly driven by families whose CAS score at (potential) entry in CS was at least 500 points. The right hand side of the same figure presents estimates for the employment status of the head of family. CS is associated with a decrease in the probability of employment of head for families whose CAS score was below 460 points at in intake and no effect for families with CAS between 460 and 584 points (although not shown, the negative effect on employment status of head detected for those families on the left hand side of the distribution of CAS is driven by families whose head was employed prior to 2002).

Sensitivity to functional form and sample size Table A.11 shows robustness to the choice of functional form of running variable. We present ITT estimates for four specifications: (1) basic, (1) using cubic and quartic in distance to cutoff as running variable, and (4) using quadratic of

CAS as running variable. There are hardly any changes in our main results.

Table A.12 shows the sensitivity of our results to using different windows of data around the discontinuity. The table includes 10 columns for five possible windows of data around the cutoff: 10-points (columns 1 and 2), 20-points (columns 3 and 4; our basic sample), 30-points (columns 5 and 6), 50-points (columns 7 and 8) and 100-points (columns 9 and 10). Our results are robust to the choice of window of data around the discontinuity points.

Table A.13 presents estimates using different sets of fixed effects for the place of residence (at potential entry). The table includes four columns: the first column has the estimates for our basic model (which controls for municipality-year effects); column (2) includes separatively municipality and year fixed effects; column (3) includes separatively neighborhood (which in Chile is the location unit immediately below municipalities) and year fixed effects; and, finally, column (4) controls for 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 for looking at heterogeneous program 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.14).

Dynamic Regression Discontinuity As discussed in section 3, the gradual rollout of CS means that subsequent program entry by ineligibles can lead to underestimate program impacts, since we assume that the initial group of ineligibles 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. In the appendix we implement a version of the dynamic regression discontinuity estimator of Cellini, Ferreira and Rothstein (2010), and show that the estimates are not substantially different from our main results (see table C.1).

6 Conclusion

Banerjee and Duflo (2007) and Duflo (2011) 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 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 very supportive social workers, with whom frequent meetings are schedule. At the same time, there is a strong effort to make immediately available to the poor all the social programs that can help their situation and to which they are entitled.

CS is, in its conception, a very ambitious program. In its implementation it is pragmatic, and 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, especially for those families who were most alienated from the system to start with. Our estimated results are encouraging. 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 from those individuals who were unemployed or outside the labor force at the onset of the program.

The impacts of CS on broader set of outcomes such as employment or housing are much more limited, and generally not statistically different from zero. One mechanism we explore in the analysis to explain the lack of average effects are the differences in supply side availability. 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. Supply side availability however, may not be enough. It is possible that a more intensive version of CS is needed to achieve the required transformation, whereby the psychosocial support is combined with larger transfers or a stronger focus on the scope and quality of programs targeted to this population.

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Tables

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

	(1)	(2)	(3)	(4)	(5)	(6)
		All			Ever eligible	
Variable	N	Mean	S.D.	N	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).

Table 2: First Stage Estimates.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Year of entry	2002		2003		2004		2005		2006	
Cutoff	Effective	Official	Effective	Official	Effective	Official	Effective	Official	Effective	Official
1[Eligible]	0.121***	0.018***	0.140***	0.058***	0.153***	0.087***	0.166***	0.101***	0.217***	0.122***
	(0.008)	(0.003)	(0.007)	(0.007)	(0.008)	(0.008)	(0.009)	(0.010)	(0.012)	(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%.

Table 3: Impact of CS: ITT estimates for the whole sample.

	(1)	(2)	(3)	(4)	(5)
Years after start		2		4	
	C. Mean	ITT	Sample	ITT	Sample
Participation					
SUF	0.648	0.023***	2004-2008	0.017	2006-2008
		(0.007)		(0.011)	
SAP	0.166	0.002	2004-2006	0.013	2006
		(0.006)		(0.014)	
Labor market programs - FOSIS (head)	0.010	0.004	2004-2007	-0.001	2006-2007
		(0.002)		(0.001)	
Labor market programs - FOSIS (spouse)	0.016	0.006**	2004-2007	0.004	2006-2007
		(0.003)		(0.002)	
Labor market					
Labor market participation (head)	0.667	0.010	2007-2008	0.003	2007-2009
		(0.011)		(0.007)	
Employed (head)	0.283	0.009	2004-2008	0.001	2006-2009
		(0.006)		(0.007)	
Self-employed (head)	0.532	0.005	2004-2008	0.001	2006-2009
		(0.006)		(0.007)	
Dependent worker (head)	0.185	0.005	2004-2008	0.000	2006-2009
		(0.005)		(0.007)	
Formal Worker (head)	0.204	0.009	2007-2008	-0.006	2007-2009
		(0.011)	(0.007)		
Labor market participation (spouse)	0.170	0.011	2007-2008	0.005	2007-2009
		(0.011)		(0.008)	
Employed (spouse)	0.148	0.003	2004-2008	0.010	2006-2009
		(0.006)		(0.010)	
Self-employed (spouse)	0.107	0.003	2004-2008	0.006	2006-2009
		(0.005)		(0.008)	
Dependent worker (spouse)	0.041	0.000	2004-2008	0.004	2006-2009
		(0.004)		(0.006)	
Formal Worker (spouse)	0.078	-0.002	2007-2008	0.005	2007-2009
		(0.010)	(0.007)		
Housing					
Legal occupation of house	0.529	-0.011	2004-2008	-0.010	2006-2009
		(0.006)		(0.007)	
Sewage connected	0.370	0.005	2004-2006	0.012	2006
		(0.005)		(0.011)	
Water from public network	0.687	0.006	2004-2008	0.008	2006-2009
		(0.005)		(0.005)	
Adequate roof	0.403	0.005	2004-2006	-0.017	2006
		(0.007)		(0.016)	
Adequate walls	0.279	0.002	2004-2006	-0.016	2006
		(0.006)		(0.016)	
Heating	0.064	0.000	2004-2006	-0.015	2006
		(0.004)		(0.009)	
Fridge	0.459	-0.002	2004-2006	0.018	2006
		(0.010)		(0.015)	

Note: The table presents the estimated coefficients (and standard errors) on eligibility (measured 2 or 4 years before the outcome) for model 3. 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 4: Balancing checks: outcomes measured in 2000, or 2001 if missing in 2000.

	(1)	(2)	(3)	(4)	(5)
Years after start		2		4	
	C. Mean	N	ITT	N	ITT
Participation					
SUF	0.576	116,163	0.000	66,171	-0.005
			(0.009)		(0.012)
SAP	0.133	129,967	0.007	19,475	0.029
			(0.005)		(0.016)
Labor market					
Employed (head)	0.775	198,464	0.012	165,689	0.010
			(0.006)		(0.007)
Self-employed (head)	0.629	198,464	0.004	165,689	0.008
			(0.006)		(0.007)
Dependent worker (head)	0.146	198,464	0.008	165,689	0.003
			(0.005)		(0.005)
Employed (spouse)	0.108	116,872	-0.007	73,751	0.001
			(0.006)		(0.007)
Self-employed (spouse)	0.081	116,872	-0.005	73,751	0.004
			(0.005)		(0.006)
Dependent worker (spouse)	0.026	116,872	-0.002	73,751	-0.003
			(0.003)		(0.004)
Housing					
Legal occupation of house	0.465	202,627	-0.004	174,269	-0.001
			(0.007)		(0.007)
Sewage connected	0.202	214,136	0.007	41,893	0.012
			(0.005)		(0.011)
Water from public network	0.662	202,627	0.006	174,270	0.005
			(0.005)		(0.005)
Adequate roof	0.360	141,943	0.009	31,850	-0.003
			(0.007)		(0.014)
Adequate walls	0.261	141,943	0.009	31,850	-0.009
			(0.007)		(0.015)
Heating	0.0249	141,943	0.000	31,850	-0.000
			(0.003)		(0.004)
Fridge	0.304	141,943	0.005	31,850	-0.008
			(0.007)		(0.013)

Note: The table presents the estimated coefficients (and standard errors) on eligibility (measured 2 or 4 years before the main outcome) for model 3. The outcomes are measured in 2000 (or in 2001 if the family does not have CAS information in 2000) and the sample used it is the same as in table 3. 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, that is, the 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 testing using the procedure in Algorithms 4.1 and 4.2 of Romano and Wolf, 2005).

Table 5: Impact of CS: IV estimates for the whole sample.

	(1)	(2)	(3)	(4)
Years after start	2		4	
	IV	Sample	IV	Sample
Participation				
SUF	0.110***	2004-2008	0.090	2006-2008
	(0.033)		(0.058)	
SAP	0.012	2004-2006	0.080	2006
	(0.033)		(0.097)	
Labor market programs - FOSIS (head)	0.023**	2004-2007	-0.004	2006-2007
	(0.008)		(0.009)	
Labor market programs - FOSIS (spouse)	0.039***	2004-2007	0.022	2006-2007
	(0.012)		(0.014)	
Labor market				
Labor market participation (head)	0.040	2007-2008	0.017	2007-2009
	(0.046)		(0.039)	
Employed (head)	0.051	2004-2008	0.008	2006-2009
	(0.029)		(0.037)	
Self-employed (head)	0.026	2004-2008	0.007	2006-2009
	(0.030)		(0.039)	
Dependent worker (head)	0.025	2004-2008	0.001	2006-2009
	(0.025)		(0.038)	
Formal Worker (head)	0.034	2007-2008	-0.031	2007-2009
	(0.045)		(0.035)	
Labor market participation (spouse)	0.042	2007-2008	0.026	2007-2009
	(0.043)		(0.041)	
Employed (spouse)	0.015	2004-2008	0.051	2006-2009
	(0.029)		(0.051)	
Self-employed (spouse)	0.014	2004-2008	0.038	2006-2009
	(0.025)		(0.041)	
Dependent worker (spouse)	0.002	2004-2008	0.013	2006-2009
	(0.019)		(0.033)	
Formal Worker (spouse)	-0.011	2007-2008	0.019	2007-2009
	(0.041)	(0.032)		
Housing				
Legal occupation of house	-0.059	2004-2008	-0.055	2006-2009
	(0.032)		(0.039)	
Sewage connected	0.029	2004-2006	0.085	2006
	(0.030)		(0.076)	
Water from public network	0.031	2004-2008	0.045	2006-2009
	(0.024)		(0.028)	
Adequate roof	0.024	2004-2006	-0.097	2006
	(0.038)		(0.106)	
Adequate walls	0.004	2004-2006	-0.086	2006
	(0.035)		(0.108)	
Heating	0.001	2004-2006	-0.093	2006
	(0.020)		(0.059)	
Fridge	0.066	2004-2006	0.137	2006
	(0.040)		(0.100)	

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 4. 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 - see Romano and Wolf, 2005).

Table 6: Participation in SUF, by initial conditions (cohorts 2002-2006).

	(1)	(2)
Years after start	2	4
	Panel A: Not receiving SUF before 2002	
Eligibility (ITT)	0.034**	0.040***
	(0.013)	(0.018)
Participation (IV)	0.180***	0.220***
	(0.064)	(0.096)
Control Mean	0.494	
	Panel B: Receiving SUF before 2002	
Eligibility (ITT)	0.019	0.002
	(0.008)	(0.013)
Participation (IV)	0.091**	0.015
	(0.035)	(0.067)
Control Mean	0.762	
P-Value: HA: $\beta_0^{iv} > \beta_1^{iv}$	0.164	0.000

Note: See table 3 for a description of specification used in rows named (ITT) and see table 5 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}.
The last row presents the p-value for the null hypothesis that the effect for those without SUF prior 2002 equals the effect on those receiving SUF, H0: $\beta_0^{iv} = \beta_1^{iv}$, against the alternative that the effect is larger for those that did not receive SUF 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 7: Participation in SUF, by cohorts and initial conditions 2 years after start.

	(1)	(2)
Cohort	2002-2004	2005-2006
	Panel A: All sample	
Eligibility (ITT)	0.016	0.039**
	(0.008)	(0.016)
Participation (IV)	0.090**	0.152**
	(0.042)	(0.061)
Control Mean	0.648	0.648
P-Value: HA: $\beta^{iv}_{0506} > \beta^{iv}_{0204}$	0.268	
	Panel B: Not receiving SUF before 2002	
Eligibility (ITT)	0.018	0.071**
	(0.015)	(0.026)
Participation (IV)	0.106	0.318**
	(0.085)	(0.060)
Control Mean	0.481	0.524
P-Value: HA: $\beta^{iv}_{0506} > \beta^{iv}_{0204}$	0.060	
	Panel C: Receiving SUF before 2002	
Eligibility (ITT)	0.020*	0.016
	(0.009)	(0.019)
Participation (IV)	0.103**	0.055
	(0.047)	(0.065)
Control Mean	0.765	0.753
P-Value: HA: $\beta^{iv}_{0506} > \beta^{iv}_{0204}$	0.736	
P-Value: HA: $\beta^{iv}_{0} > \beta^{iv}_{1}$	0.496	0.008

Note: See table 3 for a description of specification used in rows named (ITT) and see table 5 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}.

The p-values in panels A, B and C concern 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, H0: $\beta^{iv}_{0506} = \beta^{iv}_{0204}$, against the alternative that the effect is larger for those entering in the later year, HA: $\beta^{iv}_{0506} > \beta^{iv}_{0204}$. The last row presents the p-value for the null hypothesis that the effect for those without SUF prior 2002 equals the effect on those receiving SUF, H0: $\beta^{iv}_{0} = \beta^{iv}_{1}$, against the alternative that the effect is larger for those that did not receive SUF prior to 2002, HA: $\beta^{iv}_{0} > \beta^{iv}_{1}$.

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 adjusted for multiple hypotheses testing using the procedure in Algorithms 4.1 and 4.2 of Romano and Wolf, 2005).

Table 8: Participation in labor market programs (FOSIS), by initial conditions (cohorts 2002-2006).

	(1)	(2)	(3)	(4)
Variable	Head		Spouse	
Years after start	2	4	2	4
	Panel A: Not employed before 2002			
Eligibility (ITT)	0.010***	0.004	0.009***	0.003
	(0.003)	(0.003)	(0.003)	(0.003)
Participation (IV)	0.062***	0.024	0.050***	0.016
	(0.016)	(0.021)	(0.013)	(0.015)
Control Mean	0.008		0.015	
	Panel B: Employed before 2002			
Eligibility (ITT)	0.002	-0.002	-0.014	0.009
	(0.002)	(0.002)	(0.009)	(0.007)
Participation (IV)	0.014	-0.010	-0.068	0.071
	(0.009)	(0.010)	(0.051)	(0.052)
Control Mean	0.010		0.028	
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 named (ITT) and see table 5 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}.
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, H0: $\beta_0^{iv} = \beta_1^{iv}$, 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 9: 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	-0.009	0.010	-0.014	-0.012	0.005
	(0.013)	(0.016)	(0.012)	(0.015)	(0.007)	(0.010)
Participation (IV)	-0.005	-0.054	0.058	-0.082	-0.063	0.028
	(0.069)	(0.097)	(0.062)	(0.088)	(0.035)	(0.057)
Control Mean	0.757		0.183		0.060	
	Panel A2: Employed before 2002					
Eligibility (ITT)	0.005	-0.004	-0.001	0.001	0.007	-0.005
	(0.005)	(0.006)	(0.006)	(0.008)	(0.006)	(0.008)
Participation (IV)	0.027	-0.022	-0.007	0.006	0.035	-0.028
	(0.025)	(0.034)	(0.031)	(0.045)	(0.030)	(0.043)
Control Mean	0.153		0.628		0.219	
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: Employed before 2002					
Eligibility (ITT)	0.004	0.010	0.004	0.006	0.001	0.004
	(0.005)	(0.010)	(0.005)	(0.008)	(0.003)	(0.007)
Participation (IV)	0.022	0.057	0.017	0.038	0.004	0.019
	(0.028)	(0.049)	(0.024)	(0.040)	(0.017)	(0.035)
Control Mean	0.886		0.082		0.032	
	Panel B2: Not Employed before 2002					
Eligibility (ITT)	0.005	0.007	-0.002	0.003	0.007	0.005
	(0.025)	(0.032)	(0.025)	(0.032)	(0.016)	(0.022)
Participation (IV)	0.014	0.021	-0.013	0.009	0.027	0.012
	(0.133)	(0.213)	(0.132)	(0.209)	(0.086)	(0.148)
Control Mean	0.589		0.300		0.111	
P-Value: HA: $\beta_0^{iv} > \beta_1^{iv}$	0.516	0.620	0.412	0.188	0.604	0.680

Note: See table 3 for a description of specification used in rows named (ITT) and see table 5 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}. 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, H0: $\beta_0^{iv} = \beta_1^{iv}$, 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 10: Labor market outcomes, by cohorts and initial conditions 2 and 4 years after start.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variable	Head Employed				Spouse Employed			
Years after start	2		4		2		4	
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	0.014	-0.004	0.016	-0.004	0.037	0.002	0.040
	(0.007)	(0.011)	(0.008)	(0.013)	(0.006)	(0.017)	(0.011)	(0.022)
Participation (IV)	0.046	0.061	-0.026	0.077	-0.022	0.166	0.010	0.180
	(0.039)	(0.048)	(0.051)	(0.061)	(0.034)	(0.079)	(0.064)	(0.097)
Control Mean	0.720	0.709	0.737	0.707	0.119	0.277	0.242	0.273
P-Value: HA: $\beta^{iv}_{0506} > \beta^{iv}_{0204}$	0.408		0.100		0.016		0.068	
Panel B: Not employed before 2002								
Eligibility (ITT)	0.006	-0.026	-0.011	-0.005	-0.004	0.044**	0.002	0.045
	(0.015)	(0.028)	(0.017)	(0.033)	(0.006)	(0.019)	(0.011)	(0.023)
Participation (IV)	0.039	-0.127	-0.080	-0.025	-0.021	0.198**	0.009	0.198
	(0.086)	(0.136)	(0.119)	(0.157)	(0.032)	(0.084)	(0.061)	(0.099)
Control Mean	0.225	0.296	0.300	0.306	0.0875	0.236	0.202	0.236
P-Value: HA: $\beta^{iv}_{0506} > \beta^{iv}_{0204}$	0.836		0.376		0.020		0.060	
Panel C: Employed before 2002								
Eligibility (ITT)	0.002	0.013	-0.010	0.015	0.007	-0.004	0.008	0.006
	(0.006)	(0.011)	(0.007)	(0.015)	(0.028)	(0.057)	(0.037)	(0.066)
Participation (IV)	0.009	0.058	-0.065	0.071	0.044	-0.023	0.074	0.032
	(0.036)	(0.048)	(0.046)	(0.069)	(0.173)	(0.272)	(0.323)	(0.318)
Control Mean	0.863	0.808	0.839	0.799	0.379	0.506	0.513	0.488
P-Value: HA: $\beta^{iv}_{0506} > \beta^{iv}_{0204}$	0.208		0.060		0.556		0.552	
P-Value: HA: $\beta^{iv}_{0} > \beta^{iv}_{1}$	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 named (ITT) and see table 5 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}. 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, H0: $\beta^{iv}_{0506} = \beta^{iv}_{0204}$, against the alternative that the effect is larger for those entering in the later year, HA: $\beta^{iv}_{0506} > \beta^{iv}_{0204}$. 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, H0: $\beta^{iv}_{0} = \beta^{iv}_{1}$, against the alternative that the effect is larger for those not employed prior to 2002, HA: $\beta^{iv}_{0} > \beta^{iv}_{1}$. 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 adjusted for multiple hypotheses testing - see Romano and Wolf, 2005).

Figures

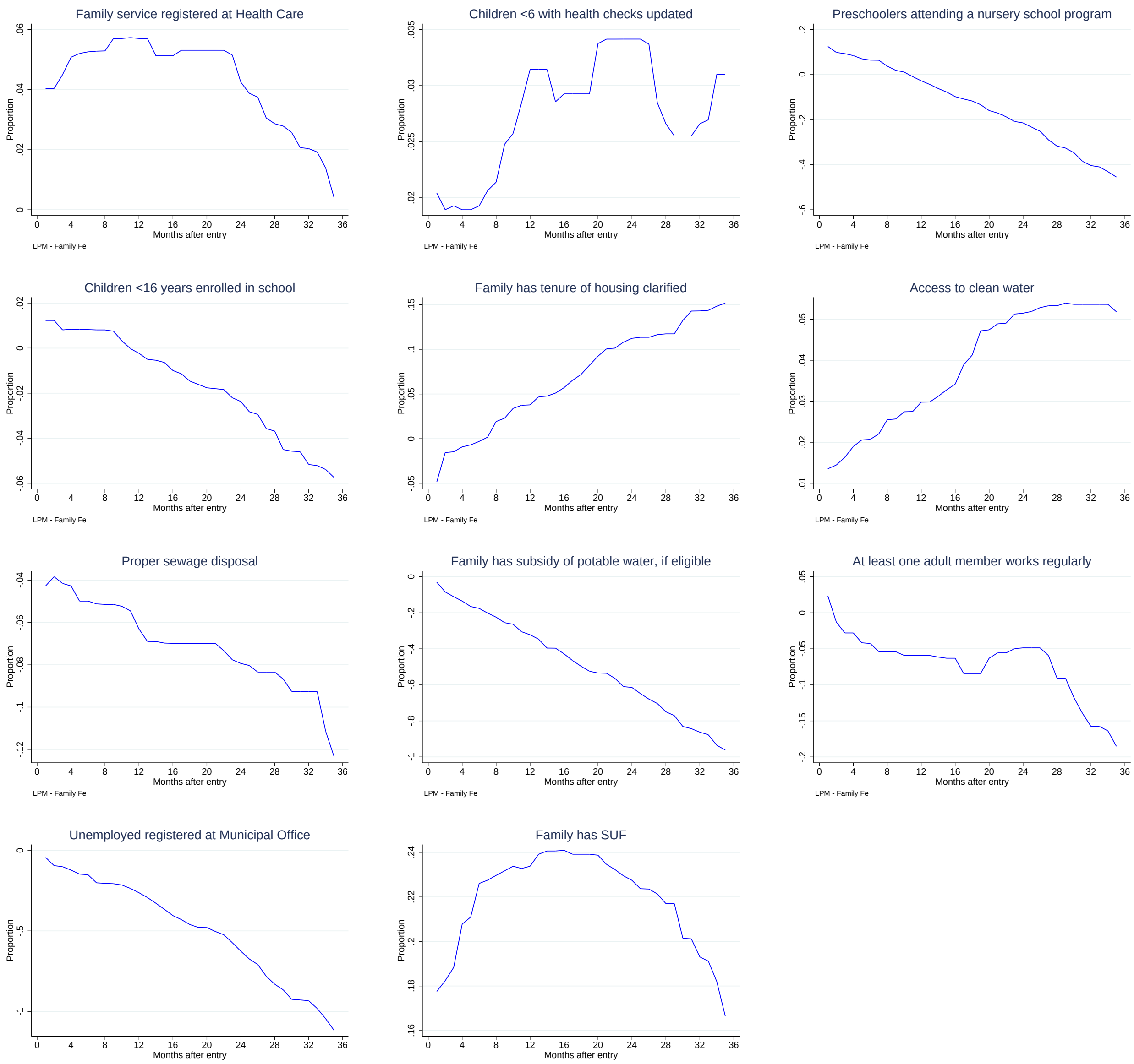


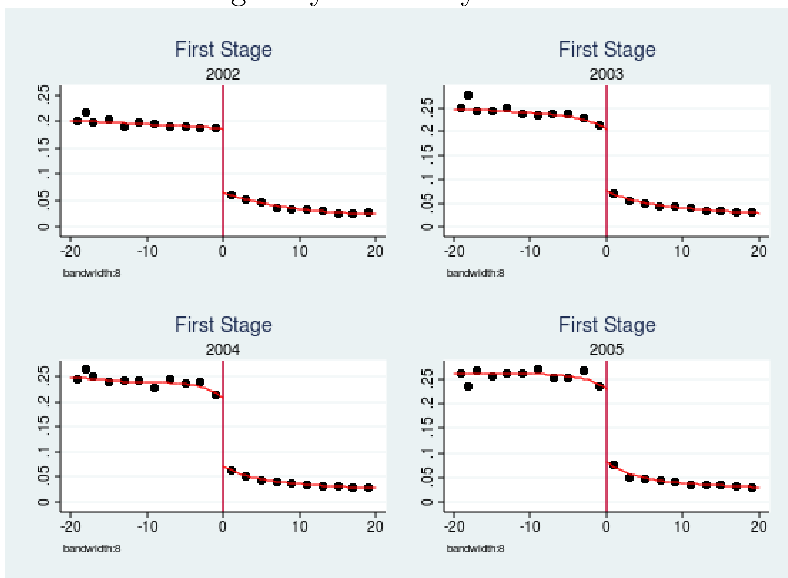
Figure 1: Changes in minimum conditions among participants with exposure to CS.

Note: The graphs present estimate for the parameter θ_t in the following equation

$$Y_{fmkt} = \alpha + \sum_{t=0}^{36} \theta_t T_{fmkt} + f(CAS_{fmk}) + \eta_f + \pi_k + \tau_{month} + \zeta_m + \nu_{fmkt}$$

where Y_{fmkt} is a minimum condition Y_{fmkt} of family f, residing in municipality m when entered CS, k is the year of entry (2002 to 2006) and $month$ is the month when the Puente data we have access to was recorded. T_{fmkt} are 35 indicators for the number of months since entry in Chile Solidario (0 is the month of entry and excluded category), indicators for month of Puente survey (τ_{month}), cohort of entry (τ_{month}) and municipality of residence (ζ_m), also a family fixed effect (η_f). The figures include only those families whose CAS at entry in CS was at most 20-points apart from the cutoff.

Panel A: Eligibility defined by the effective cutoff



Panel B: Eligibility defined by the official cutoff

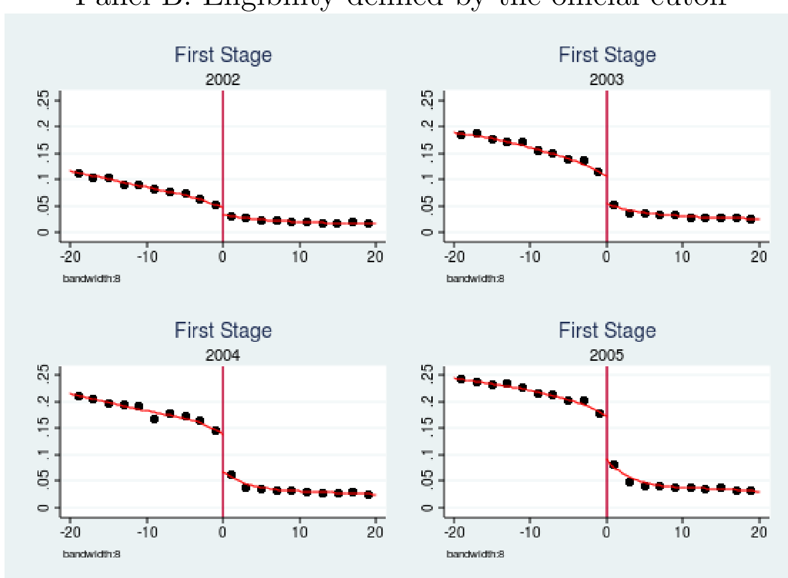


Figure 2: Participation in CS among eligible and non-eligible.

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.

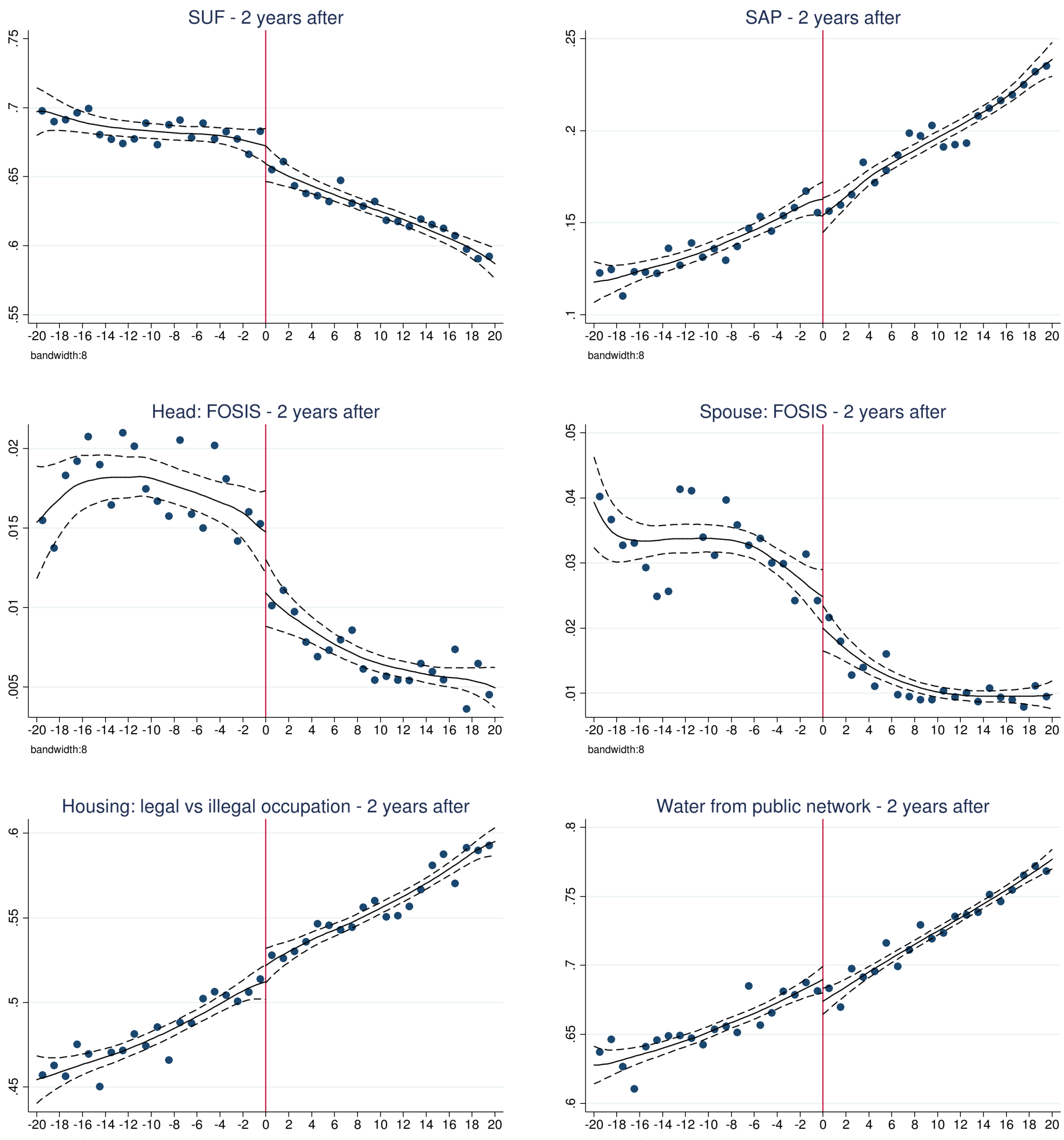


Figure 3: Average outcomes by eligibility status, Bandwidth = 8.
Note: The continuous lines in figure present local linear regression estimates of several outcomes on percentage distance to cutoff. Circles in figures represent the mean outcome by cell within intervals of 2 points of distance to cutoff.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

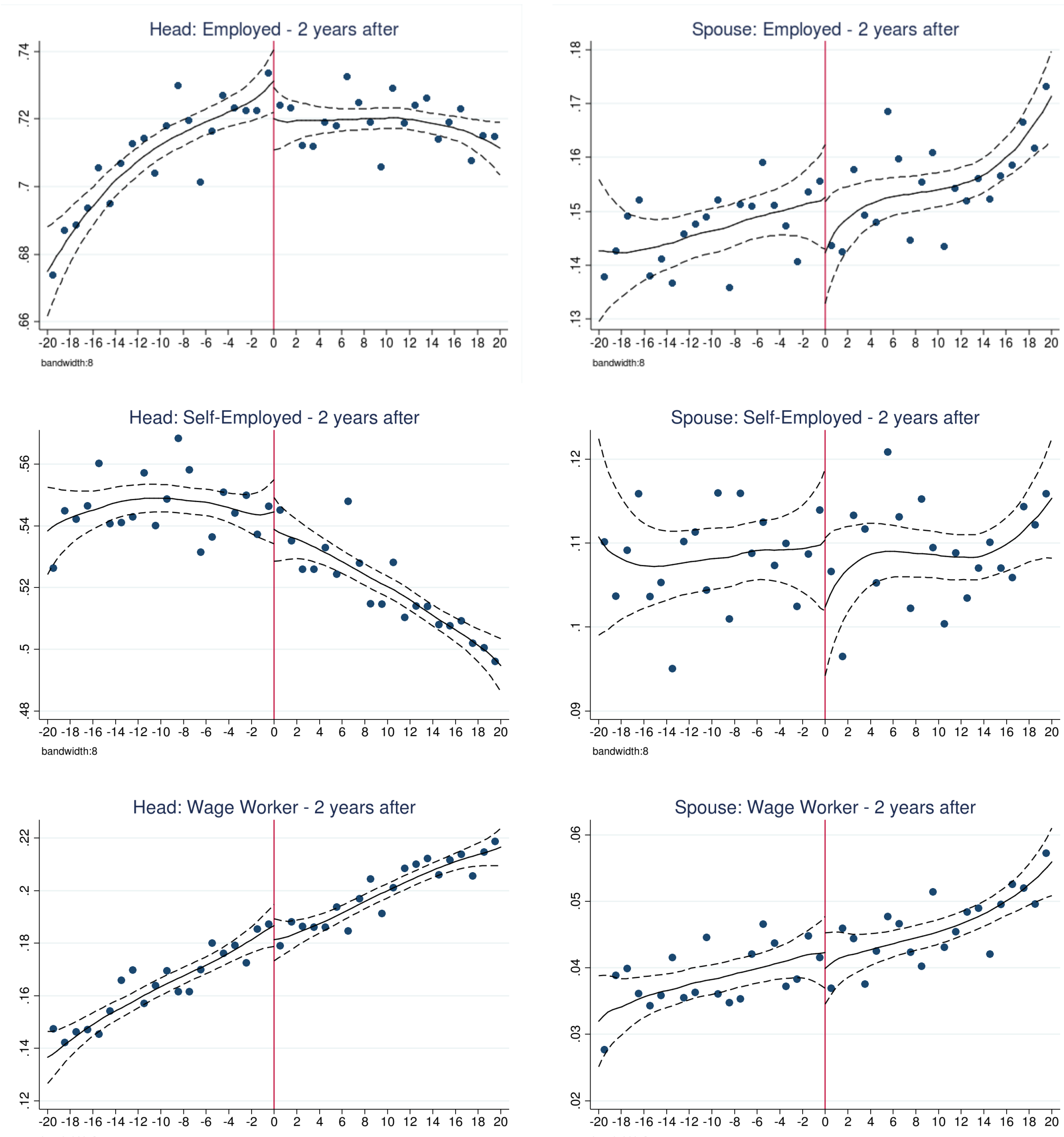


Figure 4: Average outcomes by eligibility status, Bandwidth = 8.
Note: The continuous lines in figure present local linear regression estimates of several outcomes on percentage distance to cutoff. Circles in figures represent the mean outcome by cell within intervals of 2 points of distance to cutoff.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

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	All members of the family with a disability, should have the disability certified by COMPIN (*Comisión Médica, Preventiva e Invalidez*) and registered in the National Disability.	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, should be 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	In the presence of a working mother and in the absence of another adult who can take care of the younger children, these should be incorporated 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 who are able to study should be incorporated into the educational system, regular or 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	That adults have a positive and responsible attitude towards education and school, recognizing the value of the child's participation in formal educational processes.	87%	94%
E9	That adults are literate.	99%	86%
	Family Dynamics		
F1	That exists in the daily practices of family 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%

Note: "N.A." Not Available. The results displayed in Figure B.3 do not match the mean for "Fulfilled at Entry" for the following conditions: H2, H10, E2, E5, E6, F1, F2, F3, F4, F5, F7, F8, C7, C8, C9, C12, L1, G1, G2 and G4. This discrepancy arises because in Figure B.3 we restrict the sample to those families at most 20-points apart from the cutoff.

List of Minimum Conditions to be met by families (cont).

		Applies to % fams	Fulfilled at Entry
	Family Dynamics (cont.)		
F4	That there is an equitable distribution of household tasks (among all family members, regardless of the sex of its members and according to the age of each.)	100%	85%
F5	Family knows about community resources and development programs in the local network (sports clubs, community centers, community organizations).	100%	80%
F6	If there is domestic violence, the people directly involved in this situation are incorporated into a program of support (at least know the alternatives and are in the process of joining).	45%	52%
F7	That families who have children in the protection system somewhere visit them regularly.	35%	39%
F8	That families with a young member in the correctional system, support him/her and be part of the rehabilitation program.	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	They have basic equipment for feeding the family members (pots and pans, crockery and 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 (*Asignación Familiar*) 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		
Empleo (desenlace dependiente) - Employment (dependent work)	FOSIS	Equip individuals so that they can take a job through: (1) training and technical support for strengthening and capacity development, (2) job training courses, (3) services for placement in companies, (4) training and technical support after placement in the workplace.
Bonificación a la contratación - Hiring bonus	SENCE	For hiring new workers in the company, belonging to the CS. It subsidizes up to 50% of a minimum monthly wage per worker, for a period of up to six months. In addition to a contribution for optional training for companies contracting individuals for two or more months and an optional contribution for the costs of transportation.
PROFOCAP - 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, enabling income generation.
Bonificación Jóvenes - Hiring bonus for youth	SENCE	Insert young 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 (optional) to CLP370,000 (785USD) per beneficiary (92 hours minimum). Employees receive bonuses collation and transport of up to CLP55,000 (120USD) per month, for the days actually worked.
Self-Employment		
Empleo (desenlace independiente) - Employment (independent work)	FOSIS	Contribute to develop an independent economic activity. Includes (1) training and technical support for strengthening and capacity development, (2) job training, remedial or upgrade skills; (3) training and technical support for the formulation of business plans, (4) training and technical support for the marketing of goods and services, (5) training and technical support for the management of the economic activity, (6) monitoring the implementation of economic activities. Became PAME in 2006.
Microemprendimiento (PAME) - Micro-enterprise	FOSIS	The program funds projects comprising two lines sequential intervention: (1) support for micro firms, (2) finance microenterprise (fund management capital and support in the process of acquisition of assets, inputs and services).
Microemprendimiento indígena urbano - Micro-enterprise for indigenous in urban areas	CONADI	Provides 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.
Program de Apoyo a la Producción Familiar para el Autoconsumo - Support Program for Subsistence Production	FOSIS	To increase disposable income of rural families at extreme poverty, through savings generated by food production. It comprises 3 services: (1) Access to technology simple and user-friendly to produce, prepare and preserve food efficiently (savings in water, firewood inputs); (2) Training in the use, management and repair of the technologies implemented in conjunction with materials and supplies (family visits and workshops and talks, advice in handling the family budget, analyzing and supporting savings and reallocation of resources to production for home consumption, and support the equitable distribution of tasks in activities such as site preparation for planting, care of the garden, cooking in family); (3) Educational support to improve eating habits with basic information on food preparation and nutrition. These activities are carried out within team monthly visits (each lasting at least 45 min) and conducting workshops and lectures (at least 3 workshops, a minimum of 4 hours each). The intervention lasts for nine months, and its basic steps are: diagnosis (one month), development of action plan (1 month), implementation (3 months), accompaniment (3 months) and closing (1 month).

Labor market programs to which families have access (cont.).

Programs exclusively for CS families		
Employability		
Habilitación sociolaboral - Job Skills	SENCE	Includes job readiness workshops and motivational work for users requiring employability skills. For participants in OMIL.
Competencias Laborales Mujeres - Job skills for women	PRODEMU	Training program for women that includes developing job skills, both technical and occupational training. It is expected to ensure the involvement of women in other employment programs and in PAME. The goal is to provide employment and skills for better placement.
Apoyo al empleo juvenil (PEJ) - Youth Employment Support	FOSIS	To help unemployed youth between 18 and 24 to improve their employability through the application of a specific job placement plan and to participate in public and private employment programs.
Program of preferential access to CS families		
Job Placement		
Empleo Extra (desenlace dependiente) - Employment (dependent work)	FOSIS	See Empleo (desenlace dependiente).
Self-Employment		
Empleo Extra (desenlace independiente) - Employment (independent work)	FOSIS	See Empleo (desenlace independiente).
Apoyo a Actividades Económicas (AAE) - Support for Economic Activities	FOSIS	Includes: (1) financing productive investment, (2) providing specialized services (Consulting and Technical Assistance, etc.), (3) Access to Credit (lines: credit subsidy; Guarantee Fund, and Revolving Fund) - Funding for development projects supporting the environment (lines: Shares Investment Viability; Workspaces Collective, among others).
Programa Fortalecimiento de Iniciativas Microempresariales (FIM) - Program for Strengthening of Microenterprise Initiatives	FOSIS	The goal is to contribute to the consolidation of small units that were supported by the FOSIS at levels 1 and/or 2. That is, initiatives supported by FIM include pre-production units, microenterprises and microentrepreneurs who have graduated from the PAME and AAE from FOSIS, and/or that are in a increasing level of development and require support to be linked to the network development services. Therefore, FIM works with all the production units to show that their revenue is stable, with development potential and work on gaps faced in the commercial and administrative areas and support with legal issues for participation in other programs. In this context, the program aims to contribute to further consolidation of microenterprise.
Employability		
Competencias Laborales (PNCL) - Job Skills Program	FOSIS	Oriented towards leveling studies and social training. To contribute to the beneficiaries and beneficiaries improve their employability, to through certification of their capabilities.
Programa Apoyo a Emprendimientos Sociales (PES) - Social Enterprise Support Program	FOSIS	Pilot in 2007. Offers support to initiates that offer some community service and want to expand. It includes workshops and courses related to the activity or occupation developed, which are certified by technical training centers, universities and other institutions. Also, each participant receives a financial contribution to the purchase of materials or tools that facilitate the delivery of quality care to its clients. The program provides guidance in the process of formalizing the business.
Promocion/Desarollo Social (Social Development)	FOSIS	Generic objectives, intervening in various dimensions of poverty. It works on social problems as 1) Habitability, 2) Social Capital (participation and use of social networks) 3) Family Dynamics (family relationships and / or domestic violence), 4) Education (dropout and / or school performance)

There are five institutions offering several employment programs to poor and indigent families in Chile, these are: FOSIS, SENCE, CONAF, CONADI and PROMEDU. FOSIS 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. SENCE (Servicio Nacional de Capacitacion y Empleo – National Training and Employment Service) which is responsible for supervision and to establish job training programs. CONAF (Corporacion Nacional Forestal – National Forest Corporation), which administers the forest policies. CONADI (Corporacion Nacional de Desarrollo Indígena – National Indigenous Development Corporation) that promotes, coordinates and implements development plans that promote the development of indigenous peoples. Finally, PRODEMU (Fundacion para la Promocion 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%	5.50%
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	Within Municipality		Within Neighborhood	
	2002	R2	Coeff/SE	R2	Coeff/SE
SAP	0.06	0.0474	-0.0034	0.0897	-0.0116**
			(0.0061)		(0.0055)
SUF	0.43	0.0713	0.1206***	0.1197	0.1132***
			(0.0041)		(0.0039)
Housing					
Adequate walls	0.74	0.0504	-0.0660***	0.0919	-0.0629***
			(0.0049)		(0.0035)
Adequate ceil	0.65	0.0523	-0.0785***	0.0933	-0.0732***
			(0.0047)		(0.0033)
Legal occupation of house	0.29	0.0486	-0.0406***	0.0906	-0.0425***
			(0.0040)		(0.0034)
Water from public network	0.74	0.0473	0.0064	0.0892	-0.0063
			(0.0082)		(0.0051)
Sewage connected	0.25	0.0483	0.0055***	0.0904	0.0061***
			(0.0007)		(0.0007)
Fridge	0.20	0.0473	0.0041	0.0892	0.0028
			(0.0046)		(0.0042)
Heating	0.00	0.0474	-0.1021***	0.0894	-0.0987***
			(0.0105)		(0.0100)
Electricity meter	0.36	0.0475	0.0074***	0.0896	0.0111***
			(0.0020)		(0.0018)
CAS and Employment					
CAS	469.75	0.0707	-0.0041***	0.1123	-0.0042***
			(0.0002)		(0.0002)
Head is employed	0.66	0.0505	0.0597***	0.0918	0.0541***
			(0.0036)		(0.0033)
Spouse is employed	0.15	0.0585	0.0290***	0.1127	0.0269***
			(0.0041)		(0.0038)
Imputed income	0.85	0.0530	0.0749***	0.0974	0.0695***
			(0.0032)		(0.0033)
Demographics					
Age of head	44.21	0.0624	-0.0041***	0.1020	-0.0038***
			(0.0001)		(0.0001)
Single headed	0.46	0.0557	-0.0925***	0.0967	-0.0885***
			(0.0041)		(0.0039)
Male head	0.64	0.0488	-0.0418***	0.0908	-0.0422***
			(0.0027)		(0.0025)
Years of schooling	4.78	0.0474	0.0017**	0.0893	0.0014**
			(0.0007)		(0.0007)
Presence of children	0.66	0.0869	0.2094***	0.1251	0.2024***
			(0.0050)		(0.0049)
Family Size	3.57	0.0864	0.0539***	0.1257	0.0530***
			(0.0012)		(0.0011)
Family belongs to dominant ethnicity in neighborhood	0.87	0.0507	-0.0156**	0.1154	-0.0176***
			(0.0063)		(0.0059)
Rural	0.66	0.0474	-0.0188**	0.0892	-0.0006
			(0.0088)		(0.0110)
Minutes family takes to nearest health center	31.46	0.0543	0.0004***	0.1065	0.0003***
			(0.0001)		(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.	2000-2006	2007-2008	G1	$t+2, t+4, t+6$
SAP	1 if receives SAP. Conditions on supply of water from public network before 2002.[31]	2000-2006			$t+2, t+4$
FOSIS	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).	external source 2004-2007.			$t+2, t+4$
Labor market and Income					
Labor market participation	Include employed individuals (see below), individuals 12 or older that ever worked, that search for payed worked in the two month before filling FPS or in the 30 prior to filling FPS had a job that left temporarily.		2007-2009	L1	$t+2, t+4, t+6$
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.	2000-2006		L1	$t+2, t+4, t+6$
	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.		2007-2009		
Self-employed	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.	2000-2006			$t+2, t+4, t+6$
	Family workers that do not receive wage, employer, independent worker, domestic workers.		2007-2009		
Dependent	Only defined for employed individuals. Include urban and rural dependent workers and workers in the public or private sector.	2000-2006			$t+2, t+4, t+6$
	Individuals employed in the private and public sector or armed forces.		2007-2009		
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.	2000-2006		C1	$t+2, t+4, t+6$
	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).		2007-2009		
Owner of house (if legal occupation)	Owns the place (fully paid and with or without overdue debts).	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.	2000-2006		C3	$t+2, t+4, t+6$
	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).		2007-2009		

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 if includes titles 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: 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	-0.044
	(0.032)	(0.080)
Control Mean	0.094	
	Panel B: Receiving SAP before 2002	
Eligibility (ITT)	-0.064***	0.033
	(0.022)	(0.070)
Participation (IV)	-0.301**	0.261
	(0.113)	(0.803)
Control Mean	0.639	
P-Value: HA: $\beta_0^{iv} > \beta_1^{iv}$	.364	.598

Note: See table 3 for a description of specification used in rows named (ITT) and see table 5 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, H0: $\beta_0^{iv} = \beta_1^{iv}$, against the alternative that the effect is larger for those that did not receive SAP 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; see Romano and Wolf, 2005).

Table A.7: 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	0.006	0.009
	(0.002)	(0.003)	(0.003)	(0.005)
Participation (IV)	0.026**	0.010	0.033**	0.040
	(0.011)	(0.012)	(0.017)	(0.021)
Observations	141,926	50,879	94,952	32,273
Control Mean	0.010	0.008	0.019	0.010
P-Value: HA: $\beta^{iv}_{0506} > \beta^{iv}_{0204}$	0.497		0.520	
	Panel A: Not employed before 2002			
Eligibility (ITT)	0.012**	0.007	0.008**	0.012**
	(0.004)	(0.007)	(0.003)	(0.005)
Participation (IV)	0.073***	0.035	0.043**	0.054**
	(0.022)	(0.036)	(0.017)	(0.023)
Observations	32,153	11,177	84,570	28,729
Control Mean	0.008	0.009	0.017	0.008
P-Value: HA: $\beta^{iv}_{0506} > \beta^{iv}_{0204}$	0.500		0.528	
	Panel B: Employed before 2002			
Eligibility (ITT)	0.002	0.001	-0.012	-0.019
	(0.002)	(0.003)	(0.011)	(0.013)
Participation (IV)	0.015	0.006	-0.077	-0.095
	(0.013)	(0.012)	(0.072)	(0.069)
Observations	109,730	39,688	10,382	3,544
Control Mean	0.011	0.007	0.028	0.028
P-Value: HA: $\beta^{iv}_{0506} > \beta^{iv}_{0204}$	0.564		0.516	
P-Value: HA: $\beta^{iv}_{0} > \beta^{iv}_{1}$	0.512	0.508	0.468	0.480

Note: See table 3 for a description of specification used in rows (ITT) and see table 5 for the description of specification used in rows (IV). "C. Mean" in the mean of the outcome for those at most 4-CAS points above the cutoff. Data on participation in labor market programs is available from 2004 to 2007, however for this last years the records are incomplete. 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, H0: $\beta^{iv}_{0506} = \beta^{iv}_{0204}$, against the alternative that the effect is larger for those entering in the later year, HA: $\beta^{iv}_{0506} > \beta^{iv}_{0204}$. 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, H0: $\beta^{iv}_{0} = \beta^{iv}_{1}$, against the alternative that the effect is larger for those not employed prior to 2002, HA: $\beta^{iv}_{0} > \beta^{iv}_{1}$.

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 adjusted for multiple hypotheses testing; see Romano and Wolf, 2005).

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

	(1)	(2)	(3)	(4)
	Head		Spouse	
Years after start	4		4	
Cohort	2002-2004	2005-2006	2002-2004	2005-2006
	Panel A: All sample			
Eligibility (ITT)	-0.002	0.013	-0.002	0.023
	(0.009)	(0.013)	(0.009)	(0.013)
Participation (IV)	-0.009	0.060	-0.011	0.105
	(0.054)	(0.064)	(0.052)	(0.059)
Control Mean	0.697	0.676	0.184	0.170
P-Value: HA: $\beta^{iv}_{0506} > \beta^{iv}_{0204}$	0.236		0.072	
	Panel B: Not employed before 2002			
Eligibility (ITT)	-0.014	0.007	-0.002	0.023
	(0.017)	(0.029)	(0.009)	(0.013)
Participation (IV)	-0.098	0.034	-0.009	0.104
	(0.121)	(0.135)	(0.049)	(0.059)
Control Mean	0.253	0.255	0.155	0.142
P-Value: HA: $\beta^{iv}_{0506} > \beta^{iv}_{0204}$	0.232		0.080	
	Panel C: Employed before 2002			
Eligibility (ITT)	-0.014	0.008	-0.004	0.003
	(0.009)	(0.014)	(0.035)	(0.054)
Participation (IV)	-0.082	0.040	-0.030	0.015
	(0.053)	(0.068)	(0.255)	(0.298)
Control Mean	0.817	0.791	0.428	0.399
P-Value: HA: $\beta^{iv}_{0506} > \beta^{iv}_{0204}$	0.104		0.476	
P-Value: HA: $\beta^{iv}_{0} > \beta^{iv}_{1}$	0.572	0.532	0.452	0.356

Note: The variable labor market participation is only available from the FPS for the years of 2007-2009, therefore the sample used in columns (1) and (3) includes the years of 2007 and 2008, whereas the sample used in columns (2) and (4) include only 2009. Labor market participation that takes value 1 if the individual is employed, individuals 12 or older that ever worked, individuals that search for payed worked in the two month before filling FPS or individuals that in the 30 prior to filling FPS had a job that left temporarily.
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, H0: $\beta^{iv}_{0506} = \beta^{iv}_{0204}$, against the alternative that the effect is larger for those entering in the later year, HA: $\beta^{iv}_{0506} > \beta^{iv}_{0204}$. 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, H0: $\beta^{iv}_{0} = \beta^{iv}_{1}$, against the alternative that the effect is larger for those not employed prior to 2002, HA: $\beta^{iv}_{0} > \beta^{iv}_{1}$.
See table 3 for a description of specification used in rows (ITT) and see table 5 for the description of specification used in rows (2SLS). "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.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	0.007	-0.001	-0.003	0.004
	(0.005)	(0.008)	(0.003)	(0.003)	(0.009)
Mean	0.687	0.147	0.962	0.976	0.298
A.2: Water fetched to the house	-0.008*	-0.001	-0.021	-0.001	-0.010
	(0.004)	(0.003)	(0.014)	(0.002)	(0.009)
Mean	0.192	0.0793	0.645	0.0167	0.428
Observations	202,627	164,693	37,934	118,965	83,662
	Panel B: Tenency				
B.1: House owner	-0.011	-0.009	-0.011	-0.008	-0.017
	(0.006)	(0.008)	(0.008)	(0.008)	(0.010)
Mean	0.529	0.270	0.828	0.532	0.526
Observations	202,627	106,920	95,707	118,965	83,662
	Panel C: Sewage Connection				
C.1: Sewage connected to network	0.005	0.002	0.003	0.006	-0.006
	(0.005)	(0.005)	(0.009)	(0.008)	(0.004)
Mean	0.370	0.241	0.881	0.604	0.0404
Observations	214,136	168,582	45,554	128,536	85,600
	Panel D: Quality of the walls				
D.1: Adequate walls	0.002	0.000	-0.004	0.012	-0.011
	(0.006)	(0.007)	(0.014)	(0.008)	(0.010)
Mean	0.279	0.180	0.560	0.277	0.283
D.2: Adequate walls or mixed	0.001	0.005	0.000	-0.001	0.001
	(0.005)	(0.018)	(0.004)	(0.006)	(0.007)
Mean	0.893	0.624	0.947	0.916	0.862
Observations	141,943	23,953	117,990	82,637	59,306
	Panel E: Quality of the ceiling				
E.1: Adequate ceiling	0.005	0.007	-0.005	0.015	-0.009
	(0.007)	(0.008)	(0.011)	(0.010)	(0.010)
Mean	0.403	0.253	0.668	0.451	0.341
E.2: Adequate ceiling or mixed	0.006	0.025	-0.004	0.009	0.003
	(0.006)	(0.015)	(0.005)	(0.007)	(0.009)
Mean	0.828	0.540	0.922	0.848	0.802
Observations	141,943	34,438	107,505	82,637	59,306

Impact of CS: ITT estimates for quality of housing (cont).

	(1)	(2)	(3)	(4)	(5)
	Basic	Before 2002		Area	
		No	Yes	Urban	Rural
		Panel F: Electricity			
F.1: Yes	0.006	-0.000	0.006	0.013	-0.002
	(0.007)	(0.010)	(0.009)	(0.010)	(0.011)
Mean	141,257	71,351	69,906	82,521	58,736
Observations	0.570	0.342	0.806	0.554	0.591
		Panel G: House has at least 2 habitable rooms			
G.1: House owner	0.006	0.004	0.003	0.014	-0.004
	(0.004)	(0.026)	(0.003)	(0.007)	(0.003)
Mean					
	141,942	12,503	129,439	82,636	59,306
Observations	0.943	0.681	0.968	0.915	0.979

Note: The table presents the coefficient estimates (and standard errors) on eligibility from model 3. 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" 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.10: Impact of CS: ITT and 2SLS estimates for the whole sample (behaviors).

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

Note: See table 3 for a description of specification used in columns (3) and (6) and see table 5 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: Sensitivity to the choice of functional form.

	(1)	(2)	(3)	(4)
Years after start	Basic	Distance		CAS
		Cubic	Quartic	Quadratic
Participation				
SUF	0.023***	0.013	0.011	0.022***
	(0.007)	(0.010)	(0.012)	(0.005)
SAP	0.002	0.008	0.007	0.010
	(0.006)	(0.008)	(0.009)	(0.005)
Labor market programs - FOSIS (head)	0.004	0.004	0.002	0.007***
	(0.002)	(0.002)	(0.003)	(0.001)
Labor market programs - FOSIS (spouse)	0.006**	0.001	0.004	0.014***
	(0.003)	(0.003)	(0.004)	(0.002)
Labor market				
Labor market participation (head)	0.010	0.024	0.024	0.005
	(0.011)	(0.014)	(0.017)	(0.008)
Employed (head)	0.009	0.011	0.012	0.005
	(0.006)	(0.008)	(0.010)	(0.005)
Self-employed (head)	0.005	0.006	0.006	0.001
	(0.006)	(0.008)	(0.010)	(0.005)
Dependent worker (head)	0.005	0.005	0.006	0.004
	(0.005)	(0.007)	(0.009)	(0.004)
Labor market participation (spouse)	0.011	0.019	0.019	0.012
	(0.011)	(0.014)	(0.019)	(0.008)
Employed (spouse)	0.003	0.013	0.017	0.006
	(0.006)	(0.007)	(0.010)	(0.004)
Self-employed (spouse)	0.003	0.009	0.015	0.003
	(0.005)	(0.006)	(0.008)	(0.004)
Dependent worker (spouse)	0.000	0.003	0.002	0.003
	(0.004)	(0.005)	(0.005)	(0.003)
Housing				
Legal occupation of house	-0.011	-0.013	-0.014	-0.005
	(0.006)	(0.008)	(0.011)	(0.004)
Sewage connected	0.005	0.011	0.011	0.007
	(0.005)	(0.007)	(0.009)	(0.005)
Water from public network	0.006	0.005	0.007	0.011***
	(0.005)	(0.006)	(0.008)	(0.003)
Adequate roof	0.005	0.008	0.002	-0.001
	(0.007)	(0.010)	(0.013)	(0.005)
Adequate walls	0.002	0.009	0.003	-0.003
	(0.006)	(0.009)	(0.011)	(0.005)
Heating	0.000	0.003	0.005	0.000
	(0.004)	(0.006)	(0.006)	(0.003)
Fridge	-0.002	-0.002	-0.007	0.021***
	(0.010)	(0.010)	(0.013)	(0.005)

Note: The table presents the coefficient estimates (and standard errors) on eligibility from model 3 for columns (1)-(3). Controls excluded from column (1) include quadratic in distance to cutoff, their interaction with eligibility to CS and municipality-year effects; controls excluded from columns (2) and (3) include cubic and quartic in distance to cutoff, respectively, their interaction with eligibility to CS and municipality-year effects. The municipality is the municipality of residence when eligibility is evaluated. Column (4) presents the marginal effect on eligibility from estimating the following model

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

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.12: Impact of CS: Sensitivity to trimming around cutoff.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Distance in CAS-points	10		20		30		50		100	
	N	ITT	N	ITT	N	ITT	N	ITT	N	ITT
Participation										
SUF	59,075	0.009	116,163	0.023***	136,801	0.017**	157,062	0.031***	167,796	0.044***
		(0.011)		(0.007)		(0.006)		(0.006)		(0.005)
SAP	65,924	0.007	129,967	0.002	150,446	0.006	168,689	0.007	175,849	0.002
		(0.008)		(0.006)		(0.005)		(0.005)		(0.005)
Labor market progr. - FOSIS	97,352	0.003	192,810	0.004	226,595	0.005***	259,597	0.007***	276,980	0.008***
(head)		(0.002)		(0.002)		(0.001)		(0.001)		(0.001)
Labor market progr. - FOSIS	64,416	0.003		0.006**	149,874	0.011***	171,802	0.012***	183,160	0.014***
(spouse)		(0.004)	127,225	(0.003)		(0.002)		(0.002)		(0.002)
Labor market										
Head										
Labor market partic.	30,374	0.032	60,739	0.010	74,266	0.008	90,099	0.007	101,602	0.012
		(0.015)		(0.011)		(0.009)		(0.008)		(0.007)
Employed	100,218	0.018	198,464	0.009	233,625	0.007	268,752	0.009	288,207	0.014**
		(0.009)		(0.006)		(0.005)		(0.005)		(0.004)
Self-employed	100,218	0.012	198,464	0.005	233,625	0.002	268,752	0.008	288,207	0.014**
		(0.009)		(0.006)		(0.005)		(0.005)		(0.004)
Dependent worker	100,218	0.006	198,464	0.005	233,625	0.005	268,752	0.001	288,207	-0.001
		(0.007)		(0.005)		(0.004)		(0.004)		(0.003)
Spouse										
Labor market partic.	18,895	0.016	37,888	0.011	46,542	0.013	56,648	0.003	64,000	0.007
		(0.016)		(0.011)		(0.009)		(0.008)		(0.007)
Employed	59,207	0.010	116,872	0.003	137,170	0.006	156,919	0.004	167,317	0.003
		(0.008)		(0.006)		(0.005)		(0.004)		(0.004)
Self-employed	59,207	0.011	116,872	0.003	137,170	0.003	156,919	0.002	167,317	0.003
		(0.007)		(0.005)		(0.004)		(0.004)		(0.003)
Dependent worker	59,207	-0.001	116,872	0.000	137,170	0.003	156,919	0.002	167,317	0.000
		(0.005)		(0.004)		(0.003)		(0.003)		(0.002)
Housing										
Legal occupation	102,304	-0.015	202,627	-0.011	238,664	-0.003	274,766	-0.006	294,745	-0.018**
of house		(0.009)		(0.006)		(0.006)		(0.005)		(0.005)
Sewage connected	108,928	0.010	214,136	0.005	246,400	0.008	274,958	0.004	286,634	-0.003
		(0.008)		(0.005)		(0.005)		(0.005)		(0.005)
Water from public	102,304	0.007	202,627	0.006	238,664	0.008	274,766	0.008	294,745	0.001
network		(0.006)		(0.005)		(0.004)		(0.004)		(0.003)
Adequate roof	71,958	-0.003	141,943	0.005	164,494	0.004	184,768	-0.005	193,255	-0.015**
		(0.011)		(0.007)		(0.006)		(0.006)		(0.005)
Adequate walls	71,958	0.006	141,943	0.002	164,494	-0.001	184,768	-0.008	193,255	-0.013**
		(0.009)		(0.006)		(0.006)		(0.005)		(0.004)
Heating	71,958	0.004	141,943	0.000	164,494	0.004	184,768	-0.003	193,255	-0.013**
		(0.006)		(0.004)		(0.003)		(0.003)		(0.003)
Fridge	71,958	-0.001	141,943	-0.002	164,494	0.020***	184,768	0.020***	193,255	0.011
		(0.011)		(0.010)		(0.006)		(0.006)		(0.005)

Note: The table presents the coefficient estimates (and standard errors) on eligibility from model 3 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. 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 using the procedure in Algorithms 4.1 and 4.2 of Romano and Wolf, 2005).

Table A.13: Impact of CS: Choice of Fixed Effects.

	(1)	(2)	(3)	(4)
Fixed effects	Basic	Municip Year	Neighb Year	Neighb-year
Participation				
SUF	0.023***	0.023**	0.026***	0.029***
	(0.007)	(0.007)	(0.008)	(0.008)
SAP	0.002	0.001	-0.001	0.001
	(0.006)	(0.006)	(0.006)	(0.006)
Labor market progr. - FOSIS (head)	0.004	0.004	0.003	0.003
	(0.002)	(0.002)	(0.002)	(0.002)
Labor market progr. - FOSIS (spouse)	0.006*	0.006**	0.005	0.006
	(0.003)	(0.002)	(0.003)	(0.003)
Labor market				
Labor market participation (head)	0.010	0.010	0.015	0.011
	(0.011)	(0.011)	(0.011)	(0.012)
Employed (head)	0.009	0.009	0.011	0.012
	(0.006)	(0.006)	(0.006)	(0.006)
Self-employed (head)	0.005	0.004	0.006	0.008
	(0.006)	(0.006)	(0.006)	(0.006)
Dependent worker (head)	0.005	0.005	0.005	0.003
	(0.005)	(0.005)	(0.005)	(0.005)
Labor market participation (spouse)	0.011	0.009	0.011	0.011
	(0.011)	(0.011)	(0.012)	(0.013)
Employed (spouse)	0.003	0.001	0.000	0.002
	(0.006)	(0.006)	(0.006)	(0.007)
Self-employed (spouse)	0.003	0.002	0.002	0.002
	(0.005)	(0.005)	(0.005)	(0.005)
Dependent worker (spouse)	0.000	-0.001	-0.002	-0.000
	(0.004)	(0.004)	(0.004)	(0.004)
Housing				
Legal occupation of house	-0.011	-0.011	-0.014	-0.012
	(0.006)	(0.006)	(0.007)	(0.007)
Sewage connected	0.005	0.005	0.004	0.002
	(0.005)	(0.005)	(0.005)	(0.005)
Water from public network	0.006	0.005	0.000	0.001
	(0.005)	(0.005)	(0.004)	(0.004)
Adequate roof	0.005	0.004	0.009	0.008
	(0.007)	(0.007)	(0.007)	(0.008)
Adequate walls	0.002	0.001	0.005	0.005
	(0.006)	(0.006)	(0.007)	(0.007)
Heating	0.000	-0.000	-0.001	-0.000
	(0.004)	(0.004)	(0.004)	(0.004)
Fridge	-0.002	0.009	0.007	0.005
	(0.010)	(0.007)	(0.008)	(0.008)

Note: The table presents the coefficient estimates (and standard errors) on eligibility from model 3 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 using the procedure in Algorithms 4.1 and 4.2 of Romano and Wolf, 2005).

Table A.14: Impact of CS not conditioning on presence in data prior to 2002.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years after start	2				4			
	Basic		Not conditioning		Basic		Not conditioning	
	N	ITT	N	ITT	N	ITT	N	ITT
Participation								
SUF	116,163	0.023***	208,365	0.023***	66,171	0.017	115,940	0.026***
		(0.007)		(0.006)		(0.011)		(0.008)
SAP	129,967	0.002	193,171	0.005	19,475	0.013	25,858	0.013
		(0.006)		(0.005)		(0.014)		(0.011)
Labor market progr. - FOSIS (head)	192,810	0.004	304,725	0.005***	94,708	-0.001	137,998	0.002
		(0.002)		(0.001)		(0.001)		(0.001)
Labor market progr. - FOSIS (spouse)	127,225	0.006**	195,273	0.006***	62,408	0.004	90,802	0.002
		(0.003)		(0.002)		(0.002)		(0.002)
Labor market								
Labor market participation (head)	60,739	0.010	113,328	0.020**	142,698	0.003	244,476	0.012
		(0.011)		(0.008)		(0.007)		(0.006)
Employed (head)	198,464	0.009	319,486	0.013**	165,689	0.001	270,887	0.010
		(0.006)		(0.005)		(0.007)		(0.005)
Self-employed (head)	198,464	0.005	319,486	0.003	165,689	0.001	270,887	0.002
		(0.006)		(0.005)		(0.007)		(0.006)
Dependent worker (head)	198,464	0.005	319,486	0.010*	165,689	0.000	270,887	0.008
		(0.005)		(0.004)		(0.007)		(0.005)
Labor market participation (spouse)	37,888	0.011	66,654	0.010	89,511	0.005	147,283	0.003
		(0.011)		(0.008)		(0.008)		(0.006)
Employed (spouse)	116,872	0.003	180,857	0.006	73,751	0.010	118,298	0.005
		(0.006)		(0.005)		(0.010)		(0.008)
Self-employed (spouse)	116,872	0.003	180,857	0.004	73,751	0.006	118,298	0.005
		(0.005)		(0.004)		(0.008)		(0.006)
Dependent worker (spouse)	116,872	0.000	180,857	0.002	73,751	0.004	118,298	-0.000
		(0.004)		(0.004)		(0.006)		(0.006)
Housing								
Legal occupation of house	202,627	-0.011	327,288	-0.004	174,269	-0.010	285,700	-0.009
		(0.006)		(0.005)		(0.007)		(0.006)
Sewage connected	214,136	0.005	214,136	0.005	41,893	0.012	41,893	0.012
		(0.005)		(0.005)		(0.011)		(0.011)
Water from public network	202,627	0.006	327,288	0.007	174,270	0.008	285,700	0.008
		(0.005)		(0.004)		(0.005)		(0.004)
Adequate roof	141,943	0.005	214,136	0.006	31,850	-0.017	41,893	0.003
		(0.007)		(0.006)		(0.016)		(0.013)
Adequate walls	141,943	0.002	214,136	0.002	31,850	-0.016	41,893	-0.014
		(0.006)		(0.006)		(0.016)		(0.013)
Heating	141,943	0.000	214,136	0.001	31,850	-0.015	41,893	-0.008
		(0.004)		(0.003)		(0.009)		(0.007)
Fridge	141,943	-0.002	214,136	0.009	31,850	0.018	41,893	0.018
		(0.010)		(0.006)		(0.015)		(0.013)

Note: The table presents the coefficient estimates (and standard errors) on eligibility from model 3. Controls excluded 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. 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).

B Figures

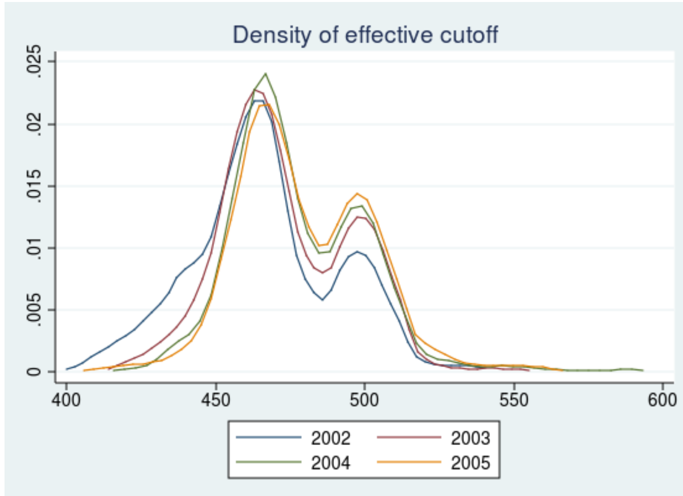


Figure B.1: Distribution of effective cutoffs

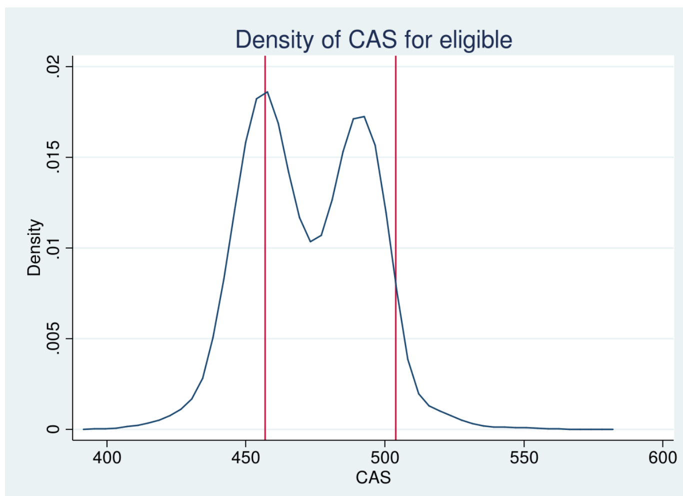


Figure B.2: Distribution of CAS

Note: This figure plots the density of CAS-scores among the sample of eligible families used in the regressions for families exposed to 2 years of CS, that is, those with CAS at most 20-points apart from the cutoff. The vertical lines are the 10th and 90th percentiles of the *effective* cutoffs.

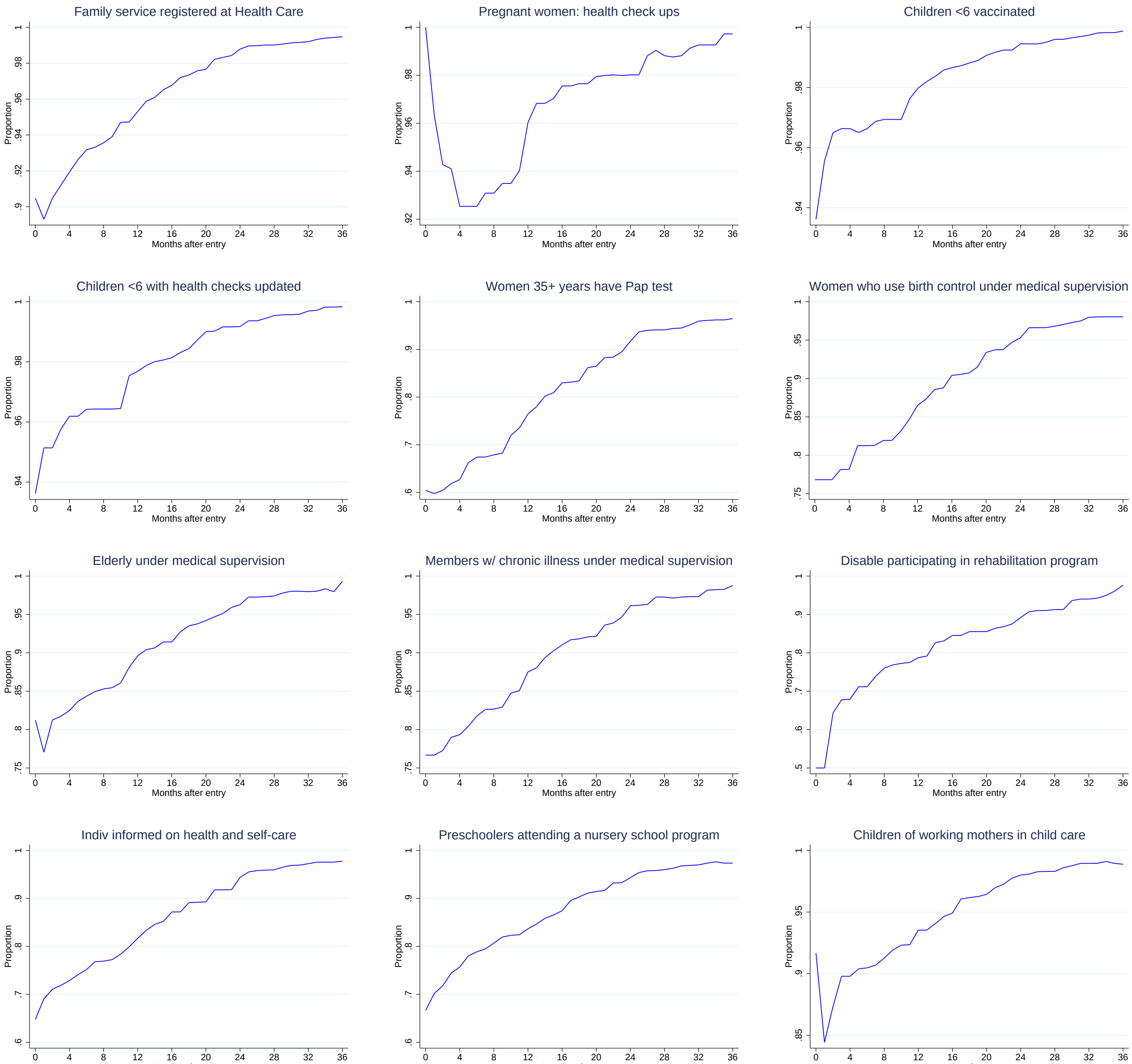
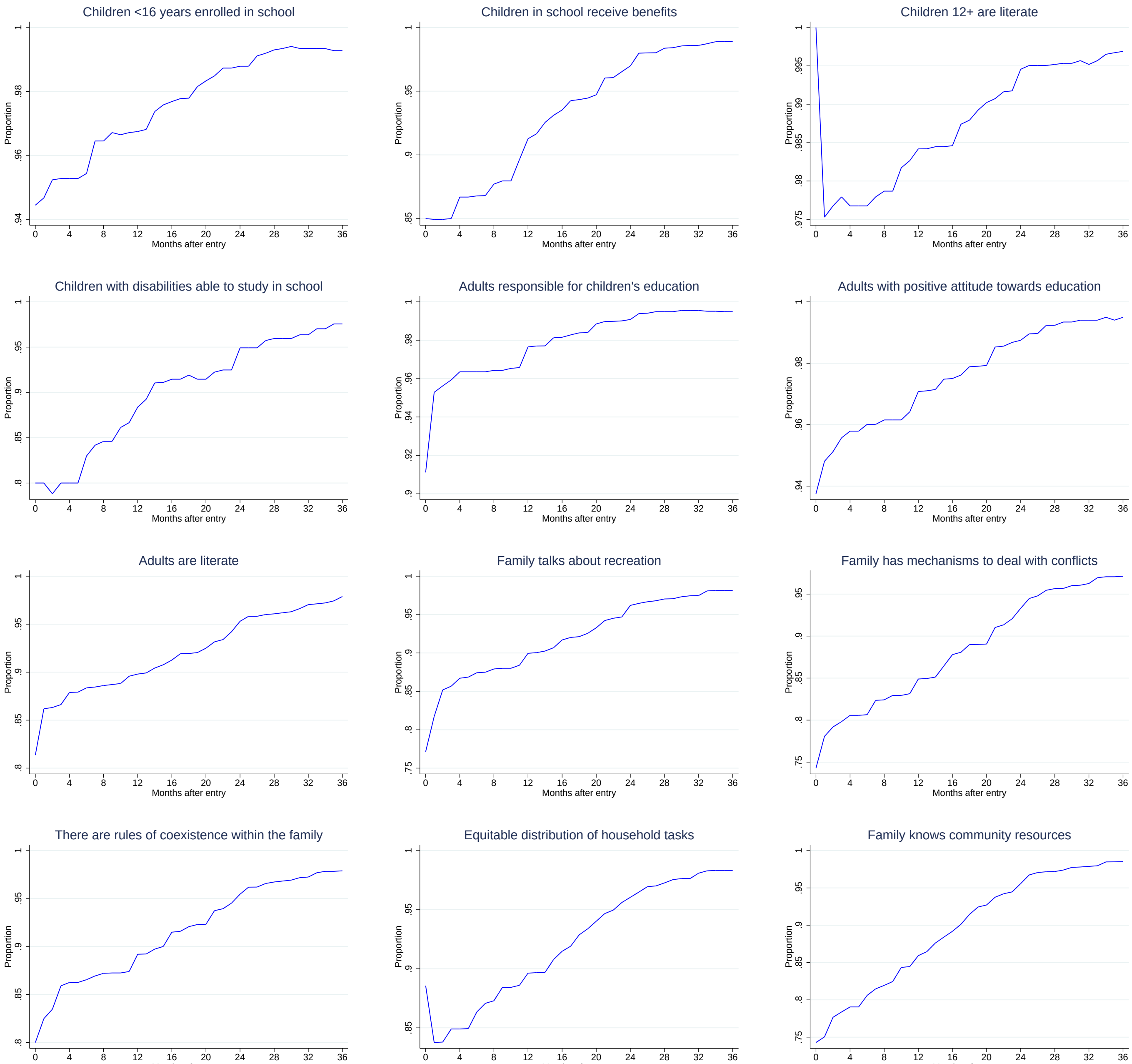
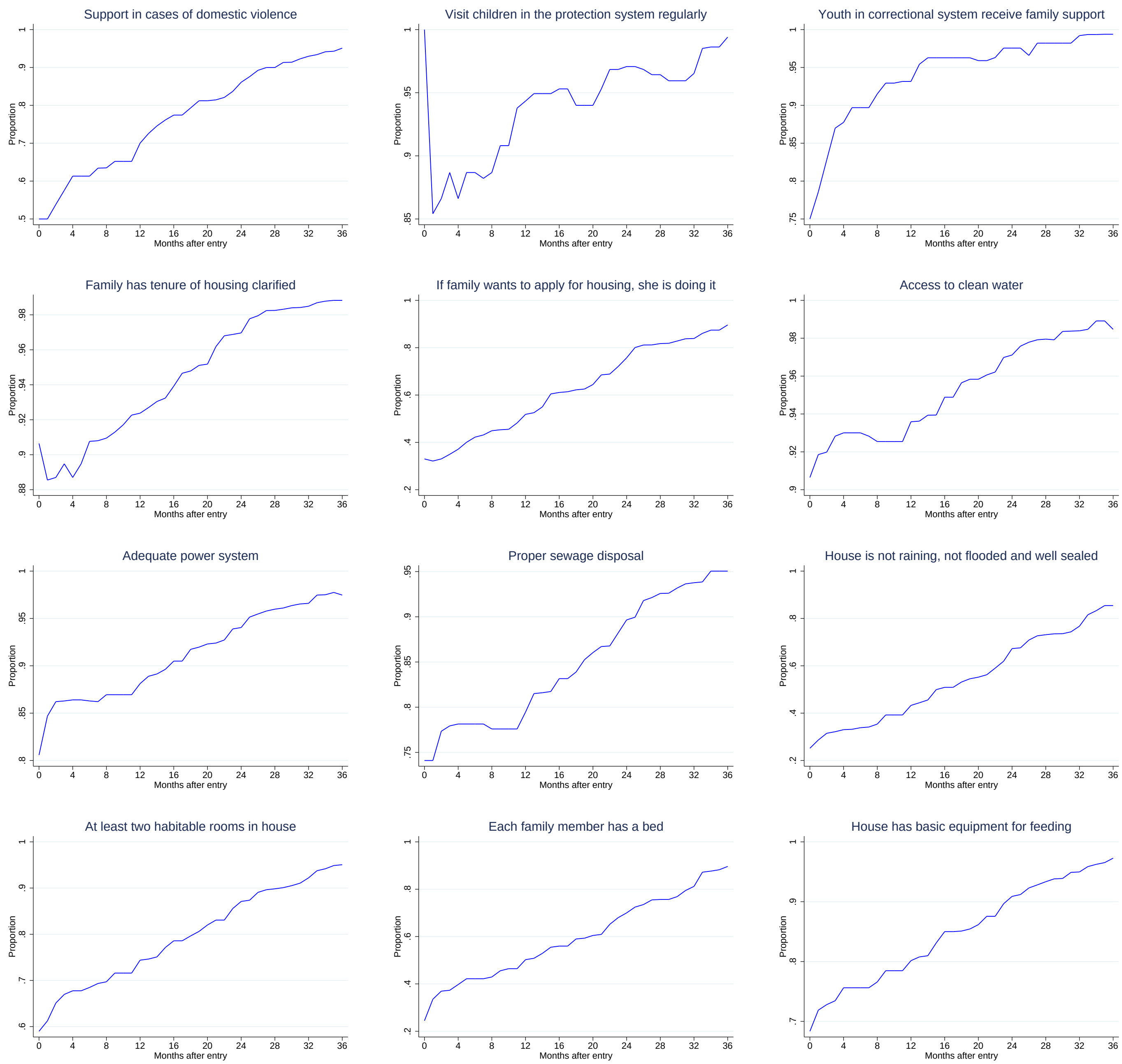


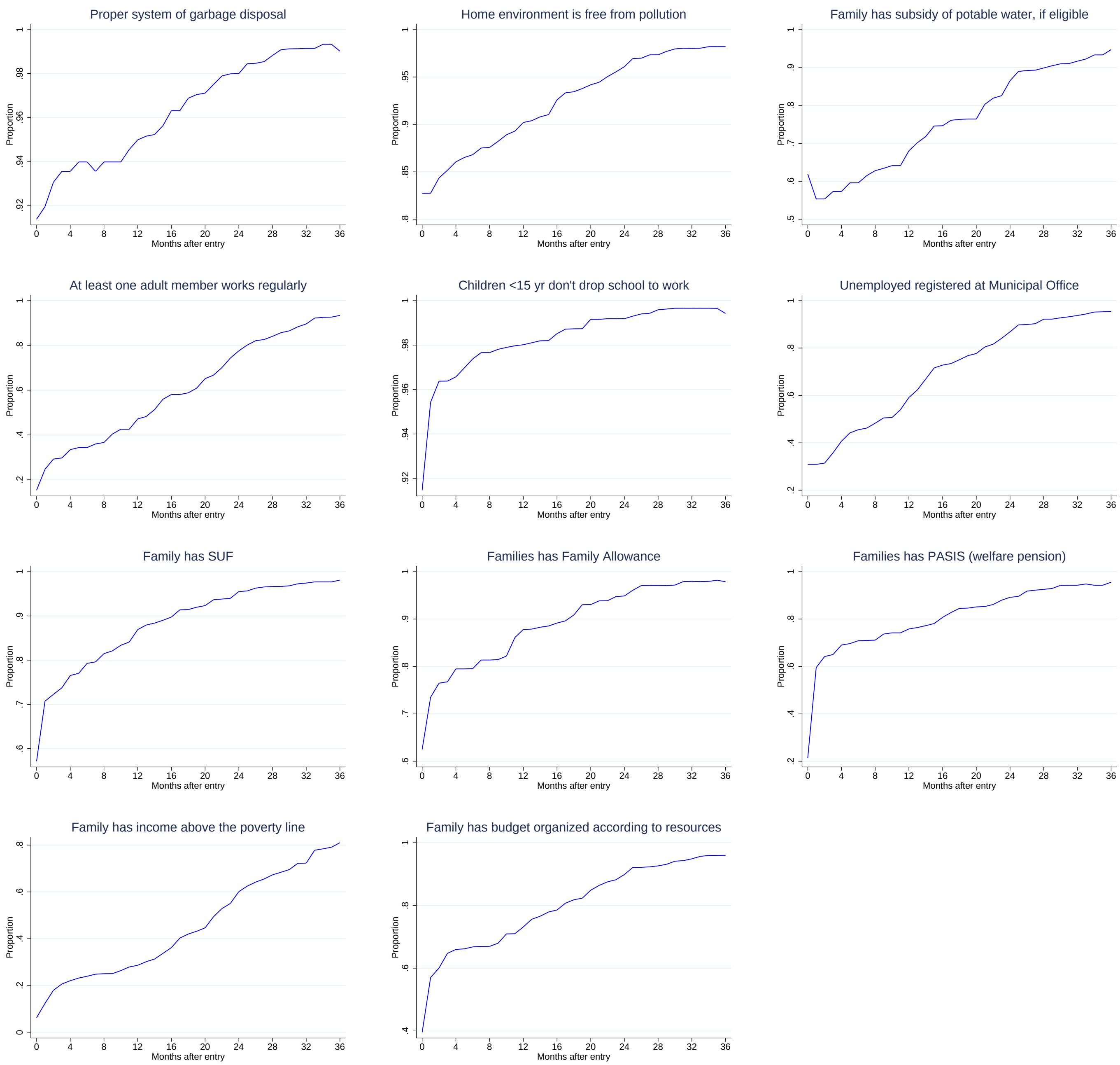
Figure B.3: Average of Minimum Conditions Fulfilled.



Average of Minimum Conditions Fulfilled (cont.).



Average of Minimum Conditions Fulfilled (cont.).



Average of Minimum Conditions Fulfilled (cont.).

Note: Figures display the average for each variable taken selected months after entry in Chile Solidario. The figures include only those families whose CAS at entry in CS was at most 20-points apart from the cutoff.

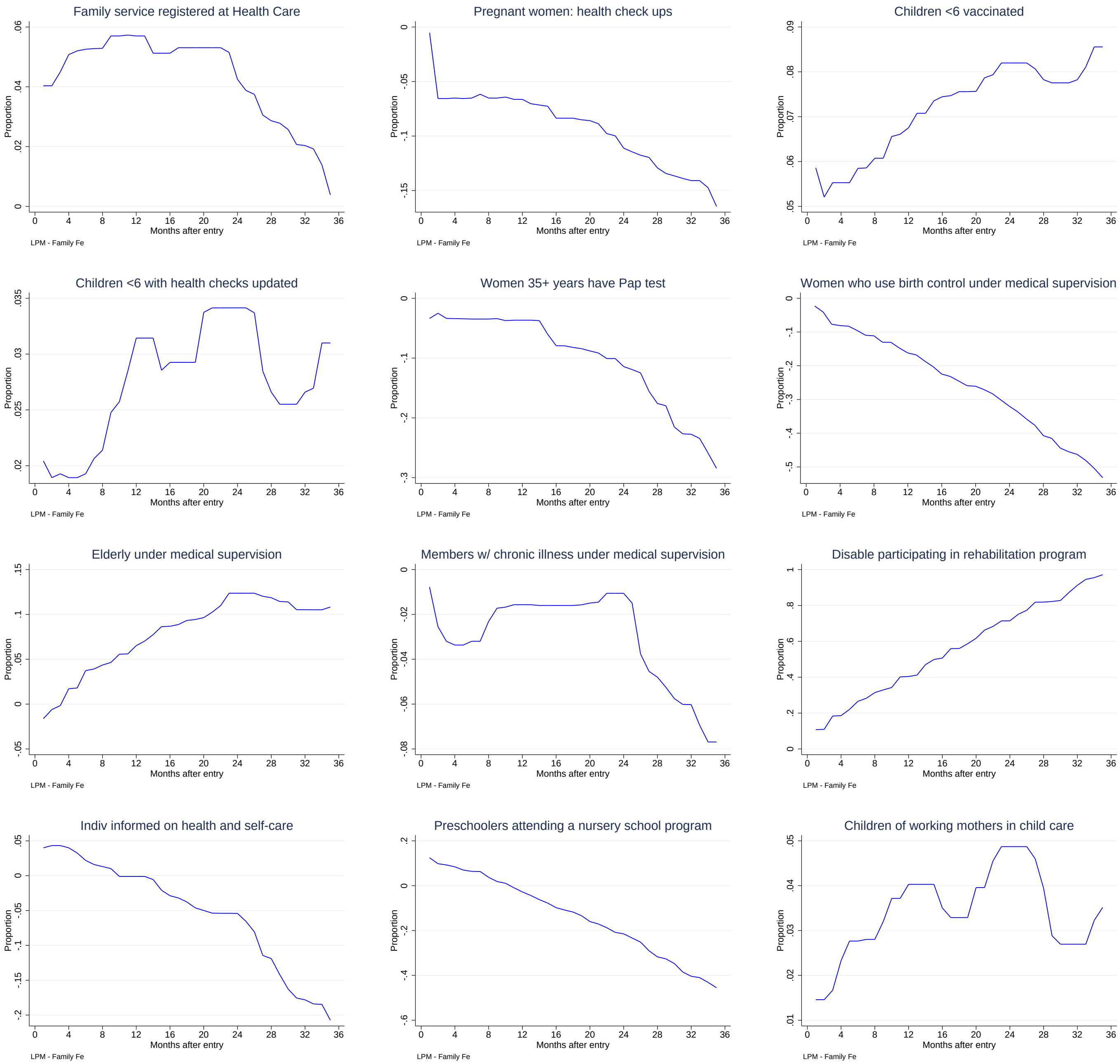
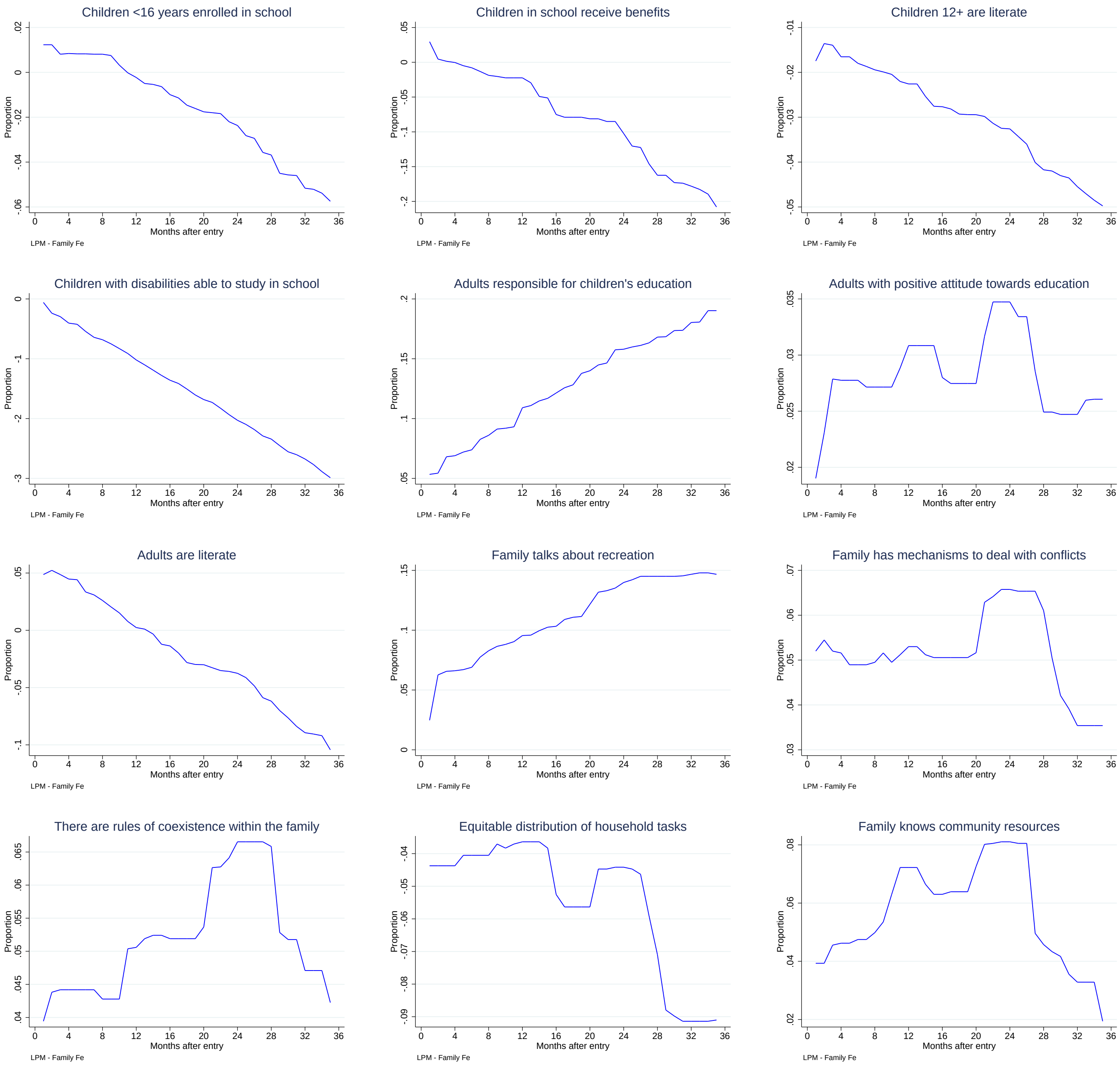
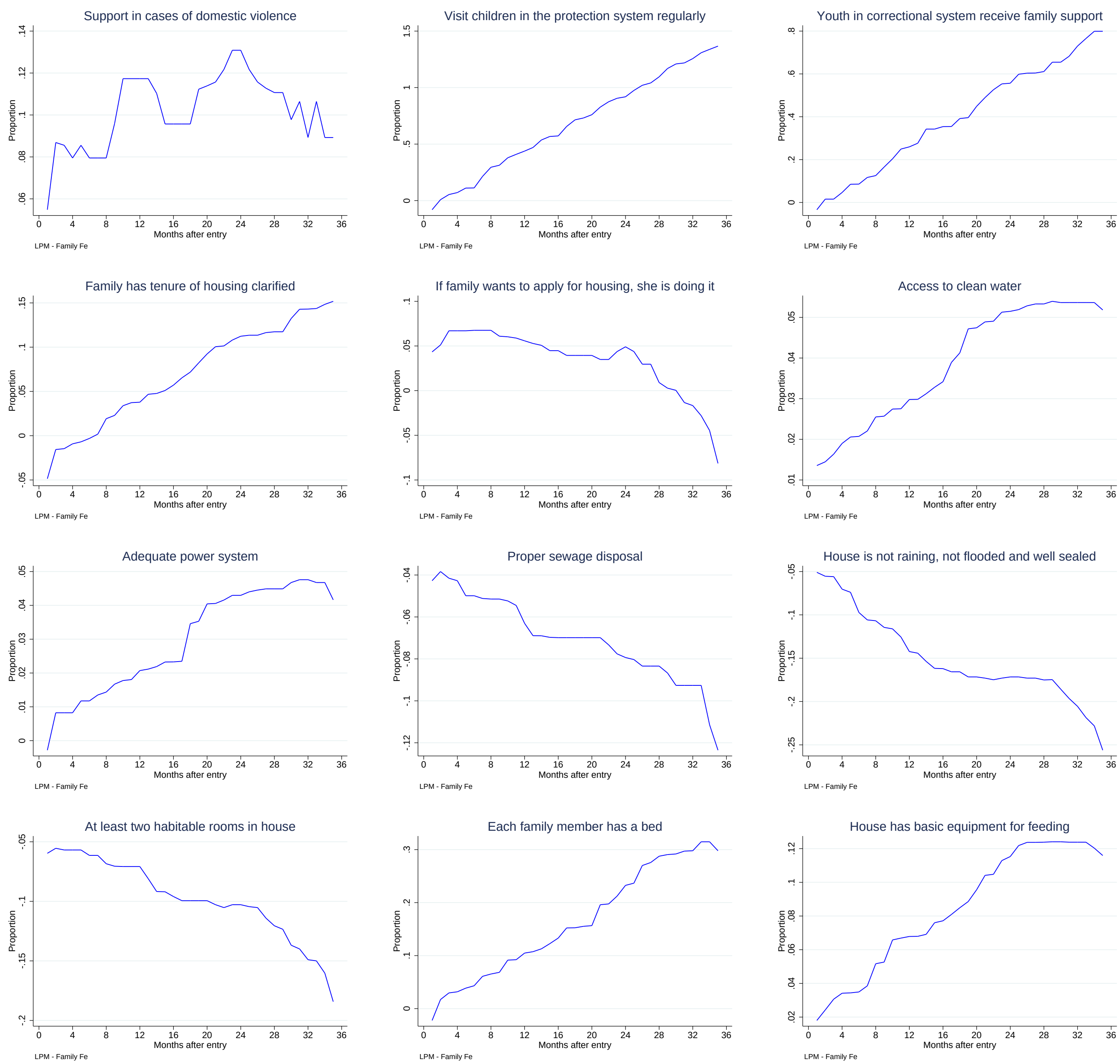


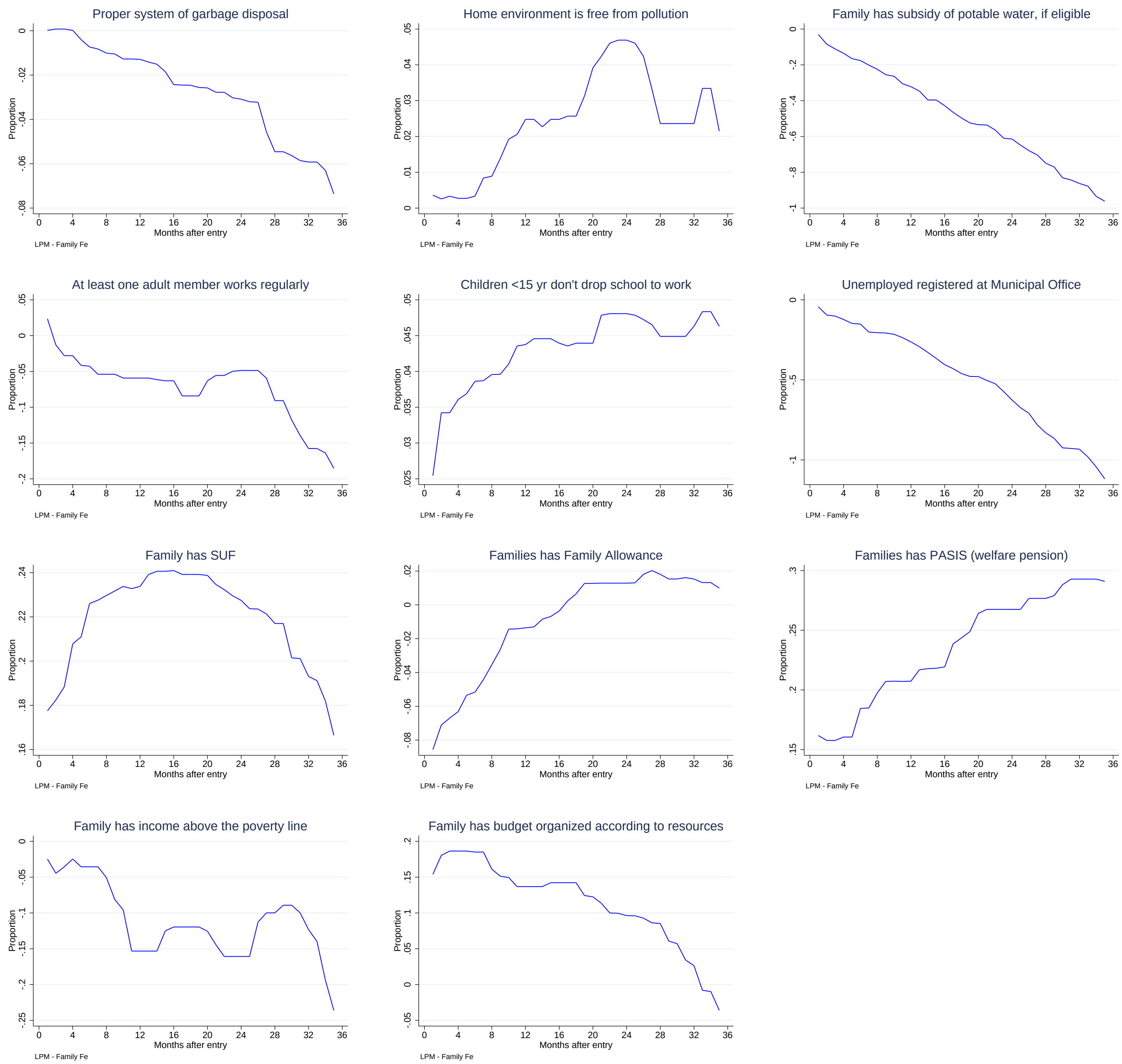
Figure B.4: Minimum Conditions.



Minimum Conditions.



Minimum Conditions.



Minimum Conditions.

Note: The graphs present estimate for the parameter θ_t in the following equation

$$Y_{fmkt} = \alpha + \sum_{t=0}^{36} \theta_t T_{fmkt} + f(CAS_{fmk}) + \eta_f + \pi_k + \tau_{month} + \zeta_m + \nu_{fmkt}$$

where Y_{fmkt} is a minimum condition Y_{fmkt} of family f, residing in municipality m when entered CS, k is the year of entry (2002 to 2006) and $month$ is the month when the Puente data we have access to was recorded (August, September or December). Y_{fmk0} is an indicator for whether the condition was fulfilled at entry in the program (whenever applied to a particular family). T_{fmkt} are 35 indicators for the number of months since entry in Chile Solidario (0 is the month of entry and excluded category), indicators for month of Puente survey (τ_{month}), cohort of entry (τ_{month}) and municipality of residence (ζ_m), also a family fixed effect (η_f). The figures include only those families whose CAS at entry in CS was at most 20-points apart from the cutoff.

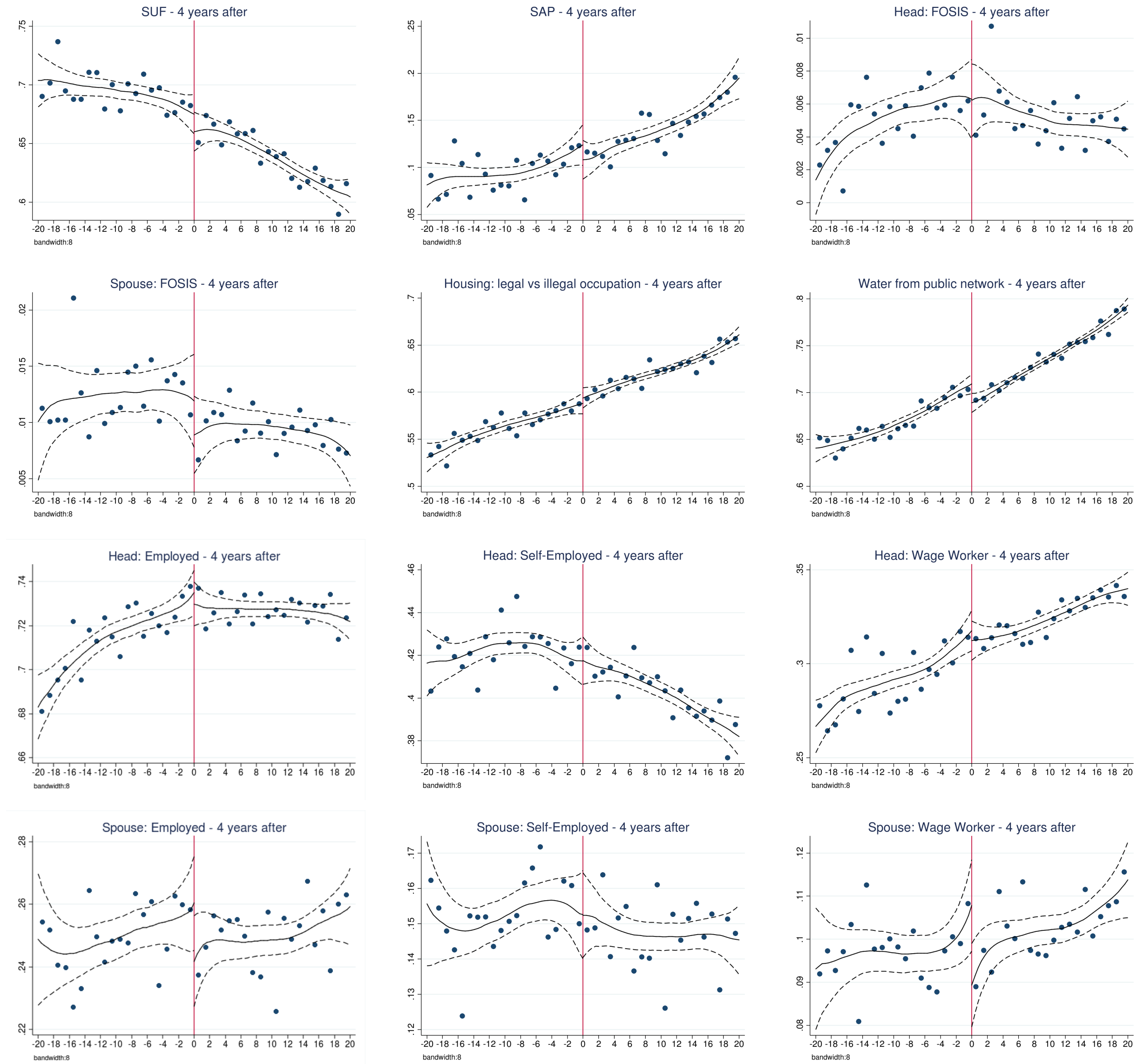


Figure B.5: Average outcomes by eligibility status, Bandwidth = 8.

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.

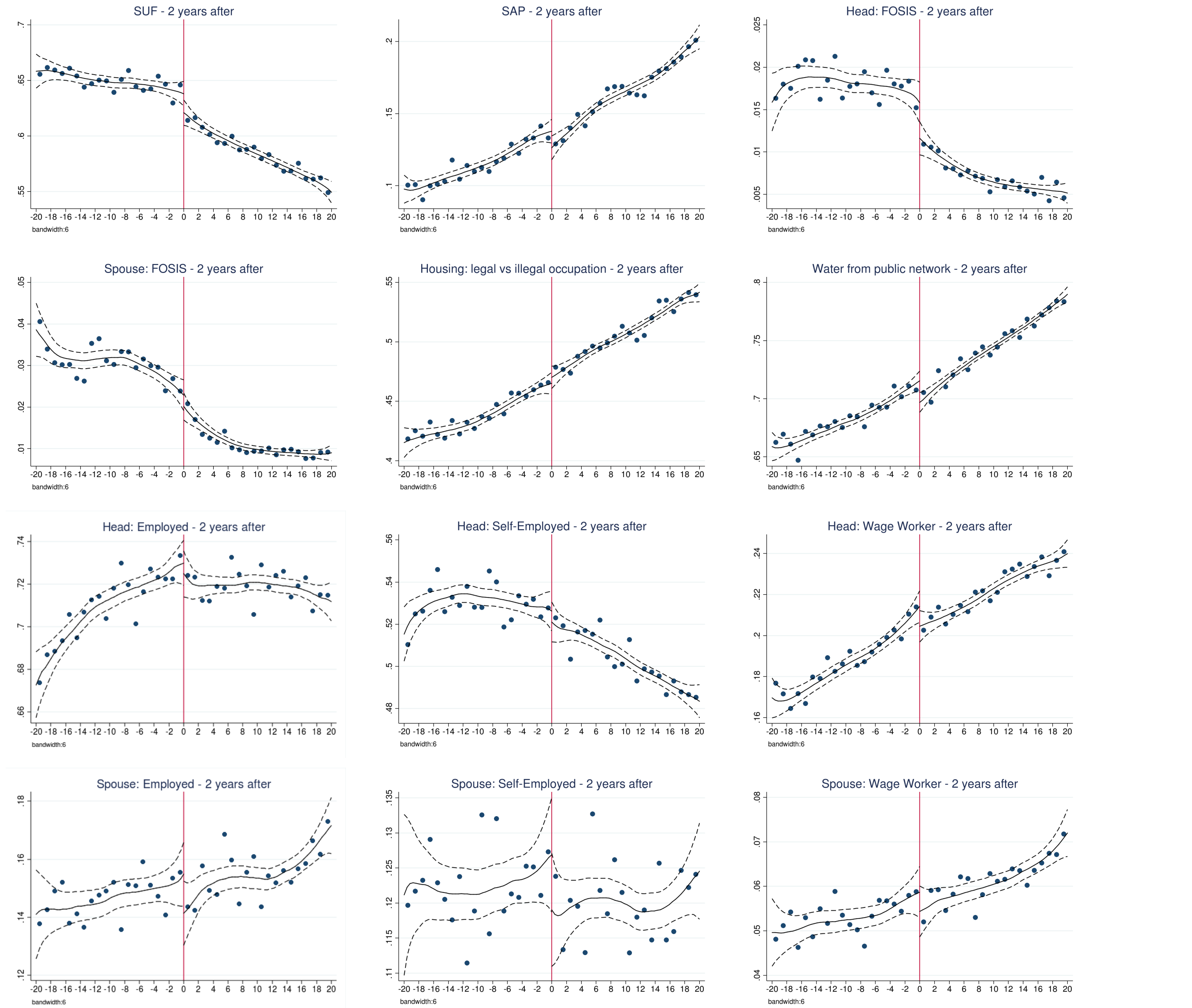


Figure B.6: Average outcomes by eligibility status, Bandwidth = 6.

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.

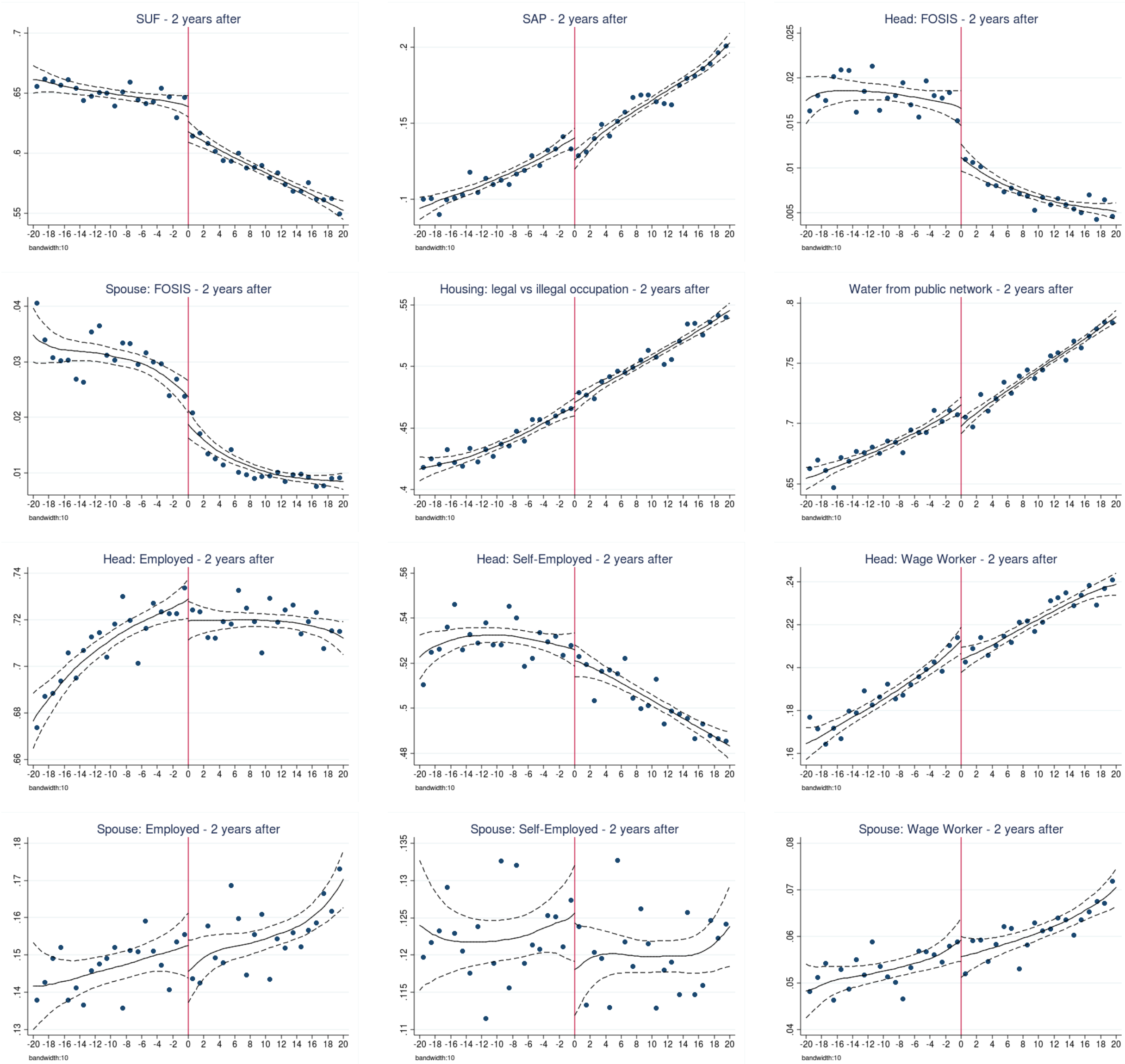


Figure B.7: Average outcomes by eligibility status, Bandwidth = 10.

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.

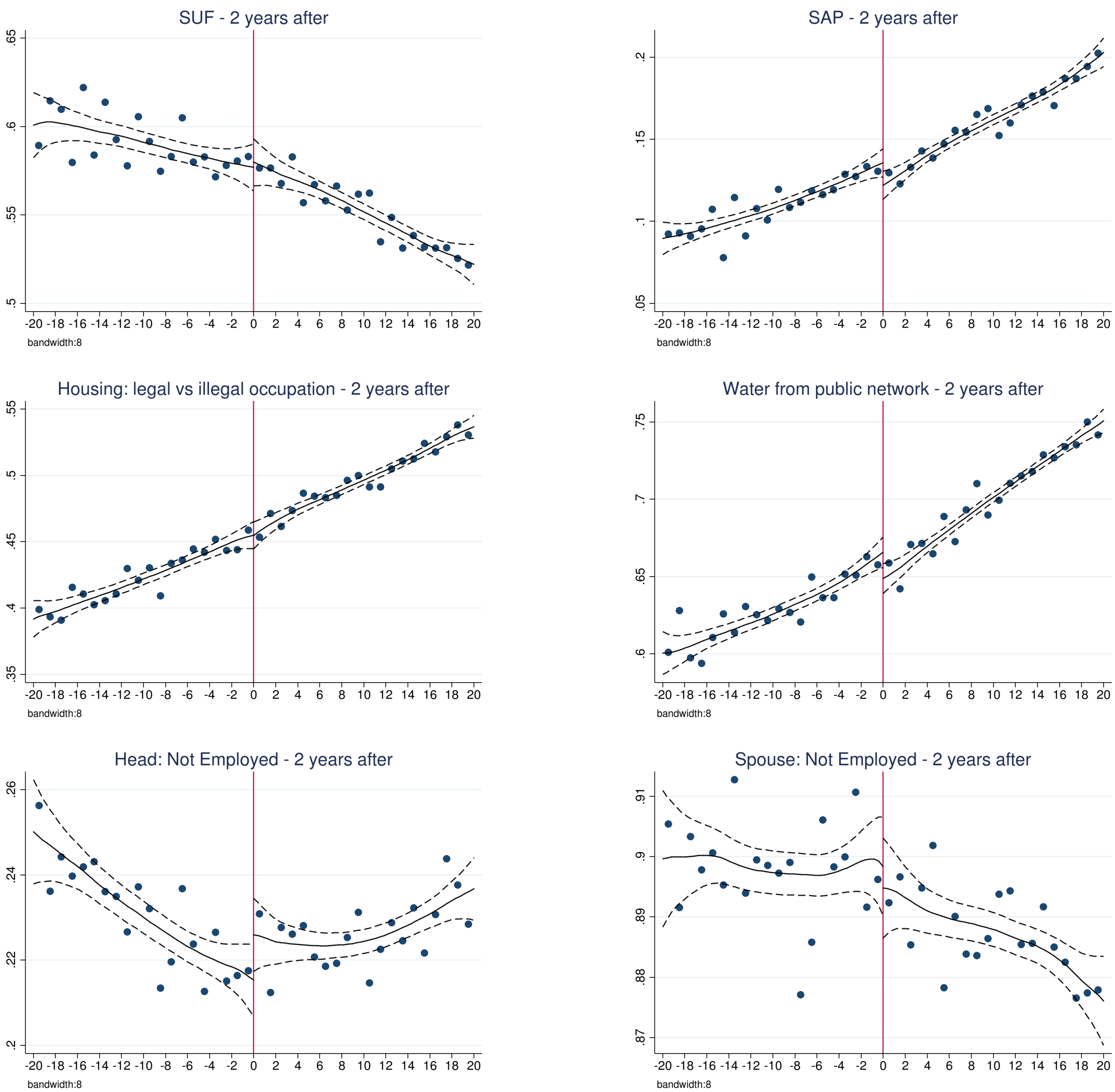


Figure B.8: Balancing checks: Pre-2002 outcomes by eligibility status, Bandwidth = 8.
Note: The continuous lines in figure present local linear regression estimates of several outcomes on percentage distance to cutoff. Circles in figures represent the mean outcome by cell within intervals of 2 points 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.

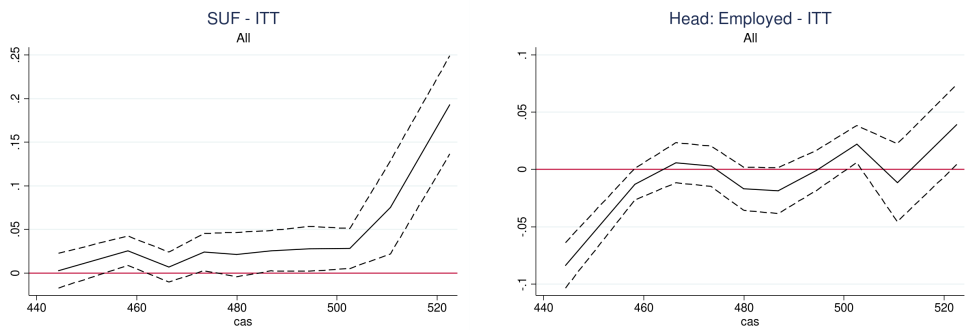


Figure B.9: Heterogeneity of effects by CAS: SUF and employment status of head.
Note: The continuous lines in figure present estimates an indicator for whether the head is employed on 10 indicator variables which take value 1 if the CAS score of the family measured two years before the outcome falls in one the 10 (equally-spaced) intervals defined between 395 and 584 points (the range of values in the sample), each of these 10 indicators interacted with eligibility measured also two years before the outcome, and fixed effect for the interaction between municipality of residence and year. The dashed lines are 95% confidence intervals.

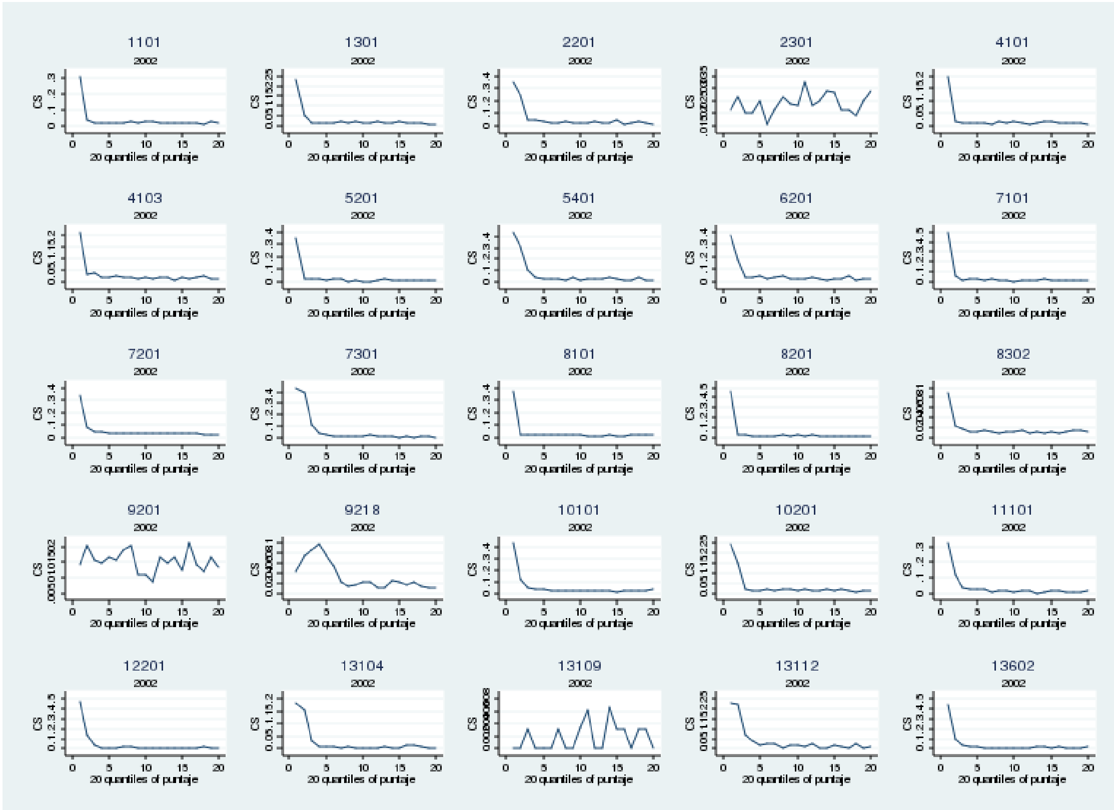


Figure B.10: Entry of families in CS in 2002 along the distribution of CAS within each municipality. Note: The graphs include the average participation in each municipality by each vingtile of the distribution of CAS.

C Dynamic RD and repeated observations

Equation (1) is static, while selection into CS is dynamic. Therefore, we need to adapt the standard RD procedure to our setting, which is similar to that of 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 can use a version of their procedure to test whether this subsequent entry invalidates our basic 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:

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

where we instrument CS_{2002im} with E_{2002im}.

Similarly, for those that could have started the program in 2002 we can estimate

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

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:

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

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

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

Assuming constant effects across cohorts and individuals, the impact of being in the program for 2 years, β^2, is given by

$$E(Y_2|E_1 = 1) - E((Y_2|E_1 = 0) = \\ \beta^2[E(CS_1|E_1 = 1) - E(CS_1|E_1 = 0)] + \beta^1[E(CS_2|E_1 = 1) - E(CS_2|E_1 = 0)]$$

then,

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

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

$$E(Y_3|E_1 = 1) - E(Y_3|E_1 = 0) = \beta^3 [E(CS_1|E_1 = 1) - E(CS_1|E_1 = 0)] \\ +\beta^2 [E(CS_2|E_1 = 1) - E(CS_2|E_1 = 0)] + \beta^1 [E(CS_3|E_1 = 1) - E(CS_3|E_1 = 0)]$$

Thus,

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

β^3 is obtained as follows. Given β^2 and β^1 from equations 8 and 9, and estimates of π^1 (which is estimated as above) and π^2, which is the ratio of the following coefficients: (i) the coefficient on eligibility in $t = 1$, E_1, from a regression of an indicator of entry in CS in $t = 3$, CS_3, on eligibility in $t = 1$, E_1 (controlling for a function of CAS in $t = 1$, which we omit above to simplify notation), conditional on not having started CS by $t = 2$, $CS_2 = 0$; 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).

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). In our empirical application we relax this assumption.[32]

Table (see table C.1) shows that estimates of the main results are largely unaffected by the dynamic entry into the program around the threshold. Most of the πs are very small in magnitude (see table C.2). The coefficients of βs for the take-up of public subsidies are all within 95% of the θs in table C.1.

[32] Cellini, Ferreira and Rothstein (2010) also suggest a one step procedure to estimate β^k which is much more efficient making explicit use of the panel structure of the data. In particular, they estimate:

$$Y_{tim} = \alpha + \sum_{k=1}^{t-2002} D_{tk} \left[\beta^k CS_{t-k,im} + f_{t-km}\left(CAS_{t-k,im} - \overline{CAS}_{t-k,m}\right)\right] + \varepsilon_{tim}$$

where $CS_{t-k,im}$ is instrumented with $E_{t-k,im}$ and $D_{tk} = 1$ if $CS_{t-k-1} = 0$. Unfortunately, due to attrition we cannot implement such model.

Table C.1: 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
Participation									
SUF	0.110***	0.096***	2004-2008	0.090	0.083*	2006-2008	0.369***	0.281**	2008
	(0.033)	(0.033)		(0.058)	(0.047)		(0.139)	(0.124)	
Labor market									
Employed (head)	0.051*	0.022	2004-2008	0.008	0.066***	2006-2009	-0.110*	-0.083	2008-2009
	(0.029)	(0.033)		(0.037)	(0.020)		(0.062)	(0.079)	
Employed (spouse)	0.015	0.016	2004-2008	0.051	0.023	2006-2009	-0.022	-0.020	2008-2009
	(0.029)	(0.026)		(0.051)	(0.057)		(0.086)	(0.080)	

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 4. 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%.

Table C.2: 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***	0.138***	0.148***	0.099***	0.072***
	(0.010)	(0.008)	(0.010)	(0.012)	(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***	0.077***	0.063***	0.038***
		(0.008)	(0.009)	(0.009)	(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***	0.046***	0.050***
			(0.012)	(0.010)	(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**	0.064***
				(0.011)	(0.022)
Observations				12,694	4,230
Control Mean				0.142	0.114
			Panel E		
$CS_{t-(k-5)}$					0.032
					(0.021)
Observations					3,583
Control Mean					0.106

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 specific to the outcome analyzed, and the results presented in table concern the sample used to estimate the effects on SUF. The estimates for other outcomes are similar and are available from the authors.

"Control Mean" is the control mean, that is, the 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%.

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.

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. Different families living in the same house may have a different CAS-score as long as they have different characteristics of income, education and activity. 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. This is a survey that should be filled at family's house and in order to attest the credibility of information provided 20% of all valid surveys are randomly chosen to be re-interviewed by a supervisor and all surveys with invalid entries are revised and if necessary households are re-interview (Ministry of Planning, 2003).

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. So the data set excludes all families who have not applied for any social benefit. However, it is important to notice that we do not necessarily need the whole population to do a proper evaluation. Indeed, the population of interest is the population of beneficiaries and potential beneficiaries of the program, and there was a strong effort on the part of the government to make sure that most of the poor did have a Ficha CAS when the program was implemented in 2002.

The variables used to construct the CAS score are as follows:

1. Walls: The variable combines the material of the walls and the area of residence. The result is a total of 81 categories, each with a recording the quality score of the walls in relation to the residence (e.g., the most extreme climates require higher quality of construction).

2. Floor: It is constructed from a combination of the flooring material and the area of residence in the country. In this case one obtains a total of 63 categories.

3. Roof: As in the previous cases, the material is constructed by combining the roof area of residence, obtaining a total of 72 categories.

4. Overcrowding: This variable is constructed as the ratio between the number of people and the number of bedrooms.

Table D.1: Relative Weights of Each Variable in the CAS score.

Area	Weight	Sub area	Weight	Variables	Weight
Housing	0.26	environmental protection	0.4	Walls	0.35
				Floor	0.35
				Ceiling	0.3
		Overcrowding	0.22	Persons/bedroom	1
		sanitation and comfort	0.38	Water	0.35
				Sewage	0.3
				Bath-tub	0.35
Education	0.25			Years of schooling of head	1
Occupation	0.22			Highest occupational category of the couple	1
Income/Wealth	0.27	Income	0.43	Family per capita income	1
		Site	0.13	Property	1
		Appliances	0.44	Fridge	0.5
				Water Heater	0.5

5. Water: The variable is constructed on the basis of indicators of the availability water utility, how one enters the item into the house or if necessary carry it from elsewhere.

6. Sewage: Distinguishes between areas of urban or rural residence and one gets a total of 18 categories for nine sewage disposal systems and two areas of residence.

7. Shower: Refers to the availability of shower in the home, which can be exclusive or shared with other homes in the same place. Also differenced by the availability of hot water.

8. Education: This variable is referred to the years of education taken by the head of household and it is truncated at 18 years of schooling.

9. Occupation: This variable is based on the highest "employment status" among the chief of household and spouse (if applicable). The classification is based on the occupational category, resulting in a total of nine categories.

10. Family income per capita.

11. Ownership of the site where the home is located.

12. Refrigerator.

13. Availability of water heater.

Application of the survey A family may request to be surveyed if they intend to apply for a benefit. It is also possible that the local authority takes the initiative to survey a family to learn about its vulnerability. The municipality is obligated by law to interview all families who submit the request to be surveyed. Whenever there is a change of address, an individual needs to contact the new municipality of residence and request a score update. 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.

Interviews are conducted by individuals hired by municipalities especially for this purpose.[33] They should be conducted in the home of each respondent, and all answers should be given by the head of the family or his/her partner. Only in special cases may other family members answer to the survey questions, once they are authorized by the head of household. The interviewer should ask for official documents when recording information about individuals' identification and income: identity card, marriage certificate, pay-slips and other income (but it is unclear how rigorously this is applied). Once the information is processed, the municipalities inform the managers of social programs about the CAS score of applicants.[34]

Verification of the quality of the data can take place in three separate ways: (1) a simple check of whether the interview was applied to a family, (2) a review of surveys commissioned by a designated reviewer and (3) a required re-interview of no less than 20% of the questionnaires without any apparent problems (i.e., without omissions, inconsistencies, values out of possible ranges or incorrectly assigned). Although the standard review process was described by the Ministry of Planning and should be implemented by each municipality, there are no records of how effectively this control is implemented (Larrañaga, 2005).[35]

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 aims at assessing the household income generating capacity and its vulnerability to shocks. This is a significant change from the CAS, which weighed heavily on assets and durables ownership, making it more persistent. 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 (for example, the 2004 wave of CAS contains data on families who (re-)enrolled between January 2003 and December 2004), the FPS-score (2007-2009) is updated monthly.[36]

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. Ficha CAS contains information on participation on welfare programs and this allows us to measure effectiveness of Chile Solidario to help families taking these programs. FPS contains variables related with use of health facilities, school

[33]Interviewers must be over 18 years and with secondary education. Persons hired must be submitted to a spelling test, calligraphy and must be familiar with an interviewer's manual.

[34]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).

[35]The variables are divided into two types according to the method of verification. Variables related to the durability of housing and durable goods are directly observable and verifiable (e.g., floor, walls, ceiling, water heater, refrigerator, water access, shower and sewage). Together these variables accounted for 32.1% of CAS index. Then, there are some variables that require external verification (e.g., education of the head, occupation of the head or spouse, site ownership, income and overcrowding).

[36]The following reasons may cause a change in the score: 1) death if a family member; 2) if an individual receives a new pension or enters retirement, or turns 60, the cross-check between data-bases means that score changes automatically; 3) the family should also notify the municipality if child is born, if it changes the address or if the head of household changes. Therefore, unlike CAS, updates to the FPS-score may be come via two methods: 1) What is called "por sistema" (by the system): which includes the history of deaths and age changes; or 2) By request.

attendance by children, disability status of members and alcohol and drugs use of family members. These changes aimed to improve the selection of the potential beneficiaries of social programs and benefits, accounting not only their socioeconomic status, but also to different needs and specific situations, such as disability, old age, unemployment, low income, illness, among other vulnerabilities. Therefore, the FPS scores resulting from the application of the Ficha combine three elements: 1) the income-generating capacity of each of the members of family; 2) the income of the family, obtained from the sum of those resources come from retirements, pensions, widow's (permanent income) and income reported by people; 3) family needs according to their size and composition: the age of its members and their dependency status, so they are used for questions relating to health and disability.

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)[37], to missing if it is too small (1000 or less) and flag observations with the same identifier[38]. 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.

[37]The 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.

[38]An 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.

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 nonmissing observations) the period of income reported is 0, which is an unassigned code.

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

We have data on all families that were ever invited to participate in Chile Solidario between 2002 (when the program was implemented) and May 2009 (since the mechanism of assignment of families into the program changed in 2007, which is beyond the scope of this paper, we do not use information on families starting CS after 2006). We obtained from the administration of the program data for the participants measured at six different dates: December 2003, September 2004, September 2005, August 2006, August 2007 and May 2009. In these six snapshots we have one observation for each family enrolled in the program and her identification number, which enables to link the six snapshots of data, and, most important, 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).

For the first four waves of data, we observe information about 47 out of the 53 minimum conditions which families should work during the two years of home visits (except that for the 2003 wave the data does not have information for the five minimum conditions on the area of income; see table D.3). For each of these four waves and for each of these conditions we have information about: (1) whether a particular condition was fulfilled at entry in CS; (2) whether it was fulfilled during program; (3) if families and social worker are working on the condition, and (4) if it does not apply to the family's situation.

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/`.

[2] Raczynski, Dagmar, 2008, "Sistema Chile Solidario y la Politica de Proteccion Social de Chile - lecciones del pasado y agenda para el futuro", Corporación de Estudios para Latinoamrica, CIEPLAN, Santiago de Chile.

Table D.2: Entry in CAS and FPS system.

	First survey										
year	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	Total
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.

Table D.3: Description of Puente Registers.

Date	Content	Information
December 2003	Contains information for the 101790 families enrolled in the program by end of December 2003.	Situation for each of 42/53 MC (no information on MC on income area); RUN of SW; situation in program (active, denied invitation to participate, quitted program).
September 2004	Contains information for the 128007 families enrolled in the program by September 2004.	Situation for each of 47/53 MC (no information on the 8 MCs on income area; no information on the 6 MCs on identification area).; RUN of SW; situation in program (active, denied invitation to participate, quitted program, in follow-up phase); date of last visit.
September 2005	Contains information for the 189534 families enrolled in the program by September 2005.	Situation for each of 47/53 MC (no information on the 8 MCs on income area; no information on the 6 MCs on identification area).; RUN of SW; situation in program (active, denied invitation to participate, quitted program, in follow-up phase); date of last visit.
August 2006	Contains information for the 235144 families enrolled in the program by August 2006.	Situation for each of 47/53 MC (no information on the 8 MCs on income area; no information on the 6 MCs on identification area); RUN of SW; situation in program (active, denied invitation to participate, quitted program, in follow-up phase); date of last visit.
August 2007	Contains information for the 239210 families enrolled in the program by August 2007.	Situation for the number of MC fulfilled, without details on the particular situation of each condition; RUN of SW; situation in program (active, denied invitation to participate, quitted program, in follow-up phase); date of last visit.
May 2009	Information for the 89548 families that enrolled in the program between September 2007 and May 2009	Situation in program (active, denied invitation to participate, quitted program, in follow-up phase).

Note: ”MC” refers to minimum condition; ”SW” refers to social workers.