Tackling Social Exclusion: Evidence from Chile

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Abstract

We study an intensive social programme in Chile that combines home visits to households in extreme poverty with guaranteed access to social services. The goal of the programme was to connect marginalized families to the social system, which, in turn, should lead to improvements in their living conditions. Programme impacts are identified using a regression discontinuity design, that explores the fact that programme eligibility is a discontinuous function of an index of family income and assets. There is no evidence of short- or long-term effects of the programme on employment or housing outcomes. However, we find short- and medium-term impacts of the programme on the take-up of subsidies and employment services, which are concentrated among families who had little access to the welfare system prior to the intervention.

Keywords: Social Exclusion, Social Protection, Chile, Extreme Poverty.
JEL Codes: C26, I38, J08

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1 Introduction

Households in extreme poverty are generally deprived in multiple dimensions. The lack of material resources, assets and skills, little access to information, and other constraints to households' decision-making ability limit the actions they can take towards improving their lives.

In 2002, Chile implemented *Chile Solidario* (CS hereafter), a very comprehensive anti-poverty programme, even by the standards of most countries in the developed world. The programme targeted the 5% poorest families in Chile, who were perceived to be not only poor, but also alienated from the welfare services potentially available to them. CS is a programme of general interest because it is a serious attempt to integrate several welfare services to tackle social exclusion, and because of its ambitious objective of connecting the most disadvantaged families in society to the welfare system in a sustained way. The programme had two main components. On the demand side, home visits and personalised counselling performed by local social workers helped households acquire the skills they need to autonomously participate in (and benefit from) the services available to them. On the supply side, the state committed to coordinating the different government agencies providing the social services.

Governments in middle and high income countries repeatedly express concern about the poorest families in their countries, and the difficulty of designing effective policies to support those who are the hardest to reach. Many other Latin American countries began looking at the Chilean system of integrated social services as an example for their own policies. A few of them have introduced programmes that mimic several aspects of CS (such as *Juntos/Unidos* in Colombia, *Brasil Sem Miséria* in Brazil and the most recent version of the Oportunidades programme in Mexico, called *Prospera*). Learning from CS as a large-scale effort to tackle social exclusion and extreme poverty is also important in light of the recent evidence of integrated and multifaceted interventions targeting the extreme poor (Banerjee et al., 2015; Bandiera et al., 2017).[1]

This paper uses administrative data to study the short- and medium-term impacts for cohorts of families who were exposed to CS between 2002 and 2006. Our main results focus on a range of outcomes for which data are available: i.e., employment of different household members, housing conditions, and the take-up of other social services, such as monetary subsidies, and training and employment programmes.[2]

[1] Most of the evidence on multifaced interventions comes from poor countries and rural contexts. The evidence is scant for urban settings and middle income or developed countries. Thus, the CS findings are also relevant to the social policy debate in Europe, where different countries are working on integrating social assistance and income-support schemes (Atkinson, 2015; Immervoll and Scarpetta, 2012; Markussen and Røed, 2016). In 2007, Colombia introduced a programme similar to CS, but which suffered from a number of implementation problems (Abramovsky et al., 2016).

[2] The choice of outcomes is dictated by the use of administrative data, which has a more limited set of outcomes than a household survey. We also analyse additional variables concerning school enrolment of children, health coverage of children and adults in the household, and participation in employment centres, but do not report them as main results as they are available only for the last three years of data. The benefit of using administrative records is that

We do not find any impacts of the programme on the employment of adult members of targeted households. We also do not find any improvements on a wide range of housing conditions. Overall, this programme does not seem to have real impacts on the lives of its participants in the short or medium term. However, we find that CS participants increase their take-up of a family allowance for poor children (the *Subsidio Único Familiar*, hereafter SUF).[3] We are also able to detect an increase in the uptake of employment programmes. In sum, CS participants become better connected with the social welfare system in Chile. We cannot say whether, as a result, they experienced real improvements in their lives.

To evaluate the programme we use a regression discontinuity design. Families are eligible to participate in CS if a poverty index is below a given cutoff, which varies across municipalities and across years. We compare, within municipality and year, the outcomes of families who are just below the cutoff with those families who are just above it. The discontinuity in the probability of participation in CS induced by the poverty index is not sharp, but fuzzy because not all families identified as a target of the programme in 2001 were immediately served due to supply constraints. CS was rolled out for a period of 5 years and about 20% of the 225,000 target families were enrolled annually, with priority given to the poorest. Thus, the cutoffs are not observed in the initial years of the programme and we estimate the effective cutoffs using the procedures applied in Chay, McEwan and Urquiola (2005) and Card, Mas and Rothstein (2008). We show graphically that there are large discontinuities in participation at the estimated cutoffs in about 80% of the municipalities in Chile. In addition, for those municipalities where the discontinuity in CS participation is not visible at the estimated cutoff, it is also not visible for household outcomes.

For households with a poverty index in the neighbourhood of the discontinuity, the average impact of being eligible for CS in a given year on participation in the programme is about 12-13%. 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 programme. We use administrative records covering a period of ten years (2000-2009). We start observing families at least a year before the introduction of CS; these families were followed for two, four and up to six years after entry into the programme. Having access to administrative data over such a long horizon is important because programmes of this type often take a few years until they become fully functional.

From a methodological standpoint, our regression discontinuity estimator is likely to produce

we can study the universe of participants to the welfare system and track them for a long period of time. The cost is that we are not able to track outcomes such as consumption or income, and more in-depth measures of subjective well being and mental health.

[3] Barham, Macours and Maluccio (2013) and Barham, Macours and Maluccio (2017) study an example of take-up of transfer for poor families with children in Nicaragua with sustained impacts on school achievement 10 years after entry in programme. This echoes the US evidence showing that access to safety nets and social assistance programs have long term effects on adult outcomes (Hoynes, Schanzenbach and Almond, 2016; Aizer et al., 2016).

more credible estimates of programme impacts than alternatives such as difference-in-difference estimators, which are likely to be affected by violations of the common trends assumption. Serious threats to the internal validity of our estimates are unlikely, but there are two challenges to their external validity. On the one hand, as in any RD estimator, the sub-sample of the population being studied is located just in the neighbourhood of the discontinuity. However, because we observe many different discontinuities across municipalities, this problem is much less serious. The group of families we are probably missing are those located at the very bottom of the income distribution, who are never in the neighbourhood of any cutoff, even in municipalities where this cutoff is relatively low. On the other hand, there are municipalities where the observed discontinuity is large and others where it is not. Only the first municipalities contribute information to our estimates. We show that there are not large differences between the two types of municipalities except that municipalities with large discontinuities are more likely to be capital of province (and, thus, urban). Therefore, our estimated impacts are more likely to be relevant for the urban poor than for the rural poor.

Previous work on CS has overlooked unique aspects of the program, particularly, the focus of the programme on families more severely excluded from access to the social services. Moreover, other existing studies either focus on short-term impacts (Galasso, 2011) or on the impacts of families starting the programme in 2002 or 2003 (Galasso, 2011; Larranaga and Tagle, 2009; de la Guardia, Hojman and Larranaga, 2011; Sarzosa and Urzua, 2012), but a focus on the early stages of the programme may be misleading because CS was not yet working at full capacity during this time.[4]

The paper proceeds as follows. We describe the programme in the next section. 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 government to reach families who lived in extreme poverty in 2002. We focus on the first five cohorts of entrants (2002-2006), because the targeting mechanism and design of CS changed substantially after 2006. Here we describe the main components of CS, and defer

[4] Galasso (2011) finds that the programme had significant impacts on the education and health of households, and the take-up of social benefits. However, her results rely on a non-representative survey, which was designed in a very specific way, to fit the use of a matching estimator. Larranaga and Tagle (2009) and de la Guardia, Hojman and Larranaga (2011) were developed contemporaneously with our paper; they find small impacts of the programme across a variety of dimensions. They rely on differences-in-differences and matching estimators, which may be subject to likely violations of the common trends assumption. Sarzosa and Urzua (2012) use the RD strategy introduced in Galasso (2011) and refined in this paper. They use only on the first cohort of CS and focus on test scores of children obtained from school records, which they were able to merge with the same CAS data set we use in this paper.

to section 3 details about eligibility rules to the programme because they are used to identify its impacts.

Home visits The home-visiting component of CS lasts for 24 months. During this period, social workers conduct 21 home visits of 40–45 minutes each. These visits are more frequent at the beginning of this 24 months period, and their frequency decreases gradually over time. During the initial CS home visits, the social worker and families sign a symbolic contract where they agree to work together to achieve a set of 53 living conditions and household behaviours, including issues such as family dynamics, housing, employment, health and education. Many of these minimum conditions were already satisfied by several households when they started the programme (see Table A.1 in Appendix A). The social worker provides information and active and practical guidance on how to access existing programmes and services which help improving these conditions.

On average, each social worker is responsible for 50 families per year (standard deviation 25).[5] The annual cost per family of home visits depends on the accessibility of municipalities; it ranges from USD100 in most accessible municipalities to USD500 in remote municipalities (this cost includes the cost of the visit, and the training and supervision of the social worker; see Mideplan (2009)).[6] In the first year of operation, home visits accounted for almost the full budget of the programme. After 2004, although the financial resources allocated to this component remained almost unchanged, its share of the total budget decreased to only 4% of the total budget (while subsidies and services directed to CS families gained weight in the overall budget; Raczynski (2008); Camacho and Silva (2014)).

Guaranteed access to monetary subsidies Participating families receive a monthly cash transfer for a period of five years (called *Bono Chile Solidario* during the first two years and *Bono de Engreso* in the last three years). This transfer is independent of family size. For example, in 2005,

[5] About 2400 social workers work under CS each year and there is substantial variability in their average caseload across the country. Social workers in remote regions, such as Aysen and Magallanes, have an average load of 16 families, whereas their workload is around 60 families in urban regions, such as Bio-Bio or Araucania. Data on the demographic characteristics for 559 social workers who worked on CS between 2007 and 2009 show that 88% are women, 80% have a degree in social work, and they are 26 years old on average. The turnover of social workers is substantial and 20% of CS families received visits by more than one social worker.

[6] For 2009, the year to which we have information on the structure of costs of CS obtained from former administrators of the program, there were three components of costs: (i) *cost of direct attention to the family*, including home visits by the social worker, materials that are given to the family and community education workshops; (2) *local service administration*, including administrative expenses, such as transportation, equipment, and life and accident insurance for the social worker and their training costs; and (3) *supervision, monitoring and technical assistance*, including central and regional staff responsible for monitoring and technical assistance and their administrative costs (such as field visits and transportation) and the maintenance of the online registration and monitoring system. The first component represents 78% of the costs, while the second and third components represent 13% and 9% of the costs, respectively.

the transfer starts at about USD19 per month and decreases over time to about USD7 per month.[7] The amount of the CS transfer is in general lower than that of other well-known cash transfers in Latin America, because its goal is just to compensate families for the costs of participating in the programme, instead of consisting of a subsistence transfer.[8]

If eligible, families in CS are also guaranteed access to the existing monthly (non-contributory) allowance available for poor families with children younger than 18 years of age (SUF - *Subsidio Único Familiar*); the pension for the elderly poor, for the disabled, and for individuals with mental disabilities (PASIS - *Pension Asistencial*); and the water subsidy (SAP - *Subsidio de Água Potable*), which covers up to 15 cubic meters of monthly consumption in the water bill. Eligibility for these transfers is based on family structure and income requirements. The score on a means test index (the CAS, which is described in detail below) is also used to determine priority in allocation of slots. In 2005 and 2006, the total payments of these three social subsidies and CS grants represented 63.5% of the total budget of the programme.[9]

Preferential access to social services and the reorganization of the supply side Participating families have preferential access to a whole array of social services available in their municipality of residence. Between 2002 and 2004, municipalities and local services providers simply improved the coordination of different programmes serving the target population without increasing the supply of services. With the approval of the law that regulates CS in 2004, there was an improvement in the quantity and quality of the supply of such services.[10] The budget share allocated to the

[7] In 2005, for the first six months of the programme, the monthly value of the transfer was 10,752 pesos (USD19.21) and for the last six months (of the 24 months of home visits) the value was 3,930 pesos (USD7.02); Raczynski (2008). These values correspond to 19.7% and 7.2% of the household monthly income among eligible families in 2005.

[8] For example, depending on the family structure, cash transfers for the poor from Mexican Oportunidades may exceed USD150 per month, and the Bolsa Familia monthly transfer in Brazil varies between USD40-60 per family. The monthly transfer associated to the Colombian programme *Familias en Acción* can be lower than the CS transfer, starting at USD8/month per child enrolled in school (Fiszbein et al., 2009).

[9] SUF is primarily aimed for every child in poor families without Social Security access. Beneficiary families must have a CAS score; the monthly income must be below the subsidy requested; and, children aged 6 or older must be enrolled in school and children younger than 6 years old must attend regular medical check-ups. The selection of beneficiaries who meet these requirements is based on the CAS score, and the programme targets the poorest 40% of the country. Since the CS targets the poorest 5%, all families that meet the requirements above, and that are eligible to CS, are also eligible to SUF. Once selected a beneficiary, the payment of the SUF is received for three years and may be renewed until the child turns 18, as long as they continue to be eligible.

The PASIS is a pension for poor elderly or poor disabled individuals over 18 who are not entitled to Social Security pensions. Individuals are required to have a Ficha CAS and their income cannot exceed the value of the pension.

The SAP includes partial or total financing of monthly water and sewer systems for families who are up to date on the payment of the services, but that are unable to pay the full account. Beneficiaries are selected to PASIS and SAP on the basis of the CAS index score and of the resources allocated to each municipality for this purpose. The cutoffs for eligibility are set at much higher levels than for CS.

[10] New programmes were created and the existent ones redirected the supply geographically in proportion to the needs of CS families in each municipality (see Table A.2 in Appendix A).

provision of programmes to CS beneficiaries increased sixfold between 2003 and 2007 (Mideplan, 2009; Camacho and Silva, 2014). In 2005-2006, funding for these services represented one-third of the overall budget of the programme.

Employment and training programmes are of particular interest due to their potential effects on labour market outcomes and the long-term earnings profile of programme participants. As a result of the increase in funds provided to social services, the coverage of employment programmes grew from serving 24% of CS beneficiaries in 2004 to 100% of those in families deemed eligible to such programmes in 2007 (i.e., families which did not meet the corresponding minimum conditions at entry in CS).

3 Empirical Strategy

Our goal is to estimate the impact of participation in CS on a wide range of household outcomes. Our most basic empirical model is given by the following equation

$$Y_{imt} = \alpha + \beta CS_{imt-k} + f(X_{imt}) + \varepsilon_{imt} \tag{1}$$

where Y_{imt} is the outcome of interest for family i in municipality m and year t, CS_{imt-k} is a dummy variable indicating whether the family entered in CS at year $t-k$, X_{imt} is a vector of controls (entering through function $f(.)$), and ε_{imt} is the error term. β is the impact of the programme on Y which, in principle, can vary across individuals.

Participation in CS is not randomly assigned to families; therefore, it is potentially correlated with unobserved determinants of the outcomes Y_{imt}. For example, β may overestimate the impact of CS if the most motivated families among the poorest are those who enrol in the programme. β could also underestimate the impact of CS if priority in enrolment is given to the most disadvantaged families, who could also be the most resistant to engagement with the social worker.

To estimate the causal effect of the programme, we use a regression discontinuity design, which relies on the fact that eligibility to CS is determined by the score on a wealth index (the CAS) with cutoffs varying across municipalities. In each municipality m and time period t, we compare outcomes of families just below (just eligible) and just above their respective cutoffs when they were potential entrants to the programme (e.g., Hahn, Todd and der Klaauw (2001); Imbens and Lemieux (2008); Lee and Lemieux (2010)).

Selection of families and coverage The CAS score is constructed from an instrument used to select families for several social programmes in Chile (i.e., the Ficha CAS). The number of families to be offered CS in each municipality was assigned in proportion of the percentage of the

population in extreme poverty, P_m in municipality, based on the income distribution in 2000 (see Law 19,949 of 2004). Thus, the official cutoff score of CAS for each municipality was the value of CAS such that the proportion of families below that score within the municipality was exactly equal to P_m.[11]

In practice, there were capacity constraints and not all eligible families with a score below P_m were invited in the first year of operation. Out of the 225,000 families deemed eligible by 2002, the government decided to gradually phase in the program, serving around 50,000 families per year up to 2005 (there was an additional provision to serve 50,000 more families in 2006). Within each municipality, families with the lowest CAS were supposed to be the first to enrol, although that did not always happen.

Effective Eligibility Cutoffs We do not know exactly how eligibility cutoffs for CAS were established in each municipality and year, but any such procedure would be one in which a cutoff was estimated at a particular date, so that all CS slots in a municipality were potentially exhausted for the following year. As mentioned above, the initial idea was to serve the poorest families first by strictly following the order of their CAS scores, which would mean that this cutoff could in principle be easily calculated from the existing CAS records.

However, this was almost never possible for many reasons. To begin with, many poor families were not even in the CAS system, or they resided in remote areas that were very hard to reach by social workers. Furthermore, there is substantial entry and exit of families from the CAS system, and among the existing families there are changes in their CAS scores (each family's CAS needs to be adjusted at least once every two years). So, even if it was possible to maintain a workable and up to date database that could be used to manage entry in real time right at the outset of the programme, the cutoffs for eligibility would have to be adjusted each time a new family entered the system, left the system, or experienced a change in the score, which was not practical. As a result, the set of families eligible or not for the programme could continuously change throughout the year.[12]

Given the limited number of available social workers in each municipality, their workload had to be organised geographically. To maximise the number of families served by the program, municipalities needed to optimise the amount of travel done by each social worker. This was done by assigning work areas to different social workers to ensure that all social workers avoided travelling around the entire area of the municipality. New families enrolling in CS in each area were

[11] In the initial stages of CS, there was an effort to register indigent families with the CAS system. However, new registration occurred only in a few isolated instances (Larranaga and Conterras, 2010).

[12] It is not likely that it would be possible to exclude families already invited even if they became ineligible in the meantime. It would also be equally difficult to exclude newly eligible families from entering the programme (and thereby exhausting one of the existing vacancies).

assigned to the social worker(s) already working in that area, provided that their caseload allowed for it. These work areas were small in dense urban locations, and large in sparsely populated rural locations. Accordingly, worker caseloads in urban areas were usually larger than in rural areas, where a social worker needs to travel large distances to see all families. If a potential new family was located in a neighbourhood with no social workers, or where social workers were operating at their maximum capacity, it was unlikely that it would have been served by the programme that year.

Therefore, in practice, municipalities had to project the number of families poor enough to enter the programme in a given year and they were simultaneously constrained to select families who were located in a catchment area of an existing social worker or in an area where the municipality was planning to introduce a social worker. The cutoff was set as a compromise between maximising the number of very poor families being served every year given the budget and minimising the social worker allocation costs.

Unfortunately, the *effective* eligibility cutoffs used by each municipality between 2002 and 2006 were not recorded in any of the programme data we have available. Therefore, we have to estimate them from data on the CAS scores and programme participation. We rely on a method used in Chay, McEwan and Urquiola (2005) and Card, Mas and Rothstein (2008), which is similar to identifying structural breaks in time series data.

This procedure works as follows. For each municipality m in year t, $t = 2002, ..., 2006$, we define a grid along the CAS distribution in the range $\left[CAS_{mt}^{\min} + 20, CAS_{mt}^{\max} - 20\right]$, where $CAS_{mt}^{\min}$ and $CAS_{mt}^{\max}$ are the lowest and highest values of CAS score in municipality m in year t, respectively. For each integer value of CAS in the grid, g, we define a dummy of potential eligibility $E_{imt}^{g} = 1\left[CAS_{imt} \leq g\right]$, thus g is a potential cutoff. Since the score is continuous and it can take values up to three decimals ranging between 350 and 770 points, the values of the grid g are restricted to be integer and the grid is update every two points in the interval $\left[CAS_{mt}^{\min} + 20, CAS_{mt}^{\max} - 20\right]$. Then, for each municipality m in year t, $t = 2002, ..., 2006$, we estimate the following equation using each value g in the grid,

$$D_{imt} = \alpha + \psi E_{imt}^{g} + \varepsilon_{imt} \tag{2}$$

where D_{imt} takes a value of 1 if family i in municipality m enters CS in year t, and 0 otherwise. We then select as the cutoff score the value of CAS g that maximises the R^2 of (2). Hansen (2000) shows that if equation (2) is correctly specified, this procedure yields a consistent estimate of the effective cutoff.[13] We use the estimated cutoff as the true cutoff in a standard regression

[13]For the sake of precision, we exclude municipalities with less than 50 families and municipalities without CS participants. This implies that we drop between 9 (in 2002) and 23 (in 2006) municipalities. In total there were 346 municipalities in Chile in this period.

discontinuity analysis.

Empirically, we show that there exist striking discontinuities in participation in CS as a function of CAS scores in several of the more than 300 municipalities in Chile. For example, in 2002, for 90% of the municipalities the estimated discontinuity size, $\widehat{\psi}$, is at least 7 percentage points. We also show that the participation rate of eligible families in the programme in each year is well below 100% for any given cohort. This is unlikely to be a demand problem, because virtually every family invited to CS accepted the invitation (out of all invited families, only 4.7% did not participate; see table A.3 in Appendix A). This just reflects binding supply constraints.[14]

Figure 1 plots the proportion of families participating in CS as a function of their CAS score measured in 2002 for 6 (randomly selected) municipalities. There are two vertical lines in each graph: a solid line indicating the estimated effective cutoff, and a dashed line indicating the official cutoff. The lines are superimposed when these cutoffs coincide. There are clear discontinuities in participation at the cut-off in the three municipalities at the top of Figure 1, while the discontinuities are not visible in the bottom three municipalities. Similar figures for all municipalities in Chile, and for 2002 and 2005 are shown in Figures C.1 and C.2 in Appendix C (the figures for 2003 and 2004 are available from the authors).

This means that our estimates are relevant only for the set of municipalities where there is a discontinuity in participation at the cutoff and municipalities with small discontinuities contribute no information to our estimates. Those municipalities with an effective cutoff are more likely to be a capital of province, and municipalities where households are, on average, relatively better off than in other places, as measured by a higher share of employed household heads and houses are also more likely to be connected to the public water supply network .

Eligibility and Participation In our setting, the mapping from eligibility to participation is not perfect. Municipalities had some discretion in the order of invitation of eligible households, according to the geographic matching of families to social workers; thus, there were families with the same CAS score but different participation status. Therefore, we will present instrumental variables estimates of the programme computed as described in expression (3) (for very small ε):

$$\beta^{IV} = \frac{\lim_{\varepsilon \longrightarrow 0^+} \Pr\left(Y_{imt} = 1 | CAS_{imt-k} = \overline{CAS}_{mt-k} - \varepsilon\right) - \lim_{\varepsilon \longrightarrow 0^+} \Pr\left(Y_{imt} = 1 | CAS_{imt-k} = \overline{CAS}_{mt-k} + \varepsilon\right)}{\lim_{\varepsilon \longrightarrow 0^+} \Pr\left(CS_{imt-k} = 1 | CAS_{imt-k} = \overline{CAS}_{mt-k} - \varepsilon\right) - \lim_{\varepsilon \longrightarrow 0^+} \Pr\left(CS_{imt-k} = 1 | CAS_{imt-k} = \overline{CAS}_{mt-k} + \varepsilon\right)}, \tag{3}$$

[14] As an illustration, Figure B.2 in Appendix B plots the distribution of estimated CS cutoffs across municipalities, for each year between 2002 and 2006. As expected by the gradual rollout from the bottom of the CAS distribution the distribution gradually shifted to the right over time on average. In 2002, the *effective cutoff* is lower than the official cutoff in 86.5% municipalities and in 2005 this proportion is reduced to 60%.

where the numerator of this expression is the discontinuity in the outcome at the cutoff of eligibility to CS ($\overline{CAS}_{mt-k}$), and the denominator is the discontinuity in the probability of participation in CS at this cutoff (see Hahn, Todd and der Klaauw (2001)). Families just above and just below the cutoff differ in their eligibility to CS, and in those variables the programme is meant to affect, but they are likely to be similar in all other (observable and unobservable) pre-programme dimensions. Below we show that this is indeed the case.[15]

We also restrict the sample to those families whose CAS is near the cutoff for the programme, as points away from the discontinuity should have no weight in the estimation of programme impacts (e.g., Black, Galdo and Smith (2007); Lee and Lemieux (2010)). We focus on the sample of families whose CAS score was at most 20 points away from their municipality's cutoff, but we also present estimates using alternative windows.

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

$$Y_{imt} = \phi + \gamma E_{imt-k} + f(CAS_{imt-k} - \overline{CAS}_{mt-k}) + u_{imt} \tag{4}$$

where E_{imt-k} is an indicator of eligibility for the programme and u_{imt-k} is an idiosyncratic shock. We control for a flexible function of CAS (normalised by the cutoff). In practice, we use a quadratic in $\left(CAS_{imt-k} - \overline{CAS}_{mt-k}\right)$, different on either side of the cutoff, but we also present a robustness analysis using other parameterisations of this function. Our models include municipality-year effects, which absorb municipality-year shocks that may affect the outcome independently of eligibility (e.g., shocks in the local supply of social services, or shocks to the local labour market). Standard errors are clustered at the municipality level (the municipality is measured at the time of eligibility).

We then compute programme 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 at time $t-k$, controlling for distance to cutoff through $f(CAS_{im} - \overline{CAS}_m)$. We then obtain the predicted probability of

[15] Among eligible families to CS, within each municipality, families who are selected to CS are less likely to have adequate walls or ceilings in their homes, less likely to be legal occupants of their home, and more likely to have a connection to the sewage network. Participant families have lower CAS scores than non-participant families; heads and spouses in participant families are more likely to be working than those in non-participating families. Selected families are more likely to have children, to be receiving subsidies, and to live in urban areas. These results are available from the authors.

participation in CS estimated, $\widehat{CS_{imt-k}}$. In the second stage we estimate:

$$Y_{imt} = \alpha + \theta \widehat{CS_{imt-k}} + g(CAS_{imt-k} - \overline{CAS}_{mt-k}) + \varepsilon_{imt} \tag{5}$$

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

We estimate models pooling all cohorts together as well as separately by cohort. When we pool them, we restrict the coefficients of the model to be the same across all cohorts.[16] We define cohorts of potential entrants in each year in terms of potential entry into the programme. Since 2002 is the first year of the program, every family who is in the CAS database in that year is a potential entrant; thus, they are labelled as belonging to the 2002 cohort. To define subsequent cohorts, we consider every family in the CAS database in a given year, but who has not enrolled in CS in any prior year. Throughout this paper, we refer interchangeably to the 2-, 4- and 6-year impacts as short-, medium- and long-term effects, respectively (the latter results are presented in the Appendix because long-term results are only available for the first two cohorts). Bai (1997) shows that sampling error in the location of a change point can be ignored in estimation of the magnitude of the break. We rely on this result and do not adjust our standard errors for the estimation of the cutoffs, as Card, Mas and Rothstein (2008).

Although it is simple to explain and implement, this procedure ignores potentially important enrolment dynamics, which may make our results hard to interpret. Take, for example, the case where the time between the measurement of the outcome and the measurement of eligibility ($t-k$) is large. Even in the case where programme impacts (e.g., θ in equation (5)) do not vary across individuals, equation (5) is potentially misspecified, because both participant and non-participant families in $t-k$ could potentially change participation status in $t-k+1$, or in any other year between $t-k$ and t. If that happened, then equation (5) would have to account for the complete history of programme participation.

This problem can be addressed. We consider this issue very carefully below following Cellini, Ferreira and Rothstein (2010). We implemented their procedure and we do not find evidence that considering explicitly these more complex dynamics substantially affect our results. Therefore, we present our simpler model in the paper, and the more complex model in Appendix D.

[16] Once a family enrols in CS, it remains in the programme for 5 years in total, even if its CAS score rises above the eligibility cutoff during this period. This means that, at each period of potential entry, eligibility only determines participation for those not yet enrolled in CS. Our estimates are valid for a sample which is changing over time, which is only important if programme impacts vary substantially across families.

Multiple Hypotheses Testing Since we examine multiple outcomes simultaneously, it is reasonable to correct our inference for multiple hypotheses testing. Whenever stated in the table notes, the stars next to each coefficient in the results indicate whether it is statistically different from zero, after accounting for multiple hypotheses testing using the procedure in algorithms 4.1 and 4.2 of Romano and Wolf (2005).

We test hypotheses for short- and medium-term estimates separately, but all the hypotheses of the different subgroups are jointly tested to control for splitting the population into multiple subgroups. This requires estimating for each bootstrap sample the models corresponding to Tables 3, 4, 5 and 6 (and A.8 in Appendix A). Thus, we test between 112 and 122 hypotheses simultaneously.[17] Algorithm 4.2 of Romano and Wolf (2005) is an iterative rejection/acceptance procedure, for a fixed level of significance. In the tables presented, the stars report significance levels of 1%, 5%, and 10%, which means that we apply the algorithm three times, once for each level. The critical values are adjusted for a two-sided test. We use 500 bootstrap replications, and we account for potential correlation of residuals within municipality using block-bootstrap.

4 Data

Our analysis is based on administrative records of all families applying for publicly provided social programmes in Chile. These records contain all components of the items used in the construction of the means test indices used to determine eligibility for these programs (the *CAS*[18] *Consolidado* for 2000-2006 and *Ficha de Proteccion Social* (FPS) for 2007-2009). In addition, we also use administrative records on participation in CS and other welfare programmes. The Ficha CAS was created in 1979. It was applied by the municipalities, and it covered about one-third of the Chilean population annually between 2000 and 2006. In 2007, the Ficha CAS was replaced by a new targeting instrument, the FPS. The FPS expanded the coverage from 2007 onward to reach two-thirds of the population by 2009 (see Appendix E for details).

In 1998, the Ficha CAS covered 91.5% of the poorest families in the country (defined as those at the bottom 10% of the country's income distribution; see Mideplan (2000) and Larranaga (2005)). The Ficha CAS consisted of a two-pages form that households were required to complete if they

[17] In columns 3 and 4 of Table 3 we test 122 hypotheses simultaneously and in columns 7 and 8 we test 112 hypotheses simultaneously. In particular, there are 17 and 19 outcomes used for placebo estimates in columns 3 and 4, respectively. To these outcomes we also add the take-up by head and spouse of employment programs exclusively for CS families (see Figure B.7 in the Appendix); we also re-estimate all outcomes in columns 7 and 8 for the sample of household that (1) live in urban areas, (2) live in rural areas, (3) fulfilled the condition before 2002 and (4) did not fulfilled the condition before 2002. Thus, in columns 3 and 4 we test $17+5\times 21=122$ hypotheses. We did the same for the outcomes in columns 7 and 8, where we test $17+5\times 19=112$ hypotheses, as take-up by head and spouse of employment programs exclusively for CS families are not used as outcome for four years of potential exposure.

[18] CAS stands for "Comites de Asistencia Social" – Social Welfare Committees.

wished to apply for social benefits. It collected information on housing conditions (e.g., material used for the construction of the house, access to water, sanitary services); socio-economic characteristics of household members (occupation, educational level, date of birth, and income); and ownership of assets and durables (housing property, heater and refrigerator). These correlates of long-term poverty were used to construct a score ranging from 350 to 770 points. Households with a CAS score below 500 were considered extremely poor, while those with a score between 500 and 540 were considered poor. The CAS score was valid for 2 years and it was used until 2007 to determine eligibility for monetary transfers (pension assistance for old age, i.e., PASIS, and family allowance, i.e., SUF), water subsidies (SAP), access to social housing and child care centres (Larranaga, 2005).

Using the Chilean national ID, we are able to merge the CAS and the FPS with the register of families participating in CS and with the register of all individuals participating in social promotion and training programmes between 2004 and 2007.

Since we use a regression discontinuity design, our main sample is restricted to families around the eligibility cutoff to CS in at least one year between 2002 and 2006. Thus, our final sample uses families who were 20 CAS points at most from the eligibility cutoff, which includes 265,987 families.

4.1 The CAS score

The CAS score is a weighted average of 13 variables. It is standardised to have a mean of 500 and a standard deviation of 100 points. Figure B.1 in the Appendix presents the distribution of the CAS score for each year between 2000 and 2006. These figures show a smooth distribution for the score in each year, which suggests that families are unable to manipulate their score.[19] This contrasts with the distributions for a similar poverty index available in Colombia (Camacho and Conover, 2011).

The Chilean score has some features that make a detailed manipulation by families and interviewers nearly impossible.[20] First, although information about the components of CAS score is

[19]The distribution of the poverty score looks similar across the 13 regions in Chile. These results are available from the authors.

[20]To understand the targeting ability of Ficha CAS reaching the eligible but excluding the ineligible, take the example of SUF, which is one of the outcomes in our paper. As mentioned above, SUF targets families below the fourth decile of the national income distribution. Castaneda et al. (2005) show that 90% of those taking up SUF in 2000 are indeed among the poorest 40% of the population in Chile, 7% fall in the middle quintile of the income distribution and just 3% belong to the top 40% of the income distribution. This performance in terms of targeting (i.e., high take-up among the poorest and very low or no take-up among the richest) is comparable to the targeting of the Temporary Assistance for Needy Families and Food Stamps in the US where the participation among the top 60% of the income distribution was 17% and 8%, respectively. The Colombian poverty index is associated with the highest leakage rate for ineligible participating in subsidised health insurance for the poor. The other two poverty indices analysed in Castaneda et al. (2005), the Brazilian Cadastro Único and the Mexican registry for the conditional cash

public, the exact algorithm for its computation has not been released publicly (Larranaga, 2005), and the weights and variables in the Ficha CAS have been evaluated and updated repeatedly (Castaneda et al., 2005). Second, the resulting score is a continuous variable which can take values with up to three decimal points. Third, the surveys are revised by reviewers to detect inconsistencies, and at least 20% of the surveys collected with some missing or inconsistent information are required to be repeated (Larranaga, 2005; Castaneda et al., 2005). Finally, the effective cutoffs are unknown, which rules out strategic underreporting by families to gain eligibility to the programme. We formally show in section 5.2 that there is no bunching of families just below the cutoff for each municipality.

4.2 Descriptive Statistics

Table 1 includes some descriptive statistics for the sample of families used in our main analysis: i.e., families who are 20 CAS points at most from the eligibility cutoff for at least one year between 2002 and 2006. The outcomes and socio-economic characteristics shown in Table 1 correspond to the years before CS was implemented.

The families in our sample are very poor, and this is reflected across many dimensions of their lives, as reported in this table. About 52% of the families receive some subsidy (48% receive the family child allowance for poor families, SUF). Only 48% of the families have a legal housing situation (i.e., they own, rent or are authorized to occupy their place of residence). Their houses are unlikely to have adequate ceiling and walls to protect their inhabitants from weather conditions (only 31% and 43% have adequate walls and ceilings, respectively). Just 69% of the families have water provided by the public network, and only 37% have a sewage connection. The mean CAS score for families in our sample was 490 before 2002 (the national mean was 542; families with a score below 500 are deemed extremely poor).

Families in our sample 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 national average (63% in our sample vs. 39% at national level; the alternative to self-employment is wage work). Spouses (of the head) in our sample are also less likely to be employed than the national average (13% vs. 22%). The heads of families in our sample are 50 years old on average (standard deviation 13.4) and have at most five years of schooling. Finally, 37% of the families in our sample live in rural areas compared to the national average of 20%.

transfer *Oportunidades* also produced higher leakages among the top 60% of the income distribution. In *Bolsa Escola* (Brazil) and in *Oportunidades* (Mexico) 35% and 21%, respectively, of those in the programmes belong to the richest 60% of the population. Therefore, although there is some fraud in the reporting of information in Ficha CAS, it is low and likely more comparable to misreporting in attempt to collect benefits in the USA, than in other Latin American countries.

5 Results

5.1 Eligibility and Participation in CS

We start by presenting non-parametric plots to show how the average participation rate in CS varies with CAS around the eligibility cutoffs, for each year of potential entry into the programme between 2002 and 2006. 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.

The dots in the figures correspond to cell means for participation in CS, after we divide the sample around the cutoff into bins of size 1. We consider only families with CAS scores within 20 points of each cutoff point, which means that there are 20 bins on either side of the cutoff. The lines in each figure are local linear regression estimates of an indicator of participation in CS on the distance to the effective cutoff, run separately for eligible ($CAS_{ijmt-k} - \overline{CAS}_{mt-k} \leq 0$) and ineligible ($CAS_{ijmt-k} - \overline{CAS}_{mt-k} > 0$) families. In each year, there is a clear discontinuity in participation in CS around the (normalised) effective cutoff.[21]

Table 2 complements these figures by showing estimates of the first stage equation (4), where the outcome variable is an indicator for CS participation at time $t-k$, 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; therefore, the variation we use is within municipality and year. For each cohort (2002-2006) 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 column 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 estimated *effective* eligibility cutoff are statistically significant, between 12-13.3%. The discontinuities around the *official* cutoff are statistically significant, but much smaller in magnitude (see Figure B.4 for correspondent nonparametric estimates).

To understand how the gradual shift in cutoffs over time allows to identify the effect of entry into CS in different years, we also redid the graphs in Figure 2 using an indicator which takes the value of 1 if a household ever enrolled in CS up to that year as a dependent variable, and zero otherwise. This variable captures cumulative participation into the program. The most important difference between Figures 2 and 3 is the following. For each cohort in figure 2, we exclude all families who have enrolled in CS in the past since they cannot re-enrol in the programme again as

[21] Since we showed in section 3 that not all municipalities in Chile contributed to the identification of the effects of CS, Figure B.3 in Appendix B presents estimates for ρ from the first stage equation $CS_{imt-k} = \tau + \rho E_{imt-k} + f(CAS_{imt-k} - \overline{CAS}_{mt-k}) + u_{imt-k}$ estimated separately for each municipality. For simplicity, all cohorts (2002-2006) are pooled together. The size of the first stage is at least 0.1 for about 3/4 of the municipalities.

new participants, while for Figure 3 we keep all families in the sample, regardless of whether they participated in CS in the past or not.

Again, we estimate local linear regressions of this indicator of cumulative participation in CS on the distance to the effective cutoff, separately for near-eligible and near-ineligible families. Each graph in the figure has five lines corresponding to the cumulative participation indicators for each year. In each of the five graphs, we use a different set of effective cutoffs, corresponding to the effective cutoffs in each year from 2002 to 2006.

There is a discontinuity of 12-13% in the probability of entry in CS up to year t around the effective cutoff of year t. For example, take the graph where the running variable is the distance to cutoff in 2004. There is a clear discontinuity at this cutoff for the probability of entry in 2004. As expected, there is no discontinuity around that cutoff for those who entered before 2004, because participation for those enrolling in CS before 2004 should not be affected by the 2004 cutoff. Notice also that eligibility in year t has the same effect on CS participation in each subsequent year $t+k, k \geq 1$. This follows from the fact that the discontinuities at the year t cutoffs affect all current and future cohorts equally. This is because there are subsequent entrants, but their entry is not affected by the year t cutoff. This explains why the discontinuity around the 2004 cutoff in the probability of entry up to 2005 and 2006 is similar to that of entry up to 2004.

5.2 Intent-to-Treat Estimates of Programme Impacts

We start by presenting intent-to-treat (ITT) estimates for three groups of outcomes for which we have information: the take-up of subsidies and of employment programmes,[22] labour market outcomes, and housing conditions. In the main text of our paper we focus on programme impacts measured two and four years after a family first enrolled in the programme. In the Appendix we also present impacts measured six years after they first enrolled in the programme which can only be calculated for families that (potentially) started the programme in 2002 or 2003. Table A.4 in Appendix A includes the definition of each variable used.

Figure 4 shows estimates of the relationship between outcomes and the distance to the municipality and cohort-specific cutoffs. The vertical line shows the point where this distance is equal to zero. Outcomes are measured two years after potential programme enrolment (Figure B.5 in the 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 group individuals into bins of

[22] We use information about two types of employment programmes offered by FOSIS: (1) programmes to which CS families had preferential access, and (2) and programmes to be attended exclusively by individuals in CS families. In the main text we present the impacts for the first type of programmes, but we also present estimates on the impacts on the second type of employment programmes (which aim exclusively at individuals in families during the years of CS home visits) in the Appendix.

size 1, according to their CAS scores. The lines in the figures are local linear regression estimates of the outcomes on the distance to the effective cutoff, with their 95% confidence intervals.[23]

Figure 4 shows that there is higher take-up of SUF (child allowance) and SAP (water subsidy) just below than just above the effective eligibility cutoff. This difference for SUF is statistically significant in the parametric regression models we present below. The probabilities of participation in the employment programmes by the head and spouse also increase at the cutoff (in Figure B.7 in Appendix we also present the estimated impact on the take-up by head and spouse of employment programs exclusively for CS families). We do not find any statistically significant differences between households on either side of the cutoffs when we look at the probability of legal ownership of the house, access to the public water supply network, and the employment status of head and spouse.

Table 3 summarises our parametric ITT results. The first column of Table 3 shows the control-mean, i.e., the mean of the variable considered for the sample of just ineligible families (with CAS score at most 4 points above the cutoff).[24] Columns (4) and (8) in this table show estimates of γ in equation 4 measured two and four years after potential enrolment, respectively. The strongest impacts of CS are on the take-up of SUF and employment programmes for the spouse. Although there are positive impact estimates in several other outcomes, they are not statistically different from zero once we adjust for multiple hypotheses testing. The impact on the take-up of SUF and employment programmes for the spouse persist up to 4 years after entry in CS. Although the point estimates in Table 3 and in Figure 4 are similar, the standard error bands are larger in the figures than in the tables. This is because the tables show estimates using parametric rather than non-parametric specifications, which greatly reduce the number of estimated parameters in the model, and the resulting standard errors, without producing noticeable changes in the estimated parameters of interest.

It is reasonable that the strongest impacts are on the take-up of subsidies and social services during the first two years when home visits are in place. During these visits, the social worker should provide information about the services and subsidies each family is entitled to, how they can benefit the family and simultaneously help them to register for these programmes.

To assess the balance in observable characteristics of individuals on each side of the cutoff we estimate equation (4) using pre-determined characteristics that should not be affected by the programme as dependent variables (Lee and Lemieux (2010)). Columns (3) and (7) of Table 3

[23] We use a bandwidth equal to 8. The results are robust to using bandwidths equal to 6 and 10 (see Figure B.6 in Appendix B).

[24] Our sample size became smaller when we estimated longer-term impact estimates because we only have data up to 2009. However, our results for the longer-run impacts were not driven by sample selection because the 2-year effects are similar if we restrict the sample to those families to whom we can estimate both 2- and 4-year impacts (results available from the authors). Table A.5 in Appendix A is a version of Table 3, including the t-statistic unadjusted for multiple hypotheses testing.

(labelled "placebo") show the results when each of the variables are measured prior to the implementation of CS in 2000 (or 2001 if the family has no information for 2000), and, therefore, they are pre-determined. There is no statistically significant estimate in these columns after accounting for multiple hypotheses testing, which suggests that individuals located just below and just above each cutoff are similar in terms of their observable pre-determined characteristics.[25] Finally, Figure B.9 in Appendix B shows that the density of the running variable at the cutoff is continuous (McCrary, 2008); therefore, there is no evidence of manipulation of CAS scores around the cutoff. Thus, our empirical strategy is likely to be valid.

One last important thing to report from this table, which is again consistent with the graphical evidence, is that there are no impacts of the programme on employment and housing outcomes, either in the short or in the medium term. Although the programme was able to link some additional individuals to the social welfare network, the programme had no real impact on any of the measures of household welfare and economic opportunities observed in our data. Perhaps there was an impact on non-durable consumption items, such as food, or on savings, since we expect participating households to have higher income because they can access SUF. Unfortunately, we are unable to observe these variables in our data.

5.3 Instrumental Variables

Table 4 shows the IV estimates corresponding to the ITT estimates in Table 3, for short- (column 1) and medium-term (column 2) effects of participation in the programme. We present estimates only for those outcomes, SUF and take-up of employment programmes, where in Table 3 we reject the null of no effect, and employment status. Our IV estimates for the remaining outcomes in the table (available on request), are not statistically different from zero.

The take-up of SUF by families just above the cutoff is 52.7%. CS participation increases the probability that a family takes-up SUF by 11% and 17% two and four years after enrolment in CS, respectively.

The mean participation in employment programmes among near-eligible households is very low (below 2%), both for the household head and for the spouse. Relative to these values, the magnitudes of short-run programme impacts on the take-up of these programmes are substantial (2.3% and 3.8% for the head and the spouse, respectively). Note that the take-up of employment programmes is a lower bound estimate of the impact of the programme because participation in programmes that exclusively target individuals in CS families is excluded from the definition and

[25]The different panels of Figure B.8 in Appendix B 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 programme 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 programmes from FOSIS prior to 2002, which is not available before 2004.

the supply of these programmes increased over time since 2004. Medium-term impacts are smaller and statistically insignificant for take-up of employment programmes by heads, possibly because the earlier cohorts were not exposed to the expansion of the supply side of programmes.

5.4 Differential Impacts Across Groups

We analyse two dimensions of heterogeneity to further understand the mechanisms underlying this pattern of programme impacts. First, we examine whether there are differential impacts for households who are disconnected from the welfare system or had not met the minimum conditions before 2002. This is interesting because the main impact of the programme seems to be linking poor families to the social welfare system. If that is really the case, we would not expect any impacts on families who were already collecting subsidies and participating in employment programs before CS was implemented. These families had already been informed about the types of benefits they can apply for and already incurred the initial costs required to get them, including any social stigma. If the impacts of CS were similar for those with and without prior participation in the welfare system, it is unlikely that CS was operating either by providing new information, or even by reducing stigma.

Second, we examine whether there are differential impacts across cohorts by comparing the effects for those who (potentially) enter CS before vs. after the approval of the law that regulates the programme in 2004 and expansion of the programmes made available to CS participants.[26] This is interesting because the supply of social services available for CS participants greatly expanded over time. In the early stages of CS, even though households were encouraged to register for a large range of social programmes, they were in short supply which meant that CS participants did not get to access them. Therefore, later CS families benefited from a much more generous programme than earlier participants. Again, if CS operated primarily by fostering the participation of CS families in the welfare system, we expect larger programme impacts for later cohorts of entrants. Of course, it is also possible that the programme was just better implemented for later cohorts as CS workers at various levels learned from the early years of implementation.

[26]We also studied two other sources of heterogeneity, but we opted to leave them out of the paper, since they are more speculative. One could argue that the availability of social services varies with the degree of remoteness of the local of residence. For example, families would incur in a higher transportation cost to access services. Thus, when we allow the effects to vary by urban and rural areas, we cannot reject the null of similar effects across areas in the take-up of subsidies (SUF), employment programmes and labour market outcomes. We also allow the effect to vary by whether families live in municipalities where social workers are allocated more or less families (in particular, municipalities where on average social workers are allocated more or less than 50 families). Such sample split can be problematic because municipalities that allocated more families to each social worker might also be those municipalities with more experienced and able social workers. Also, a higher caseload for social workers correlates strongly with geographic remoteness; therefore, a disaggregation by caseload is similar to a disaggregation by urban and rural areas. When we try such sample split we fail to detect any correlation between the effects we find and the social workers' caseload.

Subsidies The first dimension of heterogeneity looks at the impacts of CS on SUF separately for families who took-up SUF in either 2000 or 2001 (pre-2002) and for those who did not. Columns (1) and (2) of Table 5 examine the impact of CS on the take-up of SUF for the two groups of families described above and it presents estimates from equations (4) and (5), respectively. The table includes two panels, each with short- (column 1) and medium-term (column 2) estimates: panel A presents estimates for those not receiving SUF before 2002 and panel B includes estimates for families who received it before 2002. Columns (1) and (2) show that the programme has substantial short- and medium-term impacts on the take-up of SUF for those without prior take-up of this subsidy and no impact on families who have taken up this subsidy before. The difference between the two groups is statistically significant: the p-value for the null hypothesis that the effect for those without SUF prior 2002 is equal to the effect for those that did not receive SUF prior to 2002 (at the bottom of the table) is 0.08 and 0.04 for 2 and 4 years of exposure, respectively. The take-up of SUF increases for the former group of families by 17% to 28% 2 and 4 years after programme enrolment, respectively.[27]

In columns (3) and (4) of Table 5 we also study whether there are any differences in impacts between cohorts entering the programme before and after 2004. We do so by splitting the sample by cohorts that were differentially exposed to the increase in the supply of social services: the 2002-2004 cohorts (column 3) and the 2005-2006 cohorts (column 4). Columns (3) and (4) of Table 5 present estimates of the effect of the programme two years into CS, by the (potential) year of entry.[28] Panel A for the sample of families not receiving SUF before 2002 shows a large impact on those families who have not taken up SUF before starting CS in 2005-2006. We reject the null of equality of the estimates of later vs. earlier cohorts against the alternative hypothesis of a larger effect for the later cohort with a p-value of 0.04.

Panel B shows no detectable impact on take-up of SUF for families who had already received the programme before 2002 and who belonged to the later cohort, but a statistically significant impact for those in the early cohort, which is unexpected (although we cannot reject that the estimates for the early and late cohorts are statistically equal). The estimates for the early cohort (column 3) are similar both for individuals who have taken-up SUF before and for those who have not: the p-value for the null hypothesis that the effect for those without SUF before 2002 is equal to the effect for those that received SUF prior to 2002 is 0.46. This is not the case for the later cohort (column 4), where the corresponding p-value is 0.04.

[27]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 (see Table A.6). We find a small negative effect of CS on participation in SAP for those families that received it before 2002. This is a crowd out effect to which we cannot attribute a conclusive cause, but a strong possibility is measurement error in the determination of eligibility to SAP in the administrative records.

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

Since the cutoffs that determine eligibility to CS vary across year and municipalities, in Figure 5 we allow the impacts to vary by cutoff score (see Filmer and Schady (2011) and Pop-Eleches and Urquiola (2013)). The estimates in Panel A show that the impacts are driven by families living in municipalities with higher cutoffs, highlighting the difficulty of improving the lot of the very poorest households. One problem is that we cannot say whether this is due to a family effect, or due to a municipality effect, since municipalities with higher cutoffs are also richer municipalities to begin with.

Employment Programmes We next re-examine the impacts of CS on the take-up of employment programmes. There is no information about the take-up of these programmes before 2002 and, therefore, we cannot perform an analysis similar to the one presented for SUF. However, one important question is whether CS induces a larger take-up for the population who needed them the most, i.e., for individuals who were not employed before 2002. To study this, we divide individuals in the sample into two groups: for the unemployed in either 2000 or 2001 (Panel A of Table 6) and for the employed (Panel B). Within each family we consider only heads and their spouses.

Table 6 has two sets of two columns: one set for heads (columns 1 and 2), and one set for spouses (columns 3 and 4). Panel A shows that CS has large (relatively to the average value in the ineligible sample) and statistically strong impacts on the take-up of employment programs for individuals who were not employed before 2002. This is true for both heads and spouses, but mainly in the short-term (columns 1 and 3). Panel B shows no statistically significant effects on the participation in training programmes among heads or spouses employed before 2002. Table A.7 presents the estimates by cohort according to individuals' pre-programme employment status. There are no statistically significant differences across cohorts.

As for SUF, Panel B in Figure 5 shows the impact on participation in employment by cutoff score of the municipality. Also here, the impacts are driven by families living in high cutoff municipalities.

Labour Market Outcomes The short- and medium-term programme impacts on the employment of the head and spouse by cohort of entry are presented in Table 7. The results are presented for the overall sample (panel A), as well for not employed (panel B) and employed (panel C) before 2002. We only find statistically significant short-term impacts (after accounting for multiple hypotheses testing) on employment of spouses (who are females in 98% of the cases), who were not employed before 2002, and who (potentially) entered the programme after 2004 (column 6 in Panel B). We reject the null that the effects on earlier and later cohorts are the same (p-value is

0.00).[29] This suggests that perhaps CS is able to promote employment among a particular group of married women, who were unemployed at the onset of the programme. With an average participation of 30%, female labour force participation in Chile is low by Latin American standards, especially among the poorest sections of the population. In this context, CS coupled with the support of effective employment programmes could constitute an important avenue to improve female labour market participation. The expansion of access to child care might have enhanced these improvements, although there are no reasons to expect that the access to public day care centres exhibits a discontinuity at the effective cutoffs we estimate (Medrano, 2009).

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

We study seven variables related to housing conditions (collected from the CAS): i.e., connections to the water and sewage networks, ownership of the house and the quality of the walls and ceilings (see Table A.9 in Appendix A). We also analyse nine variables (collected from the FPS 2007-2009) concerning school enrolment of children, health coverage of children and elderly in the household, and enrolment in employment centres. We are unable to detect impacts of the programme in any variable across the various subgroups we consider (see Table A.10 in Appendix A).

5.5 Sensitivity Analysis

In this last section we assess the robustness of the results through a battery of tests. For parsimony, we focus on ITT estimates measured two years after (potential) programme entry.

First, panel A of Table 8 shows robustness to the choice of functional form of the running variable. Column 1 includes ITT estimates for our baseline specification and in columns 2-6, we present estimates for five alternative specifications. In columns 2 and 3, we include estimates for a variation of the baseline model, but where the function of distance to cutoff is linear (column 2) and cubic (column 3). In columns 4 and 5, the polynomial of the distance to cutoff is quadratic and cubic, respectively, and the function is the same on either side of the cutoff. Finally, in column 6, we present estimates using the quadratic of CAS as running variable. There are hardly any changes in our main results. Panel B of Table 8 shows the sensitivity of our results to the use of different windows of data around the discontinuity. We use six possible windows of data around the cutoff:

[29] Spouses not working before 2002 enter the labour market as wage workers. This result is presented in Table A.8 in Appendix A, where we study whether CS affects the probabilities of being not employed, self-employed, or wage employed, conditional on the employment status prior to 2002.

15 points, 20 points (our baseline sample), 25 points, 30 point and 50 points. Our results are robust to the choice of window of data around the discontinuity points.

Second, we estimated models using different sets of fixed effects for the place of residence at potential entry (see Table A.11 in Appendix A). Besides the municipality-year effects, we also included municipality and year fixed effects, neighbourhood (the geographic unit immediately below municipality) and year fixed effects, and, finally, neighbourhood-year effects. We find similar results regardless of whether we control for interacted 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 neighbourhood fixed effects.

Third, 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-programme data for balancing checks, and to study heterogeneous effects by pre-programme conditions. Our results do not change substantially if we relax this restriction (see Table A.12 in Appendix A).

Fourth, we estimate the effects separately for the sample of families living in municipalities where the size of the estimate of ψ in equation (2) is high and for those where it is low. Our results are driven by the first set of municipalities (see Table A.13).[30]

Fifth, Table A.14 in Appendix A shows robustness to an alternative method to obtain the eligibility cutoff, in particular, we use a split-sample approach as suggested by Card, Mas and Rothstein (2008).[31] That is, we re-estimated the *effective cutoff* using a random sample of families in each municipality-year. The *effective cutoff* for eligibility for each year between 2002 and 2006 in each municipality is estimated as in equation (2). Instead of using all families in a given year and municipality with a valid CAS, a random sample of 2/3 of families is used. For each municipality-year, the cutoff is similar to that obtained using the whole sample (the results are available from the authors). Then, we estimate models (4) and (5) using these cutoffs, but restricting the sample to those families not used to estimate the municipality-year cutoff relevant to assess the impacts of CS.

Finally, in Appendix D 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

[30] We consider that a municipality has discontinuity in the estimation of equation (2) if the estimate for ψ at the CAS score value that maximises the R^2 of (2) is at least 0.1. Figure B.10 in Appendix B shows that the CAS values that determine the eligibility to CS in municipalities with a high and low discontinuity estimation of equation (2) are similar. However, the mass of families in high discontinuity municipalities is to the left to the mass of families in municipalities with low discontinuity.

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

CS in a given year may receive it in subsequent years. The gradual rollout of CS means that subsequent programme entry by ineligibles can lead to underestimate programme impacts since we assume that the initial group of ineligible families did not receive any additional subsequent treatment beyond what we observe in the first year (defining the cohort). On the other end, if there are additional entries by eligible families and if this is not accounted for, our estimates may be too large relative to the true programme impacts. The estimates presented in Table D.1 are not substantially different from our main results. Intuitively, the reason why the results are similar with and without dynamics is that, taking a cutoff defined in year t, the amount of new entrants each subsequent year is about the same on each side of the original cutoff.

6 Conclusion

CS is a programme that attempts to help the extreme poor. Given the challenge to help such a difficult population to work with, CS was designed to be much more than a cash transfer program. Households benefit from regular home visits by trained social workers for a period of two years. These social workers provide information about social services available to poor households, and design personalised planning strategies to help household reorienting their lives. Participating households are guaranteed access to monetary transfers to provide income support in the short run. On the supply side, there is an effort to make available to the poor all the social programmes that can help sustain their exit from poverty in the medium run.

Our results show, however, that the program did not generate significant improvements in the labour market participation of household members, nor in their housing conditions, two important indicators of wellbeing. This is not because of lack of power or low quality data. We have limited but relatively good data, and we have a large sample.

There was, however, one important dimension in which CS had an impact. It led to an increase on the the take-up of a family allowance for poor families with children (the SUF, *Subsidio Único Familiar*) that were eligible to the programme but which were not taking it up, and to an increase in participation in labour market programmes by individuals not working before the introduction of CS. This was indeed a central goal of CS, which prioritised the establishment of stronger links between those who are in extreme poverty and the social welfare system that is designed to serve them. CS personalised counseling helped activating the demand for monetary subsidies and social services among families who were alienated from the system to start with. Unfortunately, connecting households with social services was not enough to produce significant gains in their wellbeing.

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7 Tables

Table 1: Baseline Characteristics of Families in the Sample.

Variable	(1) N	(2) Mean	(3) S.D.
Any subsidy	265,987	0.52	0.50
SAP	265,856	0.15	0.36
SUF	195,570	0.48	0.50
Housing			
Legal occupation of house	265,986	0.48	0.50
Owner of house (condition on legal occupation of house)	65,147	0.85	0.36
Adequate walls	265,987	0.31	0.46
Adequate roof	265,987	0.43	0.49
Water from public network	265,987	0.71	0.45
Sewage connected	265,987	0.37	0.48
CAS	265,986	489.93	37.27
Labor Market and Income			
Employed (head)	265,987	0.75	0.43
Self-employed (head)	265,987	0.63	0.48
Dependent worker (head)	265,987	0.18	0.38
Employed (spouse)	196,497	0.13	0.33
Self-employed (spouse)	196,497	0.11	0.31
Dependent worker (spouse)	196,497	0.05	0.22
Monthly Income per capita	265,987	25299.18	23841.07
Demographics			
Age of head	265,987	50.18	13.36
Single headed	265,987	0.32	0.47
Years of Schooling of Head	153,237	5.14	3.17
Presence of children	265,987	0.79	0.41
Rural	265,987	0.37	0.48

Note: The table includes the mean and standard deviation for selected variables for the families in the sample used in the main analysis (ie, at most 20-CAS points from the cutoff). There is one observation per family in the table which is measured prior to the introduction of CS in 2002. Characteristics of families are measured in 2000 or 2001. For families with information in both years, we take the mean value whenever a variable is continuous (CAS, monthly per capita income, age and schooling of head); all other variables in the table are indicator variables (which value 1 if the condition if fulfilled and 0 otherwise) and for these we consider the maximum value in either 2000 or 2001.

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]	0.122***	0.020***	0.133***	0.054***	0.125***	0.073***	0.129***	0.077***	0.120***	0.075***
	(0.008)	(0.003)	(0.007)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.005)	(0.006)
Observations	235,764	331,191	294,549	337,334	319,820	345,232	333,342	349,897	273,465	283,185
Mean	0.0540	0.0271	0.0577	0.0422	0.0495	0.0433	0.0497	0.0512	0.0392	0.0431
SD	0.226	0.162	0.233	0.201	0.217	0.203	0.217	0.220	0.194	0.203
F-Test on Eligibility										
F	233.9	35.22	419.8	70.10	436.8	126.7	369.5	113.3	567.5	169.6

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 fixed effects. 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. * significant at 10%; ** significant at 5%; *** significant at 1%.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Years after start			**2**					**4**	
	C. Mean	N	Placebo	ITT	Sample	N	Placebo	ITT	Sample
Participation									
SUF	0.527	169,965	-0.008	0.023**	2004-2008	105,422	-0.003	0.031***	2006-2008
			(0.008)	(0.007)			(0.009)	(0.009)	
SAP	0.163	138,658	0.012	0.003	2004-2006	16,421	0.019	-0.008	2006
			(0.005)	(0.006)			(0.015)	(0.014)	
FOSIS participation (head)	0.011	212,228		0.004**	2004-2007	103,581		-0.001	2006-2007
				(0.002)				(0.001)	
FOSIS participation (spouse)	0.019	142,233		0.007**	2004-2007	69,545		0.006*	2006-2007
				(0.003)				(0.002)	
Labour market									
Employed (head)	0.715	234,915	0.003	0.000	2004-2008	197,443	0.004	-0.004	2006-2009
			(0.005)	(0.006)			(0.006)	(0.007)	
Self-employed (head)	0.518	234,915	0.003	0.000	2004-2008	197,443	0.004	-0.003	2006-2009
			(0.005)	(0.006)			(0.006)	(0.007)	
Dependent worker (head)	0.198	234,915	0.003	0.000	2004-2008	197,443	0.004	-0.001	2006-2009
			(0.005)	(0.005)			(0.006)	(0.006)	
Formal Worker (head)	0.193	85,513		-0.006	2007-2008	170,326		-0.010	2007-2009
				(0.009)				(0.006)	
Employed (spouse)	0.167	137,789	-0.009	0.006	2004-2008	85,970	-0.004	-0.009	2006-2009
			(0.006)	(0.006)			(0.008)	(0.009)	
Self-employed (spouse)	0.116	137,789	-0.009	-0.001	2004-2008	85,970	-0.004	-0.003	2006-2009
			(0.006)	(0.005)			(0.008)	(0.008)	
Dependent worker (spouse)	0.051	137,789	-0.009	0.007	2004-2008	85,970	-0.004	-0.006	2006-2009
			(0.006)	(0.003)			(0.008)	(0.006)	
Formal Worker (spouse)	0.0787	32,345		0.006	2007-2008	67,129		-0.003	2007-2009
				(0.009)				(0.006)	
Housing									
Legal occupation of house	0.513	241,297	0.005	-0.002	2004-2008	209,152	0.008	-0.000	2006-2009
			(0.007)	(0.006)			(0.007)	(0.007)	
Sewage connected	0.362	149,405	0.002	0.008	2004-2006	27,117	0.003	0.008	2006
			(0.006)	(0.006)			(0.013)	(0.015)	
Water from public network	0.676	241,297	0.005	0.002	2004-2008	209,153	0.007	0.006	2006-2009
			(0.004)	(0.004)			(0.005)	(0.005)	
Adequate roof	0.410	149,405	0.015	-0.002	2004-2006	27,117	-0.001	-0.010	2006
			(0.007)	(0.007)			(0.005)	(0.010)	
Adequate walls	0.284	149,405	0.017	-0.001	2004-2006	27,117	-0.000	0.008	2006
			(0.007)	(0.006)			(0.015)	(0.021)	
Heating	0.067	149,405	-0.001	0.000	2004-2006	27,117	-0.015	0.015	2006
			(0.003)	(0.004)			(0.014)	(0.018)	
Fridge	0.456	149,405	0.009	0.008	2004-2006	27,117	-0.030*	-0.001	2006
			(0.006)	(0.008)			(0.017)	(0.020)	

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

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

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

	(1)	(2)
Years after start	2	4
Participation		
SUF	0.110***	0.172***
	(0.035)	(0.050)
Labor market programs - FOSIS (head)	0.023*	-0.003
	(0.009)	(0.009)
Labor market programs - FOSIS (spouse)	0.038**	0.033**
	(0.013)	(0.014)
Labor market		
Employed (head)	0.001	-0.022
	(0.031)	(0.039)
Not employed (spouse)	0.032	-0.047
	(0.030)	(0.045)

Note: The table presents the estimated coefficients (and standard errors) for the indicator of entry in CS 2 or 4 years before the time at which outcome is measured in model 5. Controls excluded from table include quadratic in distance to cutoff, their interaction with the CS indicator and municipality-year effects. The municipality of residence and distance to cutoff are measured when eligibility is evaluated.
Robust standard errors are reported in parenthesis clustered at municipality of residence when eligibility is evaluated. * significant at 10%; ** significant at 5%; *** significant at 1% (the critical values for inference are adjusted for multiple hypotheses).

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

	(1)	(2)	(3)	(4)
Years after start	2	4	2	
Cohort			2002-2004	2005-2006
	Panel A: Not receiving SUF before 2002			
Eligibility (ITT)	0.031*	0.045***	0.013	0.056***
	(0.011)	(0.013)	(0.013)	(0.017)
Participation (IV)	0.172*	0.281***	0.083	0.266***
	(0.059)	(0.076)	(0.072)	(0.078)
Control Mean	0.381	0.393	0.376	0.387
Observations	83,838	50,861	49,692	34,146
P-Value: HA: $\beta^{iv}_{0506} > \beta^{iv}_{0204}$			0.040	
	Panel B: Receiving SUF before 2002			
Eligibility (ITT)	0.018	0.021	0.018	0.019
	(0.009)	(0.013)	(0.011)	(0.016)
Participation (IV)	0.079	0.107	0.093*	0.069
	(0.038)	(0.065)	(0.050)	(0.055)
Control Mean	0.662	0.619	0.682	0.625
Observations	86,127	54,561	55,640	30,487
P-Value: HA: $\beta^{iv}_{0506} > \beta^{iv}_{0204}$			0.380	
P-Value: HA: $\beta^{iv}_{0} > \beta^{iv}_{1}$	0.080	0.040	0.460	0.040

Note: See table 3 for a description of the specification used in rows (ITT) and see table 4 for the description of specification used in rows (IV). The coefficient estimate in rows (ITT) is the indicator of eligibility, E_{im}, and that in rows (IV) is the indicator of participation in CS, CS_{im}.
The p-values in panels A and B concern the null hypothesis that the effect for those that entered in CS in 2005-2006 equals the effect of those that entered in 2002-2004, 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 p-value in the last row tests the null hypothesis of the effect for those without SUF prior 2002 equals the effect on those receiving SUF, 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% (critical values for inference are adjusted for multiple hypotheses testing).

Table 6: Participation in labour 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)	0.012***	-0.000	0.010***	0.005
	(0.003)	(0.003)	(0.003)	(0.003)
Participation (IV)	0.071***	-0.002	0.054***	0.027
	(0.020)	(0.018)	(0.014)	(0.015)
Control Mean	0.011	0.009	0.018	0.010
Observations	45,134	19,632	123,314	60,788
	Panel B: Employed before 2002			
Eligibility (ITT)	0.002	-0.001	-0.014	0.010
	(0.002)	(0.002)	(0.008)	(0.007)
Participation (IV)	0.013	-0.005	-0.085	0.069
	(0.010)	(0.010)	(0.045)	(0.051)
Control Mean	0.011	0.006	0.027	0.007
Observations	167,094	83,949	18,919	8,757
P-Value: HA: $\beta_0^{iv} > \beta_1^{iv}$	0.000	0.000	0.360	0.780

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

The last row presents the p-value for the null hypothesis that the effect for those not employed prior 2002 equals the effect on those employed, 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 7: Labour 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)	0.002	-0.004	-0.008	0.006	0.002	0.016	-0.018	0.022
	(0.007)	(0.010)	(0.008)	(0.011)	(0.006)	(0.015)	(0.010)	(0.018)
Participation (2SLS)	0.014	-0.016	-0.050	0.030	0.015	0.068	-0.108	0.097
	(0.039)	(0.043)	(0.051)	(0.052)	(0.034)	(0.066)	(0.055)	(0.078)
Control Mean	0.722	0.704	0.735	0.706	0.133	0.274	0.262	0.276
P-Value: HA: $\beta^{iv}_{0506} > \beta^{iv}_{0204}$	0.800		0.840		0.200		0.660	
	Panel B: Not employed before 2002							
Eligibility (ITT)	-0.005	-0.025	-0.022	-0.016	0.001	0.033*	-0.015	0.033
	(0.014)	(0.023)	(0.015)	(0.030)	(0.005)	(0.016)	(0.010)	(0.019)
Participation (2SLS)	-0.030	-0.130	-0.141	-0.076	0.008	0.143*	-0.086	0.141
	(0.085)	(0.118)	(0.095)	(0.141)	(0.032)	(0.070)	(0.057)	(0.080)
Control Mean	0.221	0.290	0.289	0.311	0.095	0.229	0.218	0.234
P-Value: HA: $\beta^{iv}_{0506} > \beta^{iv}_{0204}$	0.760		0.900		0.000		0.760	
	Panel C: Employed before 2002							
Eligibility (ITT)	0.002	0.003	-0.008	0.010	0.021	-0.048	-0.030	-0.035
	(0.006)	(0.010)	(0.007)	(0.011)	(0.026)	(0.038)	(0.031)	(0.050)
Participation (2SLS)	0.012	0.012	-0.049	0.044	0.137	-0.233	-0.198	-0.181
	(0.036)	(0.043)	(0.043)	(0.051)	(0.161)	(0.181)	(0.199)	(0.250)
Control Mean	0.859	0.794	0.828	0.786	0.380	0.478	0.490	0.474
P-Value: HA: $\beta^{iv}_{0506} > \beta^{iv}_{0204}$	0.540		0.680		0.940		0.720	
P-Value: HA: $\beta^{iv}_{0} > \beta^{iv}_{1}$	0.660	0.800	0.240	0.940	0.740	0.040	0.620	0.260

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

Table 8: ITT Estimates: robustness to functional form and trimming around cutoff.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Functional Form	Baseline	Distance		Distance: no interaction		CAS
		Linear	Cubic	Quadratic	Cubic	Quadratic
SUF	0.023**	0.019***	0.021**	0.022***	0.022***	0.020***
	(0.007)	(0.004)	(0.009)	(0.005)	(0.006)	0.00453
SUF- No SUF before 2002	0.031*	0.024***	0.033**	0.025***	0.028**	0.026***
	(0.011)	(0.007)	(0.013)	(0.007)	(0.009)	(0.007)
FOSIS participation (head)	0.004**	0.007***	0.004	0.008***	0.005***	0.008***
	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)
FOSIS participation (head)-not empl. before 2002	0.012***	0.012***	0.008	0.012***	0.013***	0.012***
	(0.003)	(0.002)	(0.005)	(0.003)	(0.003)	(0.002)
FOSIS participation (spouse)	0.007**	0.013***	0.005	0.013***	0.009***	0.014***
	(0.003)	(0.002)	(0.004)	(0.002)	(0.002)	(0.002)
FOSIS participation (spouse)-not empl. before 2002	0.010***	0.014***	0.008*	0.015***	0.011***	0.015***
	(0.003)	(0.002)	(0.004)	(0.002)	(0.002)	(0.002)
Panel B: Trimming around cutoff (CAS points)	15	20	25	30	50	
SUF	0.022**	0.023**	0.025***	0.025***	0.024***	
	(0.008)	(0.007)	(0.006)	(0.006)	(0.005)	
Observations	128,201	169,965	211,996	253,537	413,421	
SUF - no SUF before 2002	0.030**	0.031*	0.035***	0.029***	0.024***	
	(0.012)	(0.011)	(0.010)	(0.009)	(0.006)	
Observations	62,525	83,838	106,308	129,365	226,851	
FOSIS participation (head)	0.004	0.004**	0.006***	0.006***	0.007***	
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	
Observations	160,015	212,228	264,518	316,154	516,271	
FOSIS part. (head) - not empl. before 2002	0.009*	0.012***	0.013***	0.012***	0.012***	
	(0.004)	(0.003)	(0.003)	(0.003)	(0.002)	
Observations	33,686	45,134	56,640	68,510	117,245	
FOSIS participation (spouse)	0.005	0.007**	0.010***	0.011***	0.013***	
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	
Observations	107,466	142,233	177,090	211,250	343,384	
FOSIS part. (spouse) - not empl. before 2002	0.009***	0.010***	0.012***	0.012***	0.014***	
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	
Observations	93,435	123,314	153,201	182,373	293,336	

Note: Panel A of the table presents the coefficient estimates (and standard errors) on eligibility from model 4. Controls excluded from column (1) include quadratic in distance to cutoff, their interaction with eligibility to CS and municipality-year effects; controls excluded from column (2) and (3) include linear and cubic functions in distance to cutoff and its interaction with eligibility to CS, respectively, and municipality-year effects. Columns (4) and (5) have estimates with the same models as in columns (1) and (23), except that eligibility to CS is not interacted with distance to cutoff. The municipality is the municipality of residence when eligibility is evaluated. Column (6) presents the marginal effect on eligibility from estimating the following model

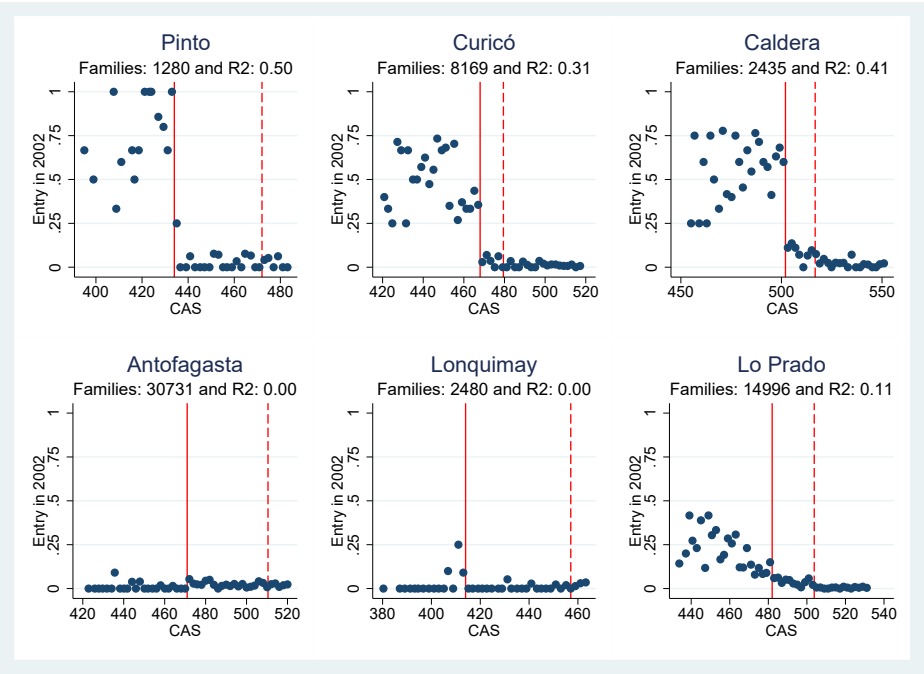
$$Y_{imk} = \phi + \gamma E_{im} + \zeta E_{im} CAS_{im} + \eta E_{im} CAS_{im}^2 + \rho CAS_{im} + \theta CAS_{im}^2 + u_{imk}.$$

Panel B presents the coefficient estimates (and standard errors) on eligibility from model 4 trimming the sample differently around the cutoff using our basic set of controls.

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).

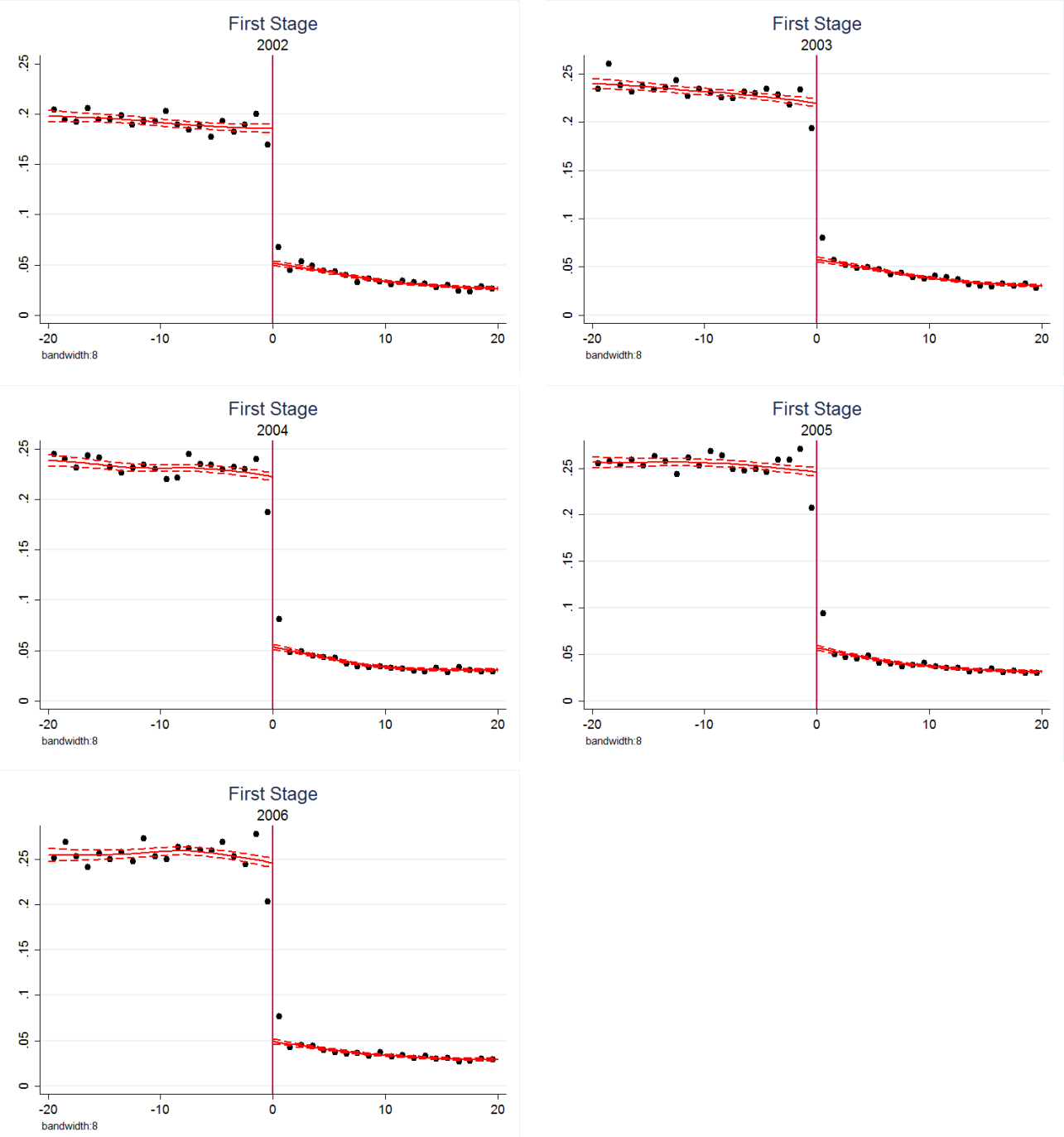
8 Figures

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



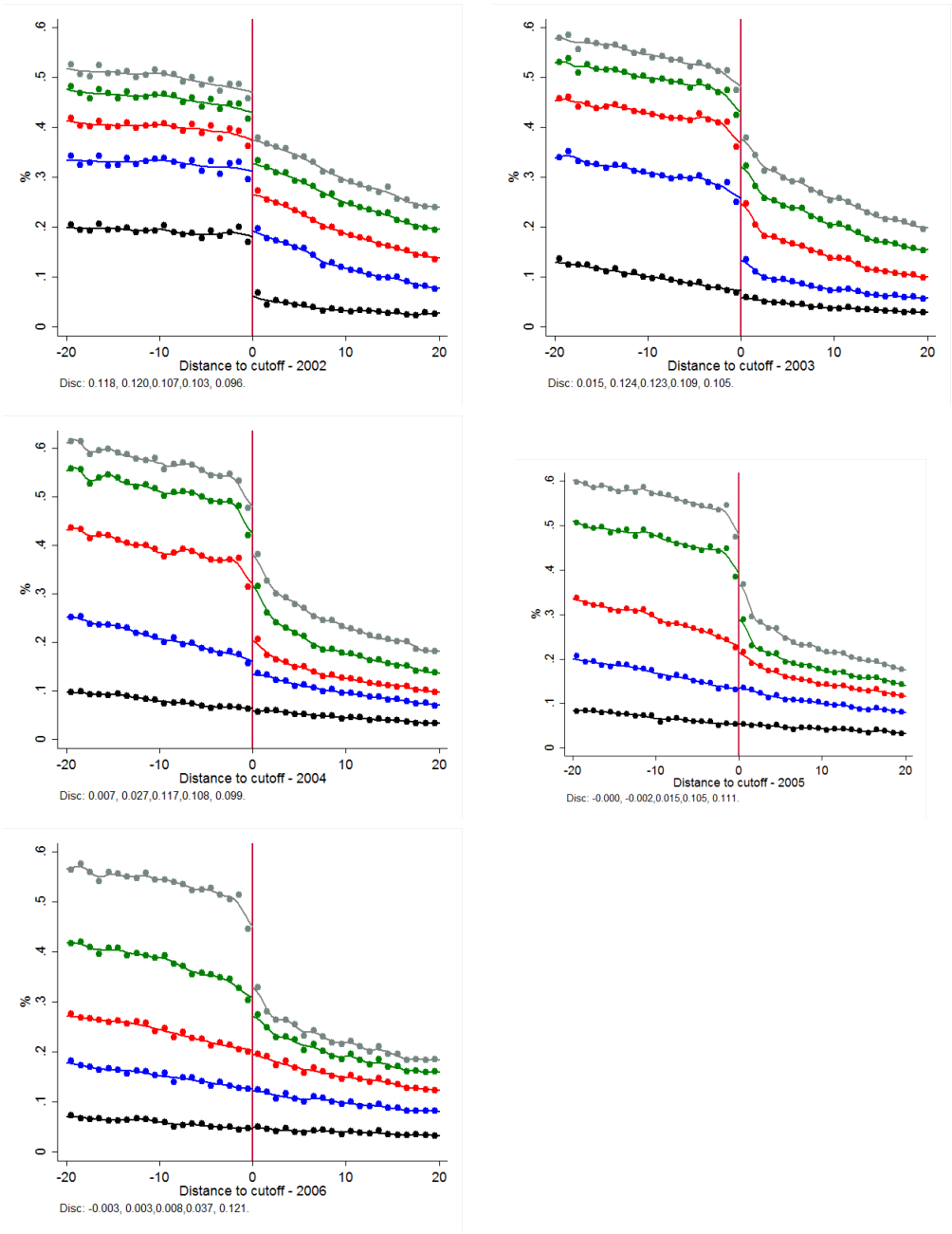
Note: The dots on the graphs are the proportion of families in municipality entering in CS in each year (see y-axis) by intervals of CAS score of 4 points in distribution of CAS in 2002 in each municipality. The solid vertical line represents the point identified as *effective cutoff* and the dashed line is the *official cutoff*. Where only one line is shown, the two coincide. The R^2 in the top of each graph is the R^2 for the cutoff score of CAS that maximises the R^2 of equation (2) - the *effective cutoff*. These figures are zoomed around the *effective cutoff*, so that only families at most 50-points apart from it are depicted (the CAS score varies between 380 and 770 points).

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



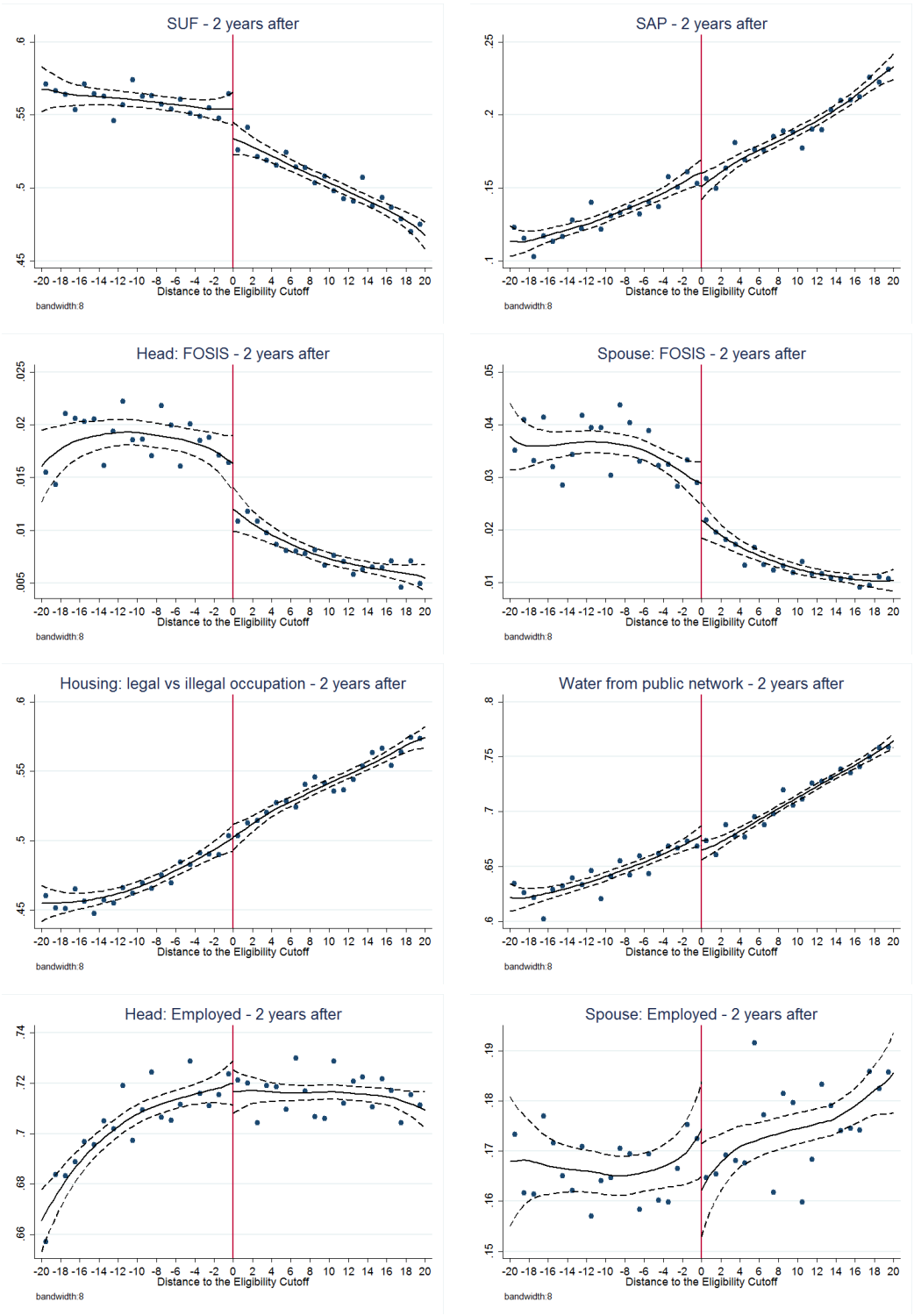
Note: The continuous lines are local linear regression estimates of an indicator for entry in CS in the year indicated on the top of each panel on distance to cutoff in that year. The dashed lines are the 95% confidence intervals. The bandwidth is set to 8. Circles in figures represent the mean outcome by cell within intervals of 1-point of distance to cutoff. The kernel used is Epanechnikov.

Figure 3: Cumulative Entry in CS: Eligibility defined by the effective cutoff.



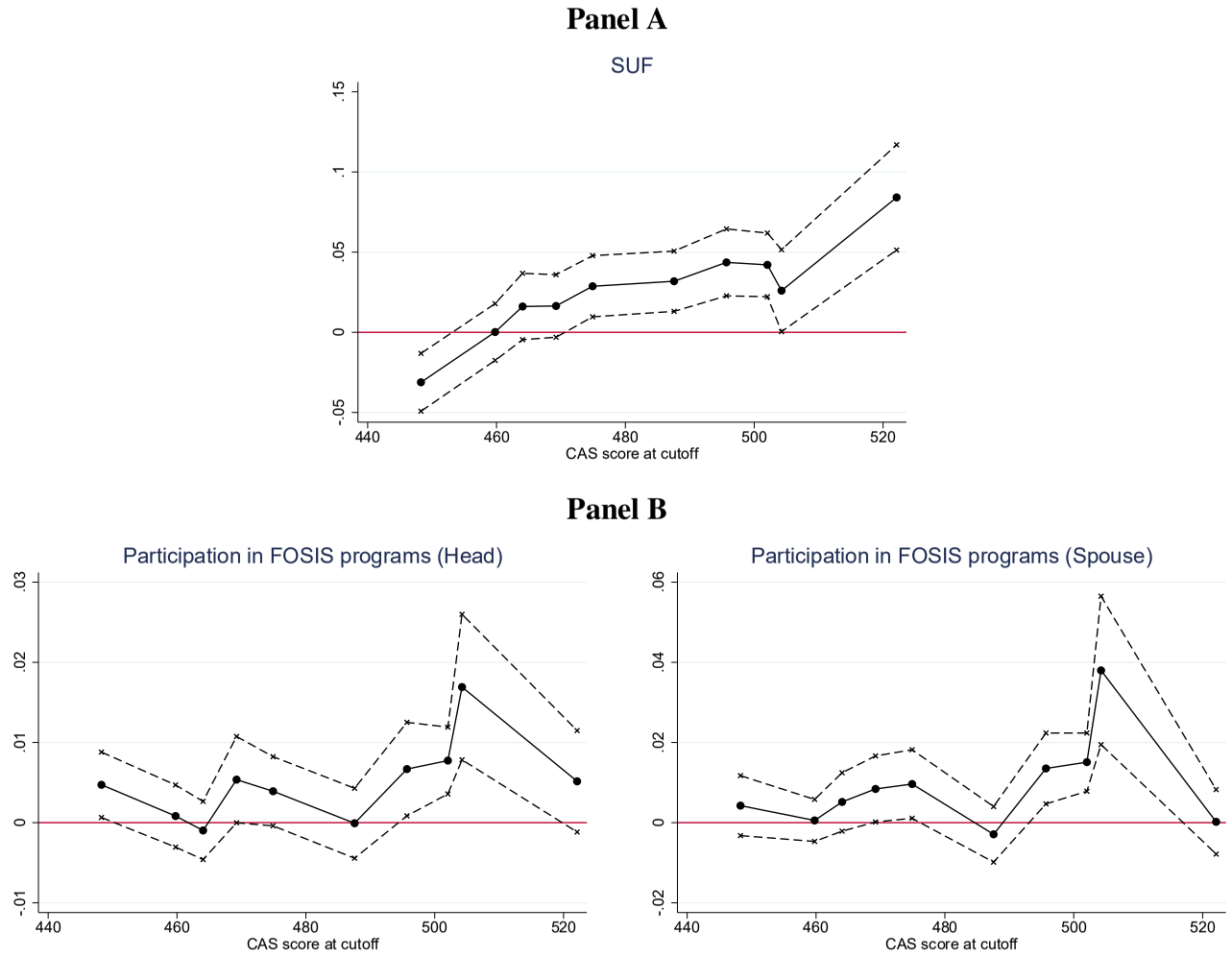
Note: The continuous lines are local linear regression estimates of an indicator for *entry in CS up a given year* on distance to cutoff on the year listed on the x-axis. The lines in each graph correspond to the following dependent variables: (1) the black line refers to entry in CS up to 2002; (2) the blue line refers to cumulative entry in CS up to 2003; (3) the red line refers to cumulative entry in CS up to 2004; (4) the green line refers to cumulative entry in CS up to 2005; and (5) the grey line refers to cumulative entry in CS up to 2006. The numbers on the bottom right of each graph display the estimated discontinuity in the dependent variable at the cutoff, and, from right to left, the values correspond to the probability of entering in the CS up to 2002 (bottom line) up to 2006 (top line), respectively. The bandwidth is set to 8. Circles in figures represent the mean outcome by cell within intervals of 1-point of distance to cutoff. The kernel used is Epanechnikov.

Figure 4: Average outcomes by eligibility status 2 years into the program, Bandwidth = 8.



Note: The continuous lines in figure present local linear regression estimates of several outcomes on the distance to cutoff. Circles in figures represent the mean outcome by cell within intervals of 1 point of distance to cutoff. The kernel used is Epanechnikov. The dashed lines are the 95% confidence intervals.

Figure 5: ITT estimates by CAS score at the cutoff (impacts measured for 2 years of exposure).



Note: The figure presents the estimated coefficients (and standard errors) on eligibility for model 4. In the model, eligibility to CS is interacted with the decile of CAS score cutoff of the municipality, and each dot in the figure corresponds to the deciles on the distribution of CAS-cutoffs. Controls in the model 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.

The dashed lines are the 90% confidence intervals, and the standard errors to construct them are clustered at municipality of residence.

Appendix
Tackling Social Exclusion: Evidence from Chile

A Tables

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

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

Note: "N.A." Not Available. This table is constructed using the Puente Data Set, which is the registry of all families participating in CS.

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

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

FOSIS stands for Fondo de Solidaridad e Inversion Social – Fund for Solidarity and Social Investment, and it implements several programs in the areas of entrepreneurship, employment and social empowerment. SENCE (Servicio Nacional de Capacitacion y Empleo – National Training and Employment Service) is responsible for supervision and to establish job training programs. CONAF (Corporacion Nacional Forestal – National Forest Corporation), which administers policies related to forest. CONADI (Corporacion Nacional de Desarrollo Indígena – National Indigenous Development Corporation) promotes, coordinates and implements development plans that promote the development of indigenous peoples. 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 labour market integration.

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

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

Note: Each year about 50,000 families were invited to participate in the system. Of these, on average, 4.9% did not participate because they refused or because it was not possible to locate the family. The rest, 95.1% started working with social workers. 5.8% 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: Definition of variables used.

Variables		Data available CAS	Data available FPS	Associated minim cond	Sample
Participation					
SUF	1 if at any family member receives SUF. Conditions on presence of age eligible children in family. The sample used for the impacts on SUF conditions on the presence of children in family before 2001, since poor families with children are the target of this subsidy. An additional requirement for the eligibility to SUF is that the family is not receiving Asignacion Familiar, which is assigned to children whose parents have Social Security. We do not observe this requirement in our data, but tabulations from CASEN 2003 show that 87% of CS participants do not receive Asignacion Familiar.	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. Another requirement to receive SAP is to be owner of the place of residence; we do not impose this requirement for two reasons: (1) ownership post-2002 can be affected by the programme and (2) the results are similar if we condition on pre-2002 ownership.	2000-2006			$t+2, t+4$
FOSIS	Individual (head or spouse) participates in at least of FOSIS programme (which are not exclusive for CHS families). In particular, we construct an indicator that takes value 1 if the individual participated in any of the programmes described in table A.2.	external source 2004-2007.			$t+2, t+4$
Labour market and Income					
Not employed	1 if the individual is retired, if he/she did not have any gainful activity in the three months prior to the survey or if he/she is a first time job seeker.	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		
Formal worker	1 if individual is working and affiliation with a Social Security scheme (*cotizando*)		2007-2009		$t+2, t+4, t+6$
Housing					
Legal occupation of house	Owns the place (fully paid and with or without overdue debts) or is a tenant (without overdue rents). The alternative is to simply occupy the place of current, with or without a possible threat of eviction.	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	Data available 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	$t+6$
All children in preschool age in preschool	Indicator of enrolment.		2007-2009	E1	$t+6$
All children with controls by family ($<$8y)	Indicator for health check-ups.		2007-2009	H4	$t+6$
All elderly with controls by family ($\geq$65)	Indicator for health check-ups.		2007-2009	H7	$t+6$
Someone in family had problems w/ alcohol/drugs			2007-2009		$t+6$
Any training program	Any adult 18-65 in family attended a training program.		2007-2009		$t+6$
OMIL	At least of the unemployed members of the family is registered at OMIL (Employment office)		2007-2009	L3	$t+6$

Table A.5: ITT estimates and balancing tests for the whole sample.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Years after start				2					4		
	C. Mean	N	Placebo	ITT	t-stat	Sample	N	Placebo	ITT	t-stat	Sample
Participation											
SUF	0.527	169,965	-0.008	0.023**	3.286	2004-2008	105,422	-0.003	0.031***	3.444	2006-2008
			(0.008)	(0.007)				(0.009)	(0.009)		
SAP	0.163	138,658	0.012	0.003	0.500	2004-2006		0.019	-0.008	-0.571	2006
			(0.005)	(0.006)			16,421	(0.015)	(0.014)		
Labour Market Programs (FOSIS)											
By Head	0.011	212,228		0.004**	2.000	2004-2007	103,581		-0.001	-1.000	2006-2007
				(0.002)					(0.001)		
By Spouse	0.019	142,233		0.007**	2.333		69,545		0.006*	3.000	
				(0.003)					(0.002)		
Labour market											
Head											
Employed	0.715	234,915	0.003	0.000	0.000	2004-2008	197,443	0.004	-0.004	-0.571	2006-2009
			(0.005)	(0.006)				(0.006)	(0.007)		
Self-employed	0.518	234,915	0.005	0.000	0.000	2004-2008	197,443	0.011*	-0.003	-0.429	2006-2009
			(0.006)	(0.006)				(0.007)	(0.007)		
Dependent worker	0.198	234,915	0.000	0.000	0.000	2004-2008	197,443	-0.004	-0.001	-0.167	2006-2009
			(0.005)	(0.005)				(0.005)	(0.006)		
Formal Worker	0.193	85,513		-0.006	-0.667	2007-2008	170,326		-0.010*	-1.667	2007-2009
				(0.009)					(0.006)		
Spouse											
Employed	0.167	137,789	-0.009	0.006	1.000	2004-2008	85,970	-0.004	-0.009	-1.000	2006-2009
			(0.006)	(0.006)				(0.008)	(0.009)		
Self-employed	0.116	137,789	-0.006	-0.001	-0.200	2004-2008	85,970	-0.005	-0.003	-0.375	2006-2009
			(0.005)	(0.005)				(0.007)	(0.008)		
Dependent worker	0.051	137,789	-0.002	0.007	2.333	2004-2008	85,970	0.000	-0.006	-1.000	2006-2009
			(0.004)	(0.003)				(0.005)	(0.006)		
Formal Worker	0.079	32,345		0.006	0.667	2007-2008	67,129		-0.003	-0.500	2007-2009
				(0.009)					(0.006)		
Housing											
Legal occupation of house	0.513	241,297	0.005	-0.002	-0.333	2004-2008	209,152	0.008	-0.000	0.000	2006-2009
			(0.007)	(0.006)				(0.007)	(0.007)		
Sewage connected	0.362	149,405	0.002	0.008	1.333	2004-2006	27,117	0.003	0.008	0.533	2006
			(0.006)	(0.006)				(0.013)	(0.015)		
Water from public network	0.676	241,297	0.005	0.002	0.500	2004-2008	209,153	0.007	0.006	1.200	2006-2009
			(0.004)	(0.004)				(0.005)	(0.005)		
Adequate roof	0.410	149,405	0.015**	-0.002	-0.286	2004-2006	27,117	-0.001	-0.010	-1.000	2006
			(0.007)	(0.007)				(0.005)	(0.010)		
Adequate walls	0.284	149,405	0.017**	-0.001	-0.167	2004-2006	27,117	-0.000	0.008	0.381	2006
			(0.007)	(0.006)				(0.015)	(0.021)		
Heating	0.067	149,405	-0.001	0.000	0.000	2004-2006	27,117	-0.015	0.015	0.833	2006
			(0.003)	(0.004)				(0.014)	(0.018)		
Fridge	0.456	149,405	0.009	0.008	1.000	2004-2006	27,117	-0.030*	-0.001	-0.050	2006
			(0.006)	(0.008)				(0.017)	(0.020)		

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

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

Table A.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)	0.005	-0.019
	(0.005)	(0.011)
Participation (IV)	0.030	-0.161
	(0.030)	(0.093)
Control Mean	0.089	0.075
	Panel B: Receiving SUF before 2002	
Eligibility (ITT)	-0.051*	-0.015
	(0.021)	(0.058)
Participation (IV)	-0.287	-0.289
	(0.122)	(0.668)
Control Mean	0.635	0.529
P-Value: HA: $\beta_0^{iv} > \beta_1^{iv}$	0.000	0.340

Note: See table 3 for a description of the specification used in rows named (ITT) and see table 4 for the description of specification used in rows named (IV). The coefficient estimate in rows (ITT) refers to the indicator of eligibility, E_{im}, whereas the coefficient estimate in rows (IV) refers to the indicator of participation in CS, CS_{im}. "C. Mean" in the mean of the outcome for those at most 4-CAS points above the cutoff.
The last row presents the p-value for the null hypothesis that the effect for those without SAP prior 2002 equals the effect on those receiving SAP, 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 labour 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	0.005*	0.007	0.008
	(0.002)	(0.002)	(0.003)	(0.004)
Participation (IV)	0.026*	0.022**	0.044**	0.035**
	(0.012)	(0.010)	(0.017)	(0.017)
Control Mean	0.013	0.006	0.023	0.009
Observations	149,403	62,825	101,369	40,864
P-Value: HA: $\beta^{iv}_{0506} > \beta^{iv}_{0204}$	0.660		0.660	
	Panel B: Not employed before 2002			
Eligibility (ITT)	0.013***	0.011	0.010***	0.010**
	(0.004)	(0.007)	(0.003)	(0.004)
Participation (IV)	0.081***	0.055	0.061***	0.045**
	(0.025)	(0.032)	(0.018)	(0.018)
Control Mean	0.012	0.008	0.022	0.007
Observations	32,375	12,759	87,774	35,540
P-Value: HA: $\beta^{iv}_{0506} > \beta^{iv}_{0204}$	0.640		0.760	
	Panel C: Employed before 2002			
Eligibility (ITT)	0.002	0.003	-0.016	-0.009
	(0.002)	(0.002)	(0.010)	(0.009)
Participation (IV)	0.014	0.015	-0.082	-0.036
	(0.013)	(0.009)	(0.055)	(0.040)
Control Mean	0.013	0.005	0.030	0.019
Observations	117,028	50,066	13,595	5,324
P-Value: HA: $\beta^{iv}_{0506} > \beta^{iv}_{0204}$	0.580		0.280	
P-Value: HA: $\beta^{iv}_{0} > \beta^{iv}_{1}$	0.000	0.080	0.020	0.000

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

The p-value in the table concerns the null hypothesis that the effect for those that entered in CS in the years of 2005-2006 equals the effect of those that entered between 200-2004, 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).

Table A.8: Impacts on Labour Market Outcomes (cohorts 2002-2006).

Variable	(1) Employed	(2) Employed	(3) Self-Employed	(4) Self-Employed	(5) Wage worker	(6) Wage worker
Years after entry	2	4	2	4	2	4
			Panel A: Head			
			Panel A1: Not Employment before 2002			
Eligibility (ITT)	-0.012	-0.020	0.001	-0.018	-0.013	-0.002
	(0.012)	(0.014)	(0.011)	(0.013)	(0.006)	(0.010)
Participation (IV)	-0.067	-0.110	0.006	-0.103	-0.073	-0.008
	(0.068)	(0.084)	(0.061)	(0.076)	(0.037)	(0.058)
Control Mean	0.244		0.181		0.0625	
Observations	48,062	34,451	48,062	34,451	48,062	34,451
			Panel A2: Employed before 2002			
Eligibility (ITT)	0.002	-0.003	-0.000	-0.001	0.003	-0.002
	(0.005)	(0.006)	(0.006)	(0.008)	(0.006)	(0.007)
Participation (IV)	0.012	-0.016	-0.002	-0.001	0.014	-0.015
	(0.028)	(0.032)	(0.033)	(0.043)	(0.031)	(0.039)
Control Mean	0.835		0.603		0.231	
Observations	186,853	162,992	186,853	162,992	186,853	162,992
P-Value: HA: $\beta_0^{iv} > \beta_1^{iv}$	0.840	0.860	0.400	0.840	0.880	0.540
			Panel B: Spouse			
			Panel B1: Not Employment before 2002			
Eligibility (ITT)	0.008	-0.004	0.000	0.000	0.008*	-0.004
	(0.005)	(0.009)	(0.005)	(0.008)	(0.003)	(0.006)
Participation (IV)	0.045	-0.021	0.002	0.001	0.043*	-0.023
	(0.028)	(0.047)	(0.025)	(0.040)	(0.018)	(0.033)
Control Mean	0.126		0.0875		0.0381	
Observations	117,331	71,586	117,331	71,586	117,331	71,586
			Panel B2: Employment before 2002			
Eligibility (ITT)	0.002	-0.031	-0.011	-0.018	0.013	-0.013
	(0.022)	(0.026)	(0.019)	(0.027)	(0.014)	(0.020)
Participation (IV)	0.030	-0.181	-0.073	-0.114	0.103	-0.067
	(0.131)	(0.160)	(0.113)	(0.166)	(0.083)	(0.123)
Control Mean	0.409		0.286		0.124	
Observations	20,458	14,384	20,458	14,384	20,458	14,384
P-Value: HA: $\beta_0^{iv} > \beta_1^{iv}$	0.440	0.140	.360	0.200	0.560	0.240

Note: See table 3 for the specification used in rows (ITT) and table 4 for rows (IV). The coefficient estimate in rows (ITT) is the indicator of eligibility, E_{im}, and the coefficient estimate in rows (IV) is the indicator of participation in CS, CS_{im}.
Robust standard errors are reported in parenthesis clustered at municipality of residence when eligibility is evaluated. * significant at 10%; ** significant at 5%; *** significant at 1% (the critical values for inference are adjusted for multiple hypotheses testing).

Table A.9: ITT estimates for quality of housing (impacts measured two years after potential entry in CS).

	(1)	(2)	(3)	(4)	(5)
	Basic	Before 2002		Area	
		No	Yes	Urban	Rural
			Panel A: Water Connection		
A.1: Water from Public Network	0.002	0.002	-0.002	-0.003	-0.003
	(0.004)	(0.007)	(0.003)	(0.003)	(0.008)
Control Mean	0.676	0.145	0.956	0.977	0.287
A.2: Water fetched to the house	0.001	0.002	0.008	-0.002	0.011
	(0.004)	(0.004)	(0.016)	(0.002)	(0.009)
Control Mean	0.149	0.0661	0.552	0.00967	0.329
			Panel B: Tenency		
House owner	-0.000	0.004	-0.006	0.010	-0.013
	(0.006)	(0.006)	(0.007)	(0.008)	(0.009)
Control Mean	0.479	0.234	0.864	0.452	0.514
Observations	241,297	123,031	118,266		
			Panel C: Sewage Connection		
Sewage connected to network	0.008	0.007	0.008	0.011	-0.003
	(0.006)	(0.006)	(0.011)	(0.009)	(0.004)
Control Mean	0.362	0.132	0.861	0.616	0.0346
			Panel D: Quality of the walls		
Adequate walls	-0.001	-0.011	0.009	0.005	-0.008
	(0.006)	(0.006)	(0.014)	(0.008)	(0.010)
Control Mean	0.284	0.184	0.539	0.285	0.282
			Panel E: Quality of the ceiling		
Adequate ceiling	-0.002	-0.008	-0.002	0.011	-0.018
	(0.007)	(0.008)	(0.011)	(0.009)	(0.009)
Control Mean	0.410	0.256	0.654	0.456	0.351
			Panel F: Electricity		
House with electricity	0.002	0.018	-0.004	0.000	0.001
	(0.005)	(0.027)	(0.003)	(0.003)	(0.011)
Control Mean	0.929	0.625	0.986	0.989	0.854

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

Table A.10: Impact of CS: ITT and IV estimates for the whole sample collected from FPS (2007-2009).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Years after start			2			4	
	C. Mean	N	ITT	IV	N	ITT	IV
All children with controls by family (<8y)	0.990	23,383	-0.001	-0.006	48,403	-0.003	-0.015
			(0.004)	(0.018)		(0.002)	(0.012)
All elderly with controls by family (≥65)	0.646	24,085	0.026	0.189	46,315	0.015	0.113
			(0.019)	(0.148)		(0.015)	(0.115)
Unemployed individuals enrolled in OMIL	0.289	5,663	0.039	0.212	11,285	0.042	0.185
			(0.037)	(0.188)		(0.028)	(0.129)
All children in preschool age in preschool	0.471	8,500	0.004	0.076	17,885	-0.004	-0.015
			(0.033)	(0.141)		(0.025)	(0.120)
All children 6-11 enrolled in school	0.952	30,017	0.010	0.038	60,442	-0.012	-0.050
			(0.007)	(0.026)		(0.006)	(0.029)
All children 12-14 enrolled in school	0.987	20,180	-0.002	-0.009	41,204	0.003	0.015
			(0.005)	(0.023)		(0.003)	(0.014)
All children 15-18 enrolled in school	0.849	20,923	-0.003	-0.010	45,267	0.000	-0.000
			(0.015)	(0.062)		(0.009)	(0.040)
At least one indiv 19-24 in college	0.234	16,763	0.026	0.106	38,870	0.018	0.086
			(0.021)	(0.082)		(0.014)	(0.065)
Someone in family had problems w/ alcohol/drugs	0.0360	90,575	0.005	0.021	180,540	0.005	0.027
			(0.004)	(0.020)		(0.003)	(0.015)

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

Table A.11: ITT Estimates: Choice of Fixed Effects (impacts measured two years after potential entry in CS).

Fixed Effects	(1) Baseline	(2) Municip Year	(3) Neighb Year	(4) Neighb-Year
SUF	0.023***	0.022**	0.025***	0.023***
	(0.007)	(0.007)	(0.008)	(0.008)
Labour Market Programs - FOSIS (Head)	0.004**	0.004**	0.003	0.004
	(0.002)	(0.002)	(0.002)	(0.002)
Labour Market Programs - FOSIS (Spouse)	0.007**	0.007***	0.006*	0.006*
	(0.003)	(0.003)	(0.003)	(0.003)

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

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years after start	2				4			
	Baseline		Not conditioning		Baseline		Not conditioning	
	N	ITT	N	ITT	N	ITT	N	ITT
SUF	169,965	0.023**	355,261	0.021***	105,422	0.031***	209,769	0.026***
		(0.007)		(0.005)		(0.009)		(0.006)
FOSIS participation (head)	212,228	0.004**	312,393	0.006***	103,581	-0.001	141,916	0.000
		(0.002)		(0.001)		(0.001)		(0.001)
FOSIS participation (spouse)	142,233	0.007**	199,755	0.008***	69,545	0.006*	93,155	0.003
		(0.003)		(0.002)		(0.002)		(0.002)

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

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

	(1) First Stage	(2) SUF	(3) Participation in FOSIS Programs Head	(4) Spouse
			Panel A: Municipalities with discontinuity	
Eligibility (ITT)	0.298***	0.030***	0.006**	0.009**
	(0.008)	(0.009)	(0.002)	(0.003)
Observations	113,701	113,701	139,055	91,875
Control Mean	0.100	0.530	0.012	0.023
			Panel B: Municipalities with low or no discontinuity	
Eligibility (ITT)	0.090***	0.010	0.001	0.004
	(0.006)	(0.012)	(0.002)	(0.003)
Observations	56,264	56,264	73,173	50,358
Control Mean	0.042	0.517	0.008	0.011

Note: The table presents estimates for coefficient on eligibility from model 4. The effects are measured two (potential) years after entry in CS. Controls excluded are: quadratic in distance to cutoff, their interaction with eligibility to CS and municipality-year effects. The two panels in the table include estimates separately for those municipalities with (Panel A) and without (Panel B) discontinuity in estimation of equation 2. We consider that a municipality has discontinuity in estimation of equation (2) if the estimate for ψ at the value of CAS-score that maximizes the R^2 of (2) is at least 0.1.
Robust standard errors are reported in parenthesis clustered at municipality of residence when eligibility is evaluated. * significant at 10%; ** significant at 5%; *** significant at 1% (the critical values for inference are not adjusted for multiple hypotheses testing).

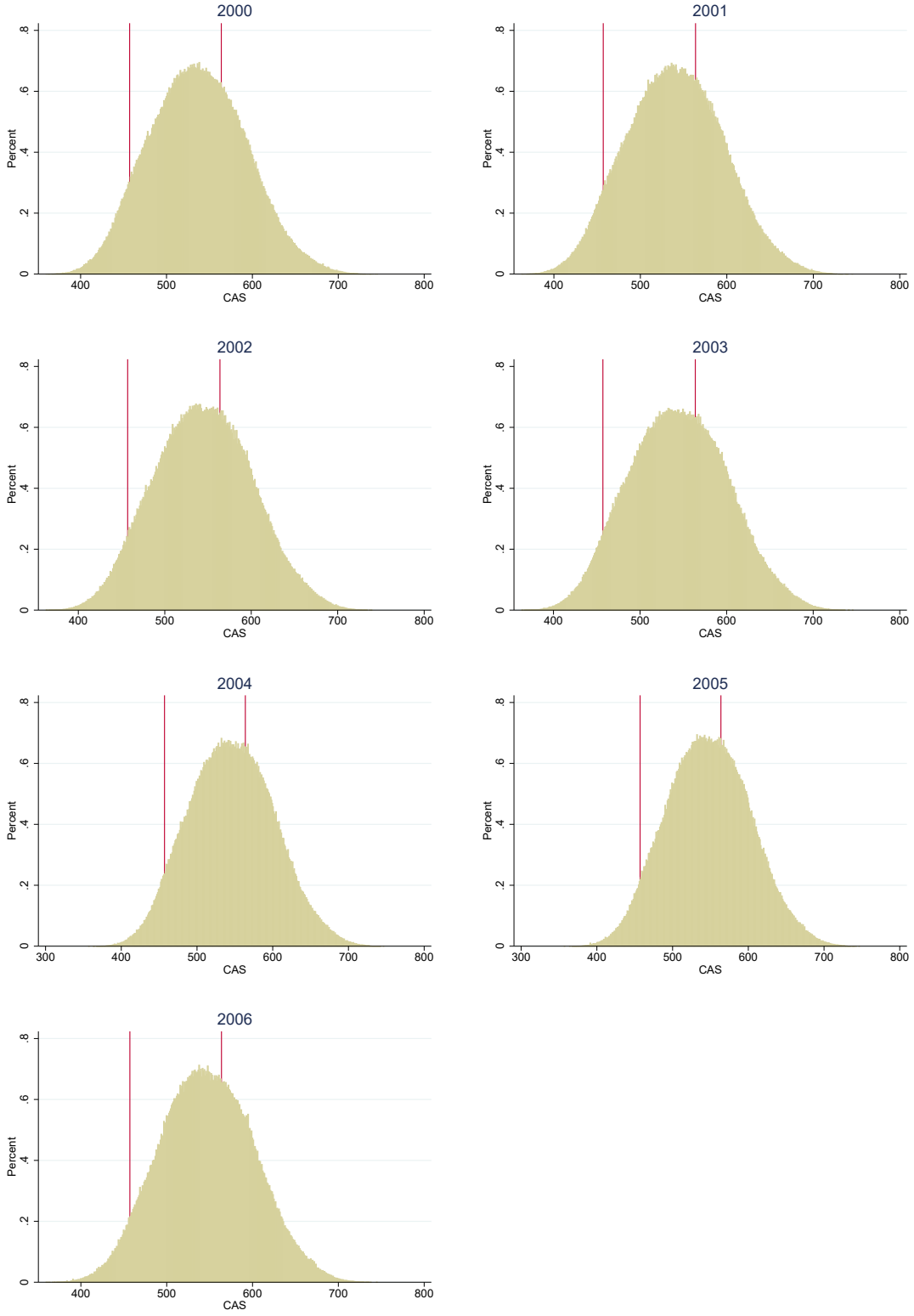
Table A.14: ITT Estimates: Cutoff obtained through split-sample.

	(1)	(2)	(3)
	SUF	Participation in FOSIS programs	
		Head	Spouse
Eligibility (ITT)	0.038***	0.009***	0.014***
	(0.006)	(0.002)	(0.003)
Participation (IV)	0.133***	0.036***	0.061***
	(0.020)	(0.006)	(0.010)
Observations	119,852	138,736	93,002

Note: The table presents estimates for the coefficients on eligibility and participation from models 4 and 5. The effects are measured two years after (potential) entry in CS. Controls excluded are: quadratic in distance to cutoff, their interaction with eligibility to CS and municipality-year effects.
The estimates presented in this table are obtained in the following way. First, the *effective cutoff* for eligibility for each year between 2002 and 2006 and for each municipality is estimated as in equation 2. A random sample of 2/3 of families is used. Models 4 and 5 are estimated using the sample of families not used to estimate the municipality-year cutoffs.
Robust standard errors are reported in parenthesis clustered at municipality of residence when eligibility is evaluated. * significant at 10%; ** significant at 5%; *** significant at 1% (the critical values for inference are not adjusted for multiple hypotheses testing).

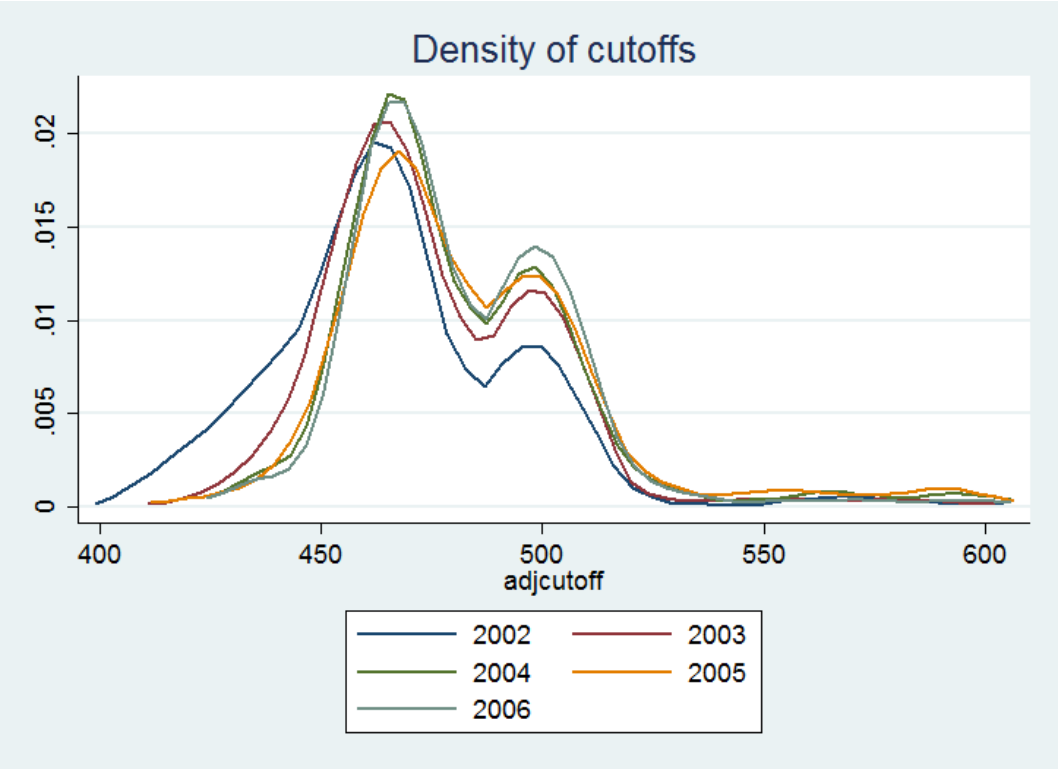
B Figures

Figure B.1: Distribution of the poverty index - CAS score (2000-2006).



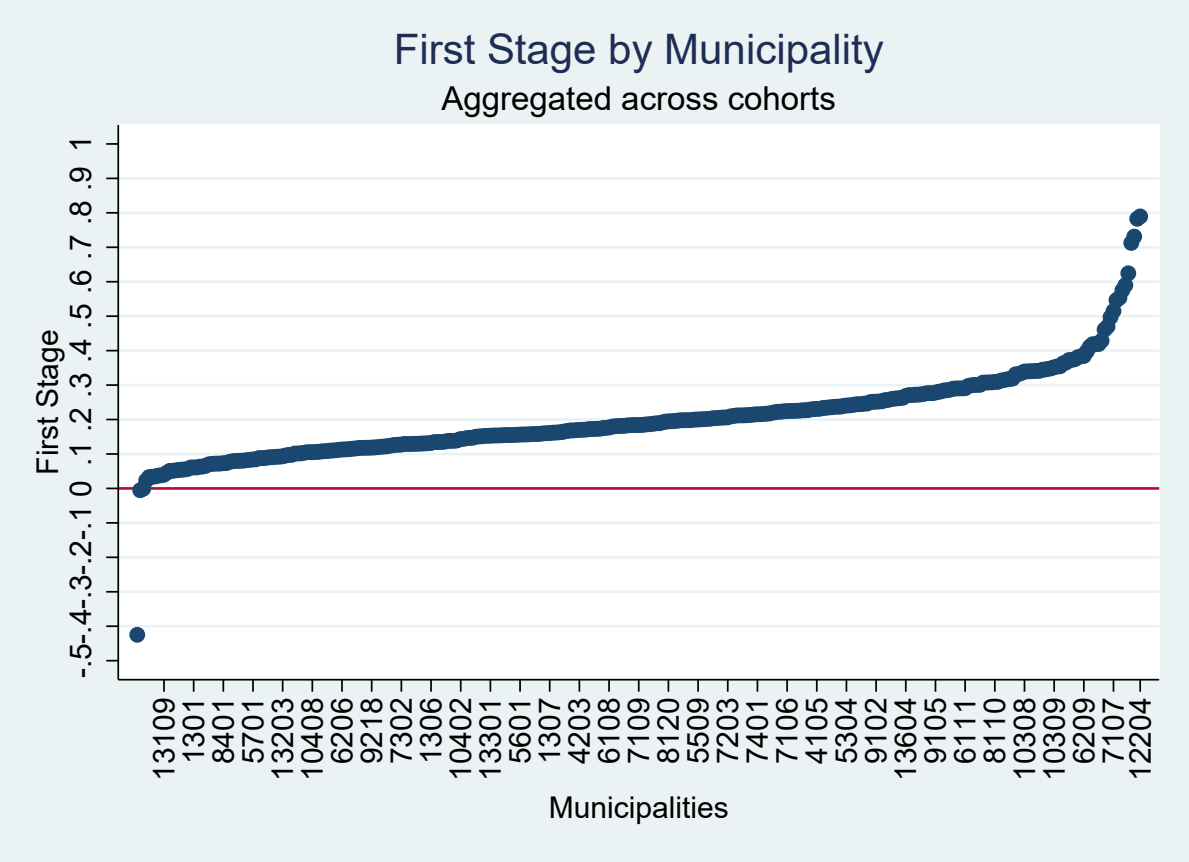
Note: Each figure corresponds to the families with a valid score in a given year. The vertical lines represent the maximum and minimum cutoff scores in each year.

Figure B.2: Distribution of effective cutoffs



Note: The figure presents the distributions of cutoffs for each municipality that correspond to the value of CAS g that maximises the R^2 of (2).

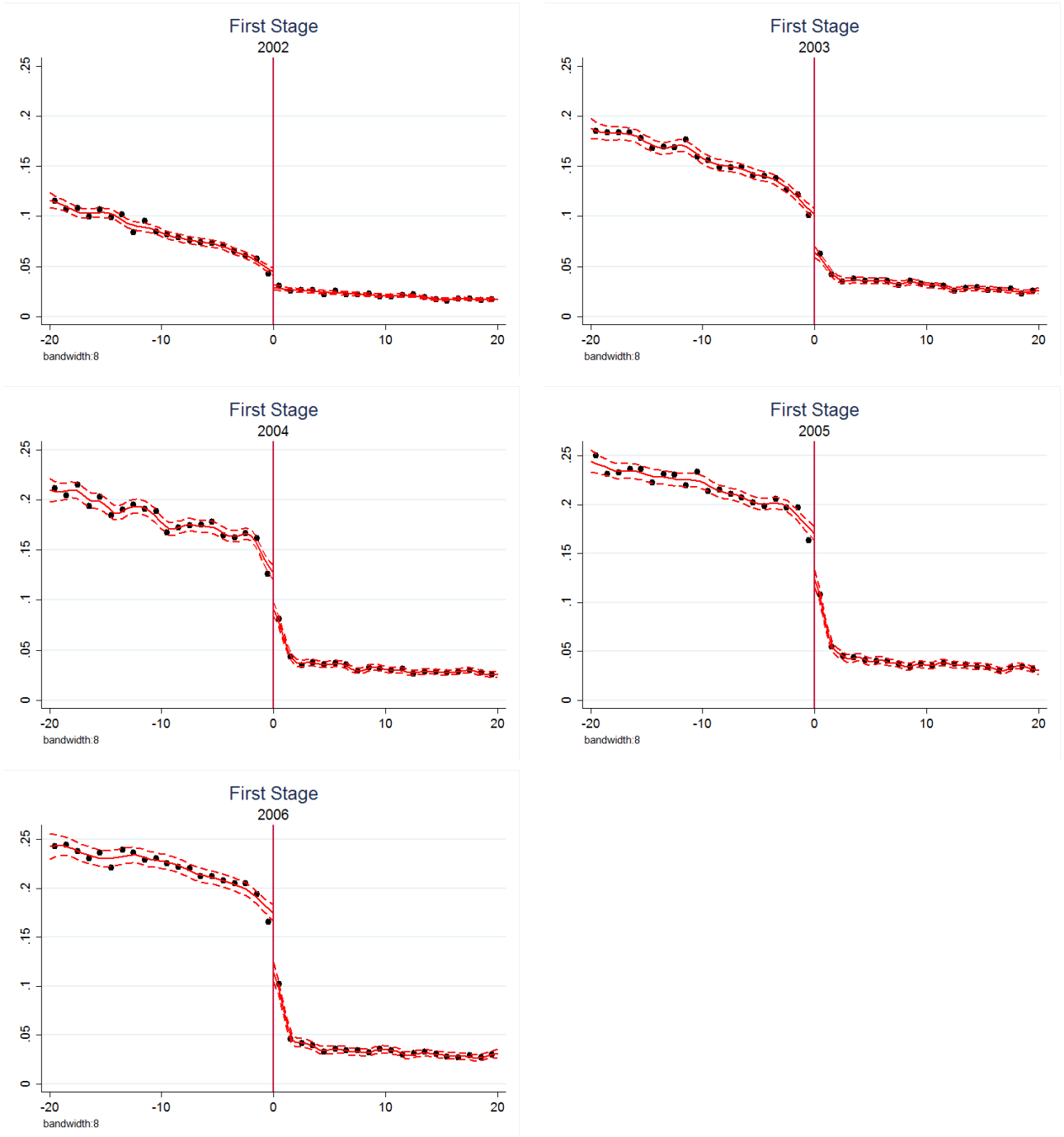
Figure B.3: First Stage Estimates by Municipality.



Note: Each circle in the figure presents estimates for ρ from the following first stage equation $CS_{imt-k} = \tau + \rho E_{imt-k} + f(CAS_{imt-k} - \overline{CAS}_{mt-k}) + u_{imt-k}$ estimated separately for each municipality. All cohorts (2002-2006) are pooled together.

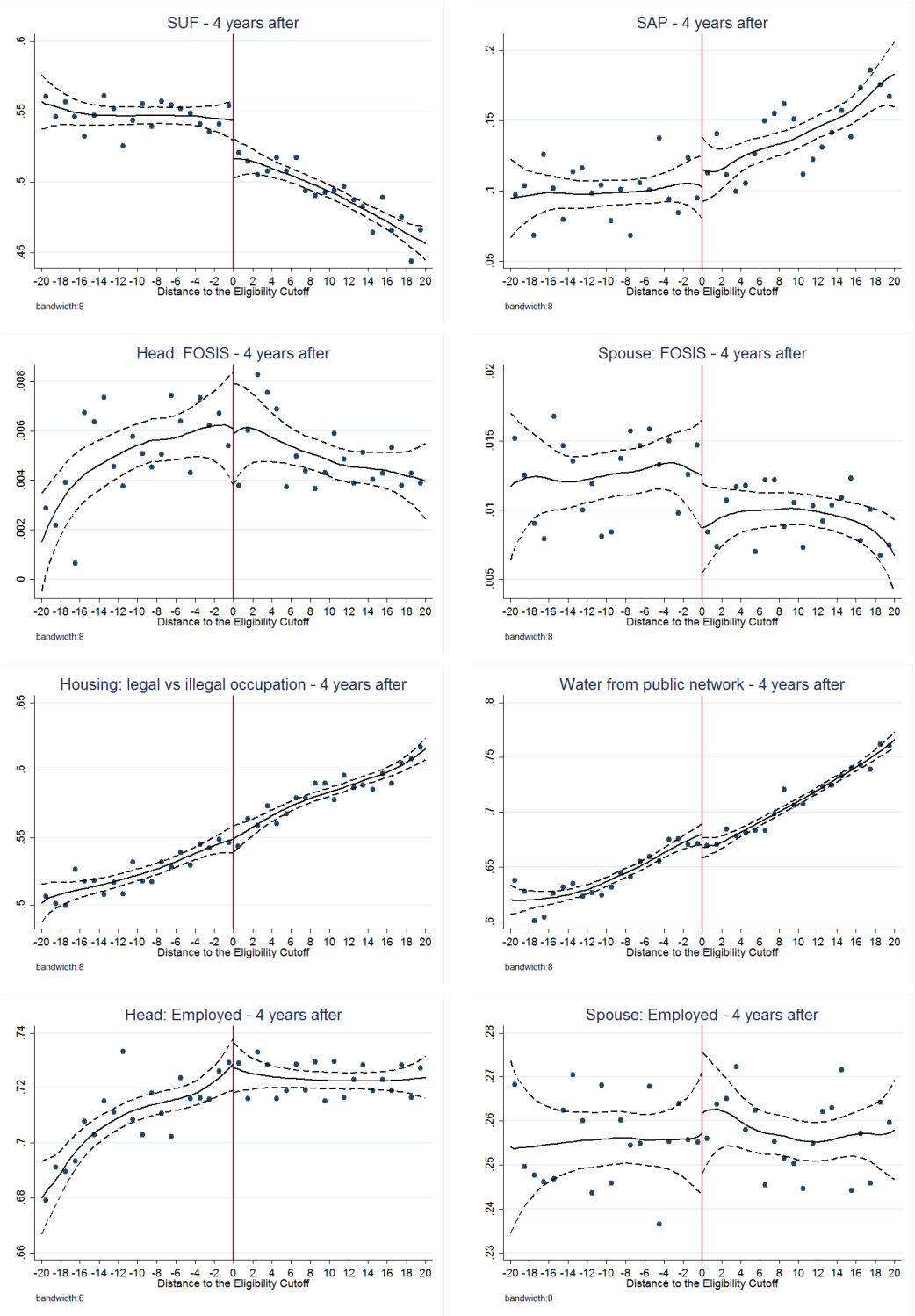
The codes in the x-axis refer to the municipalities, which are ordered by size of $\hat{\rho}$. The codes correspond to the Chilean official coding and are the same codes using in figures C.1 and C.2. Not all municipalities are labelled for presentation proposes.

Figure B.4: Participation in CS among eligible and non-eligible: Eligibility defined by the official cutoff.



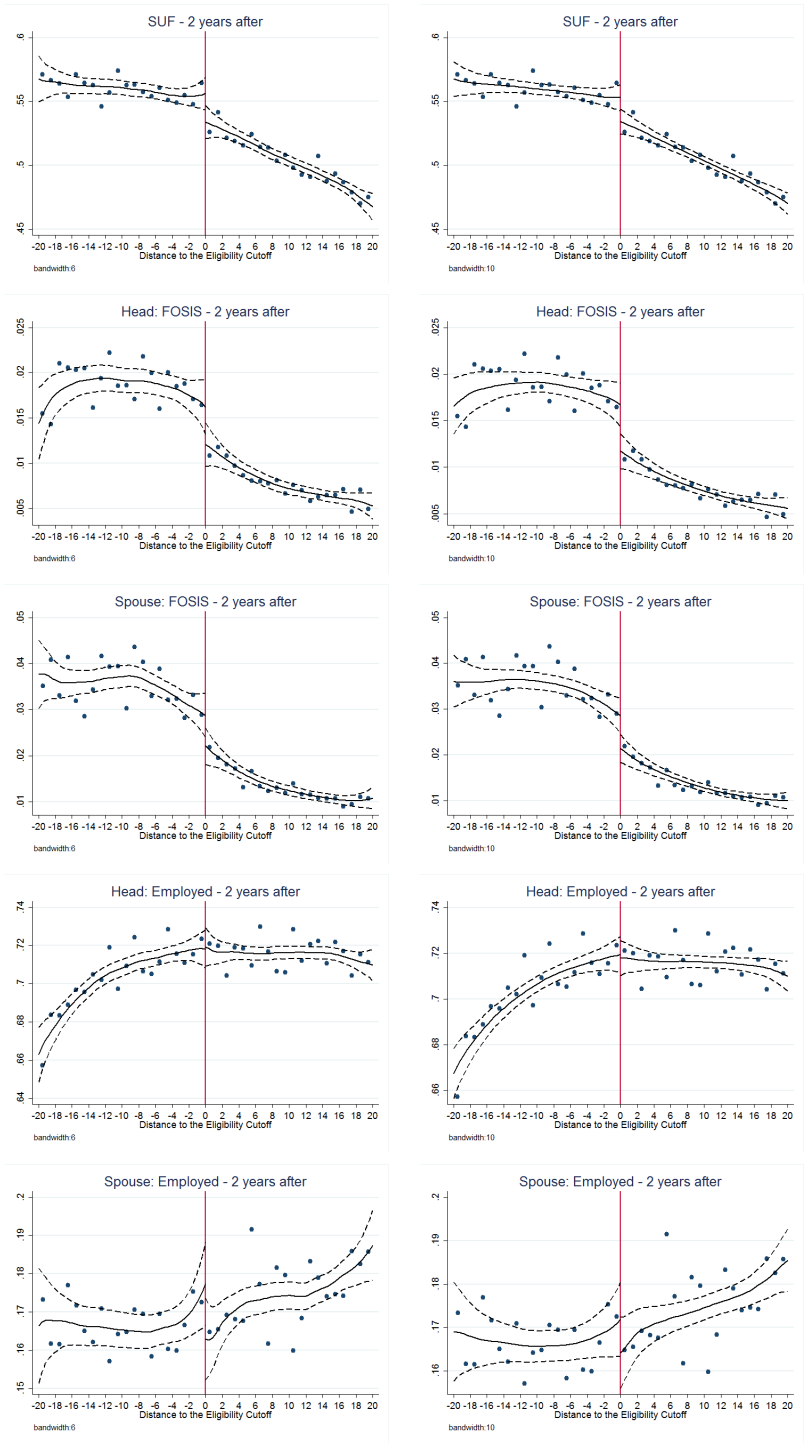
Note: The continuous lines are local linear regression estimates of an indicator for entry in CS in the year indicated on the top of each panel on distance to cutoff in that year. The bandwidth is set to 8. The dashed lines are the 95% confidence intervals. Circles in figures represent the mean outcome by cell within intervals of 1-point of distance to cutoff. The kernel used is Epanechnikov.

Figure B.5: Average outcomes by eligibility status 4 years into the program, Bandwidth = 8.



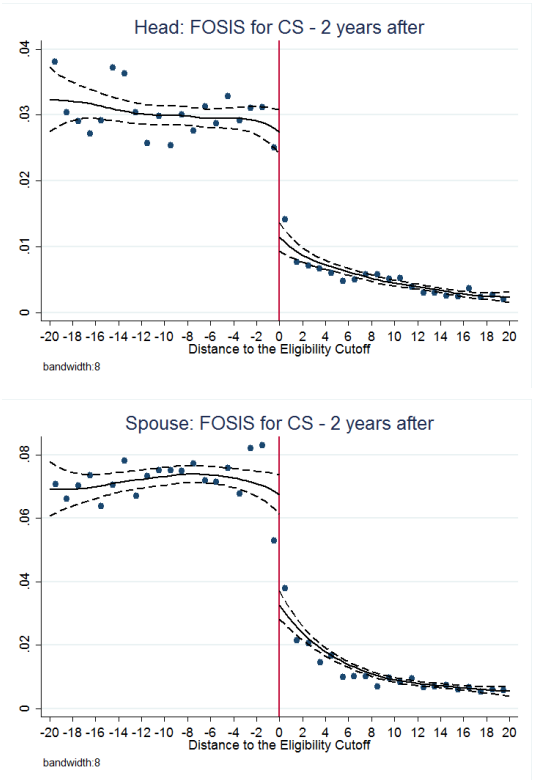
Note: The continuous lines in figure present local linear regression estimates of several outcomes on the distance to cutoff. Circles in figures represent the mean outcome by cell within intervals of 1 point of distance to cutoff. The kernel used is Epanechnikov. The dashed lines are the 95% confidence intervals.

Figure B.6: Average outcomes by eligibility status 2 years into the program: different bandwidths.



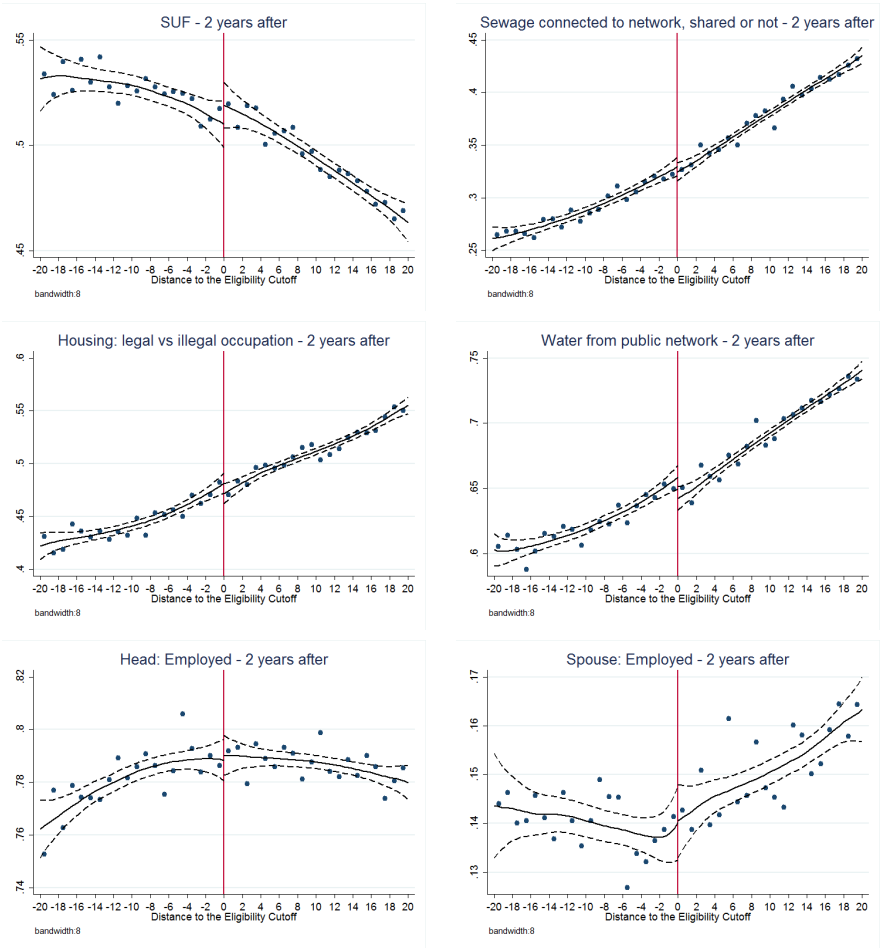
Note: The figures on the left hand side use a bandwidth of 6-points, whereas the figures on the right hand side use a bandwidth of 10-points. The continuous lines in figure present local linear regression estimates of several outcomes on the distance to cutoff. Circles in figures represent the mean outcome by cell within intervals of 1 point of distance to cutoff. The dashed lines are the 95% confidence intervals. The kernel used is Epanechnikov

Figure B.7: Average outcomes by eligibility status 2 years into the program, Bandwidth = 8.



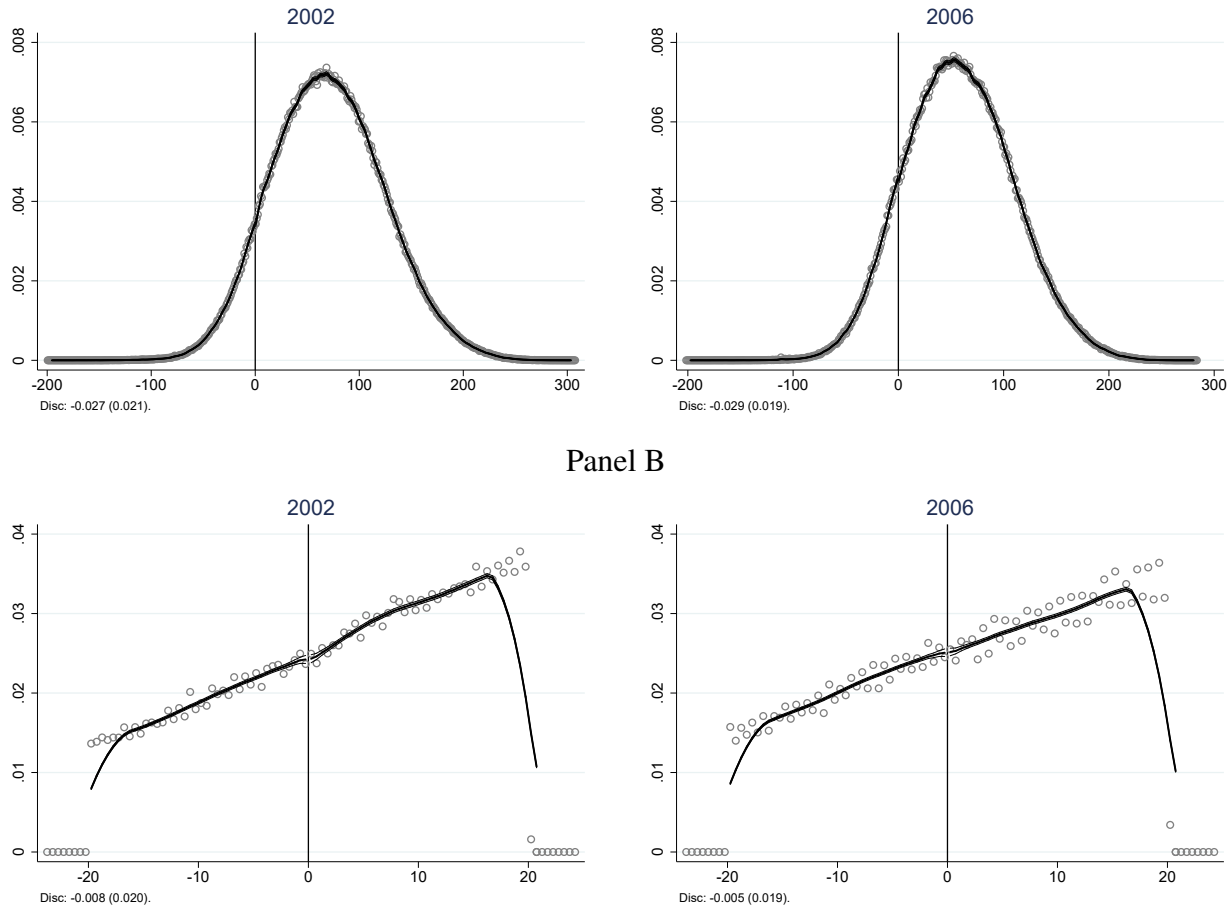
Note: The continuous lines in figure present local linear regression estimates of several outcomes on the distance to cutoff. Circles in figures represent the mean outcome by cell within intervals of 1 point of distance to cutoff. The kernel used is Epanechnikov. The dashed lines are the 95% confidence intervals.

Figure B.8: Balancing checks: Pre-2002 outcomes by eligibility status, Bandwidth = 8.



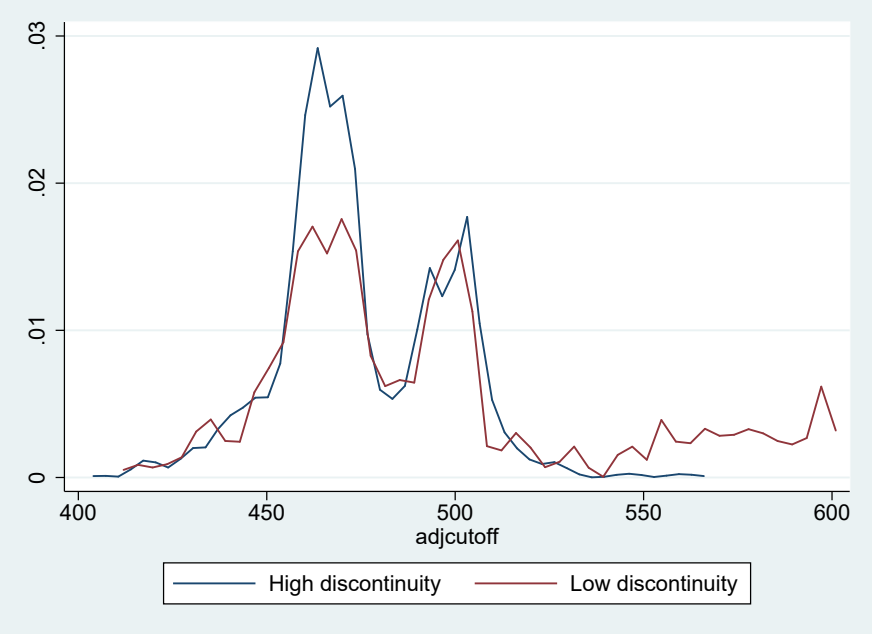
Note: The continuous lines in figure present local linear regression estimates of several outcomes on percentage distance to cutoff. Circles in figures represent the mean outcome by cell within intervals of 1 point of distance to cutoff. The kernel used is Epanechnikov.
Next to the outcome used we include the designation of the sample used, that is, this figures uses the sample used in the estimation of potential exposure to Chile Solidario for 2 years.

Figure B.9: Density of observations around the cutoff



Note: The graphs present nonparametric estimates of the density of observations on either side of the cutoff score for eligibility to CS following McCrary (2008). The cutoffs are centred around zero and the running variable is the distance to the cutoff. The top figures present estimates for the whole sample, whereas the bottom graphs are restricted to families with CAS score at most 20-points away from the cutoff, corresponding to the main sample used. In the bottom right of each graph we include the estimated discontinuity for the density at the cutoff and in parenthesis the standard errors.

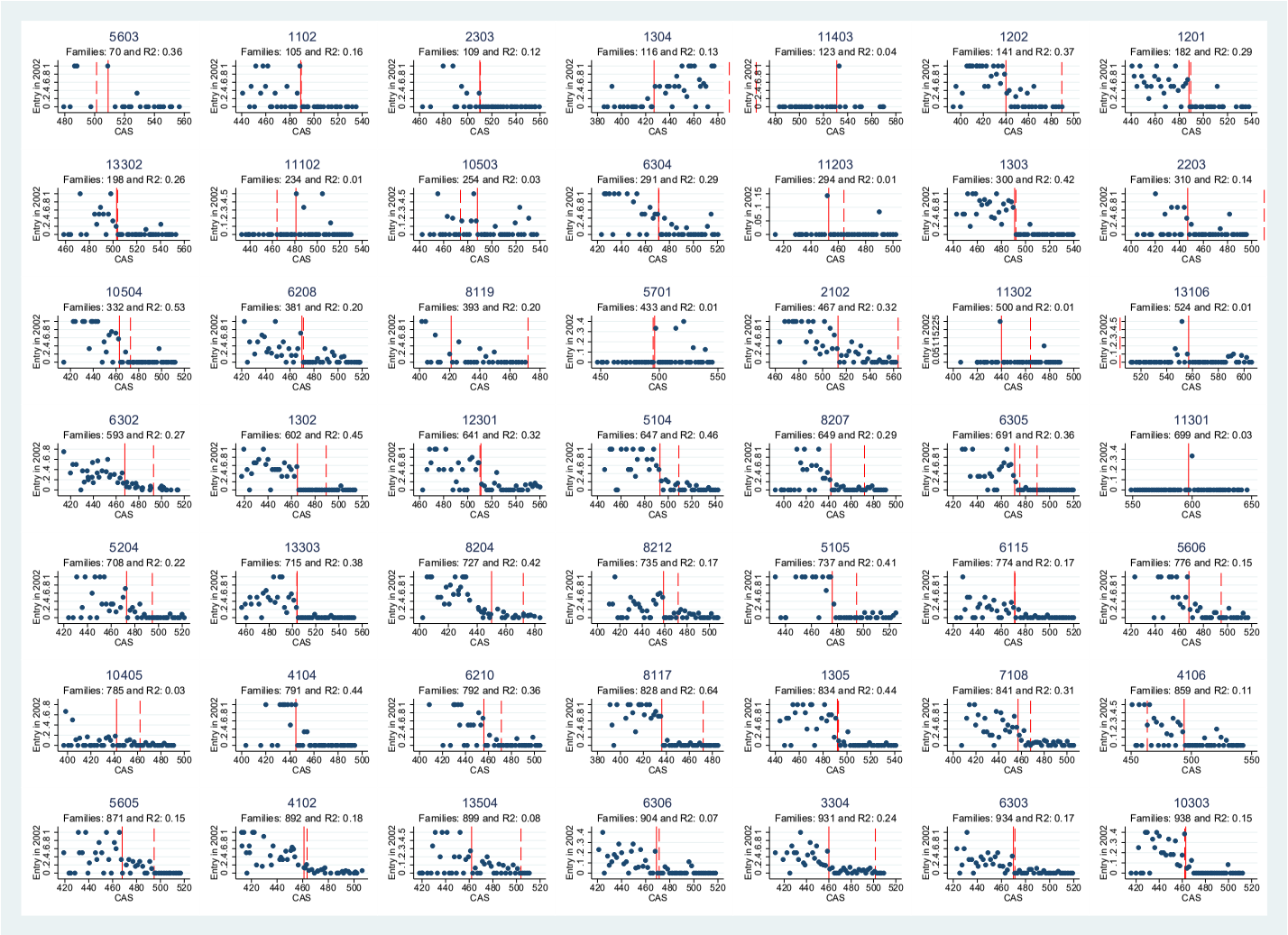
Figure B.10: Distribution of effective cutoffs



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

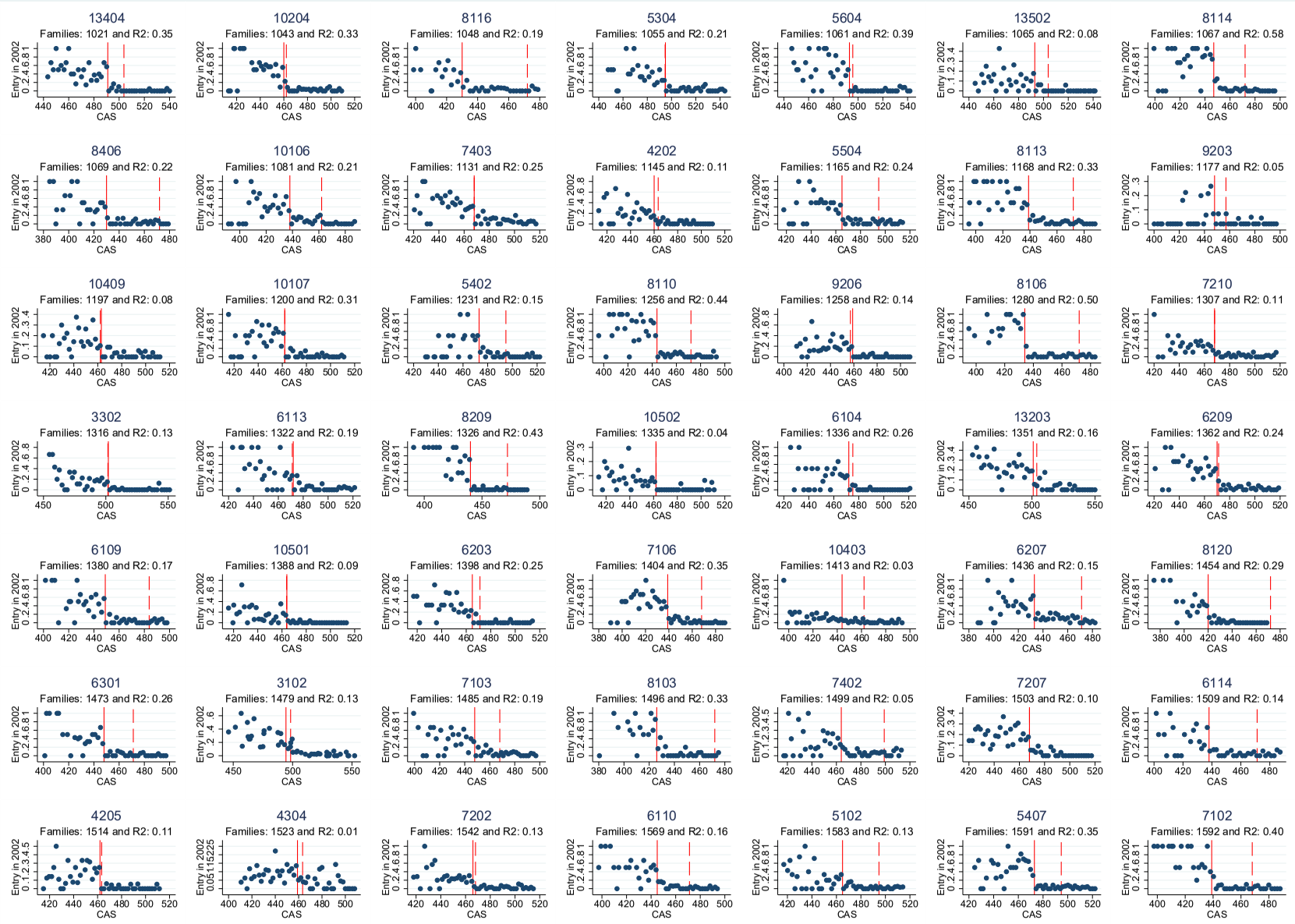
C Cutoffs by Municipalities

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

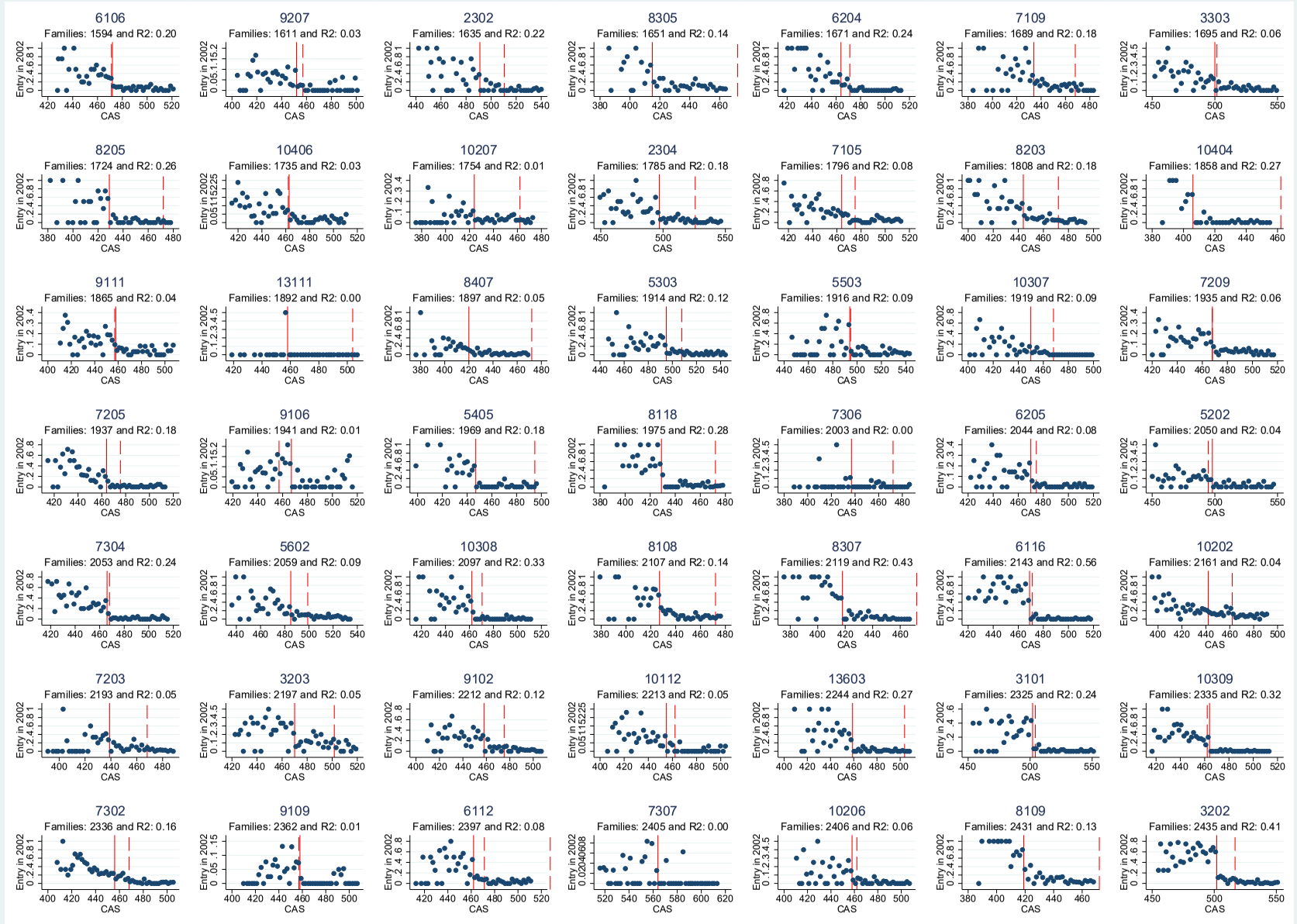


Note: The dots on the graphs are the proportion of families in municipality entering in CS in each year by intervals of CAS score of 4 points in 2002 in each municipality. The solid vertical line represents the point identified as *effective cutoff* and the dashed line is the *official cutoff*. Where only one line is shown, the two coincide. The graphs in the figure are ordered by increased size of municipality of residence in 2002. The R^2 in the top of each graph is the R^2 for the cutoff score of CAS that maximises the fit of equation (2) - the *effective cutoff*. The figures are zoomed around the *effective cutoff*, and only families at most 50-points apart from it are depicted (the CAS score varies between 380 and 770 points).

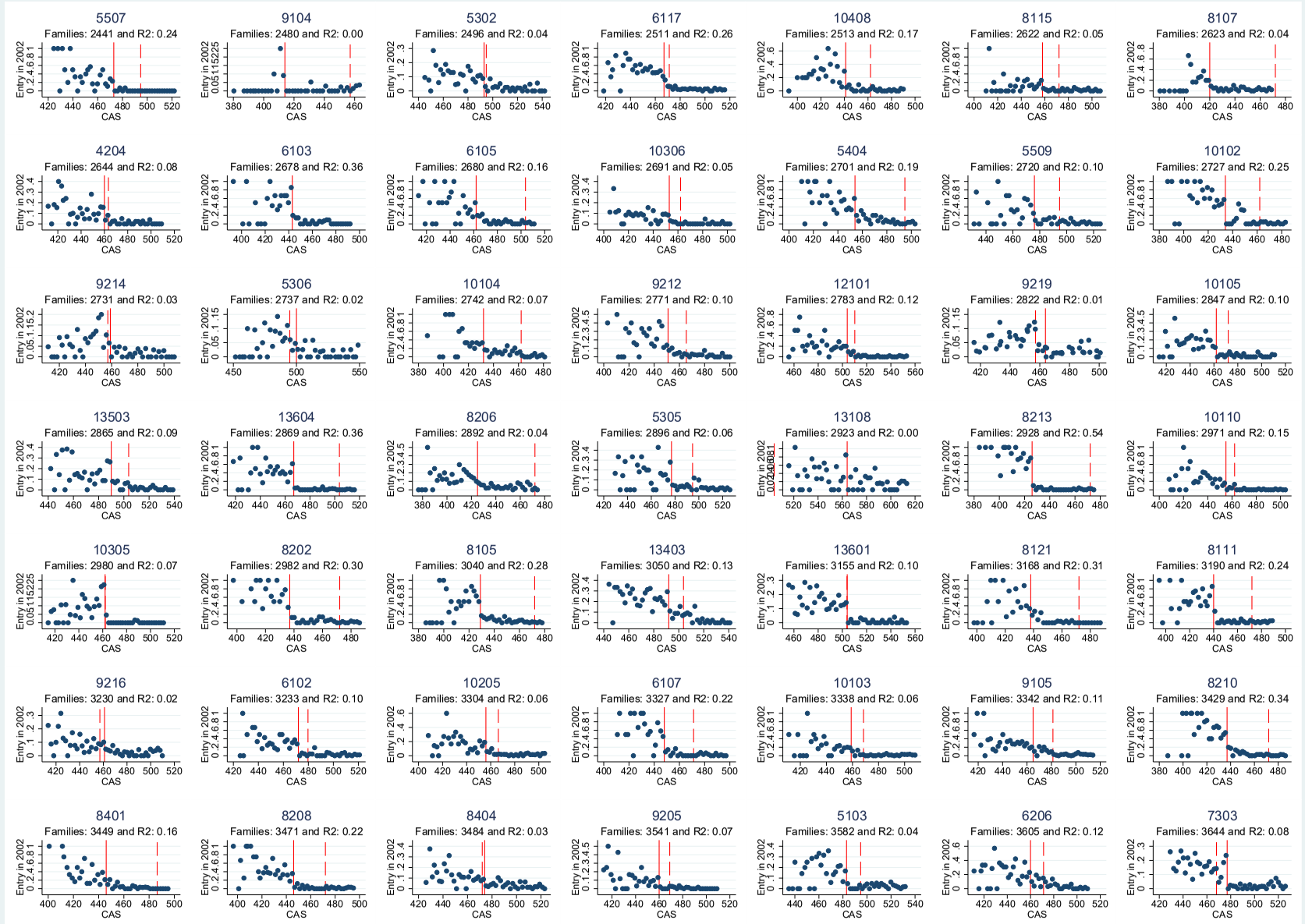
Participation in CS and *effective cutoff* and *official cutoff* in illustrative municipalities (cont.).



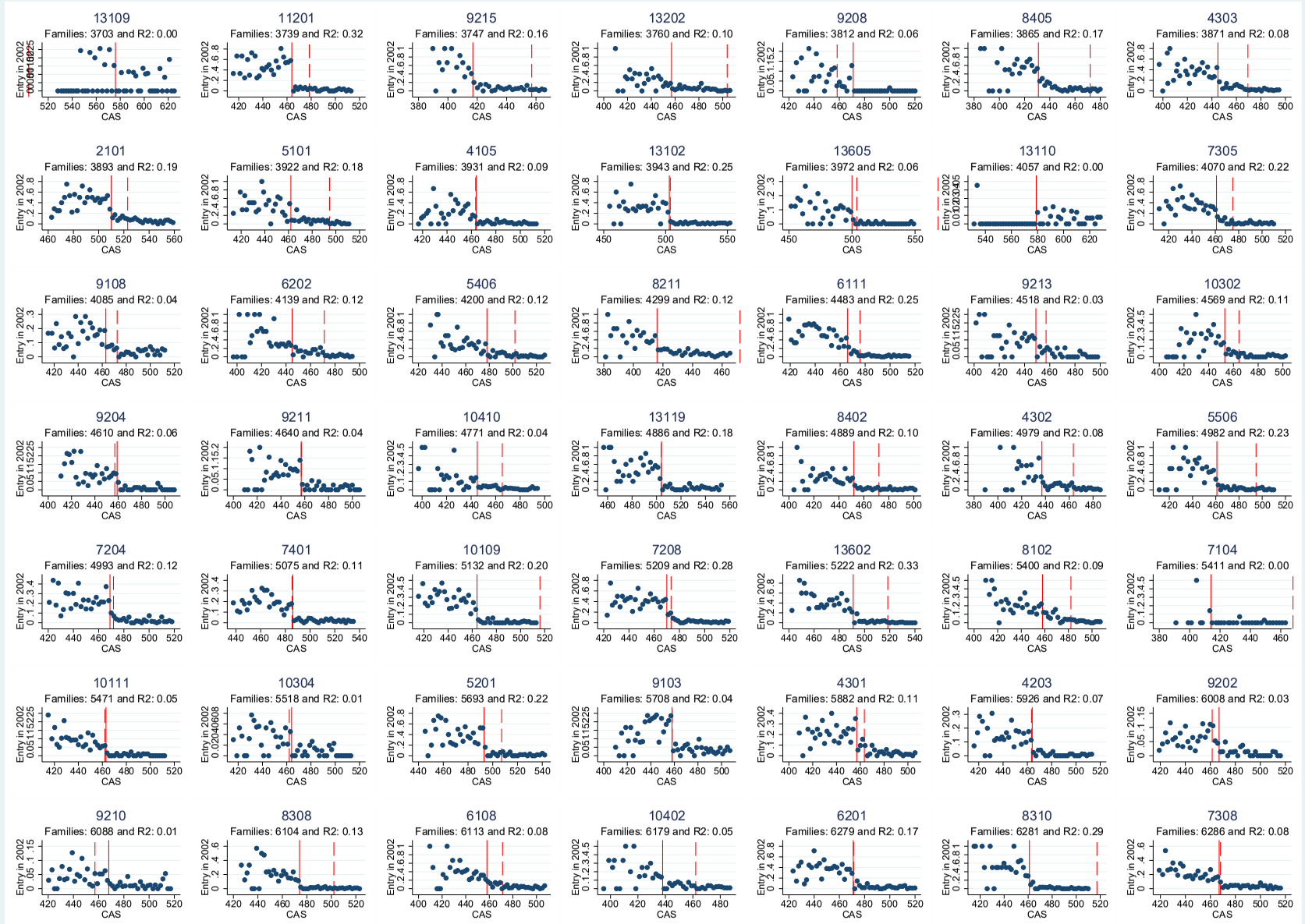
Participation in CS and *effective cutoff* and *official cutoff* in illustrative municipalities (cont.).



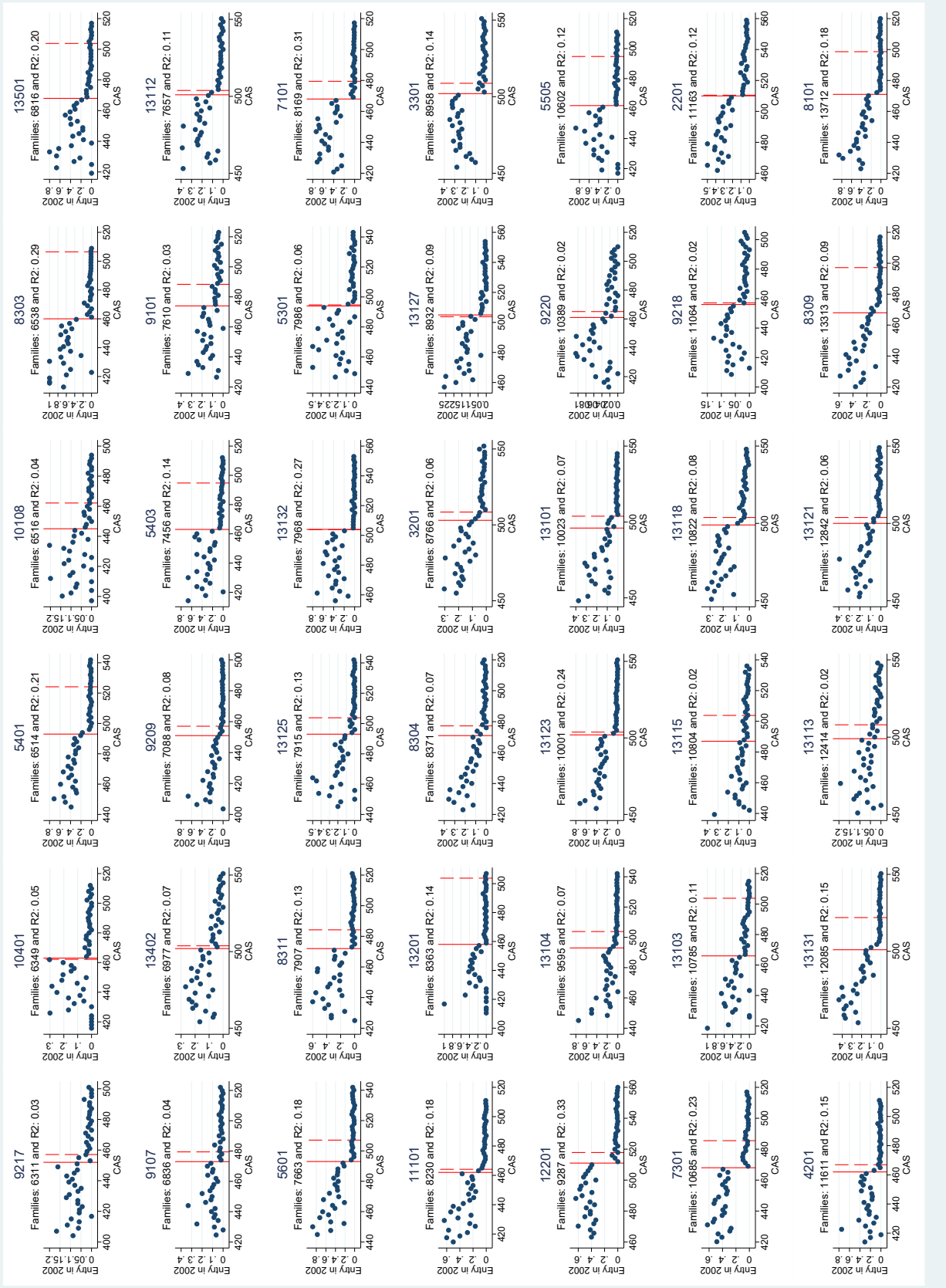
Participation in CS and *effective cutoff* and *official cutoff* in illustrative municipalities (cont.).



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



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



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

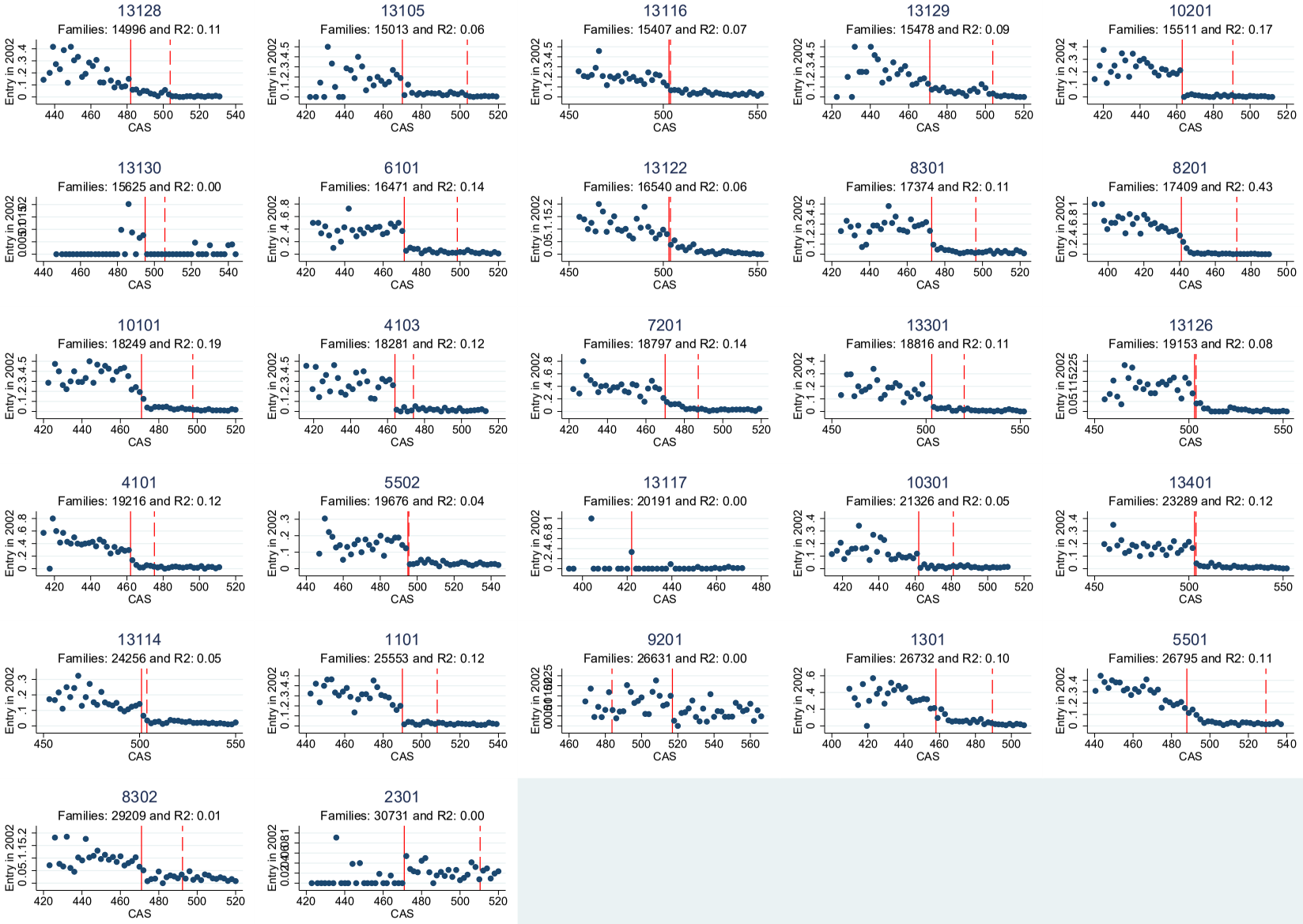
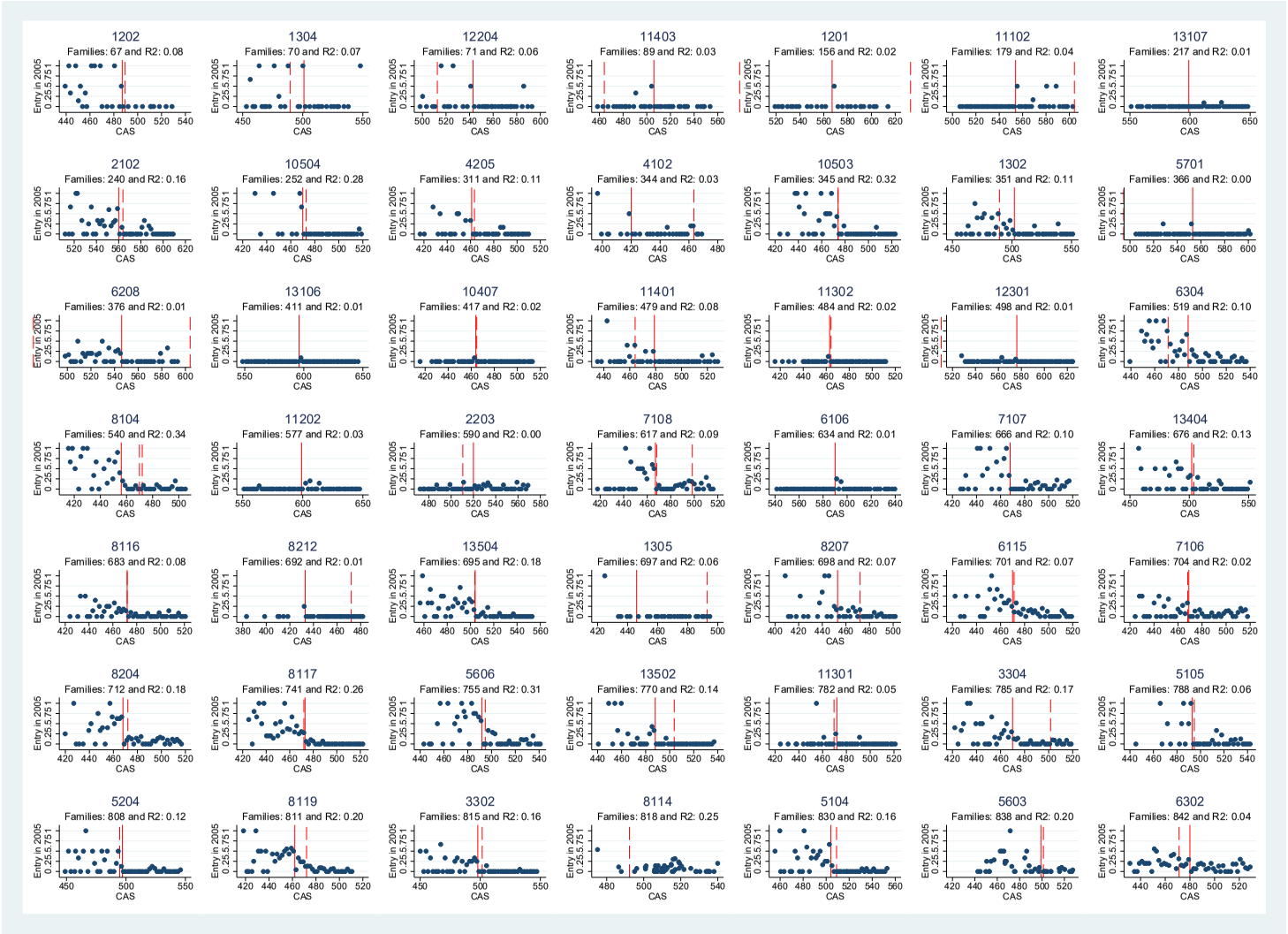
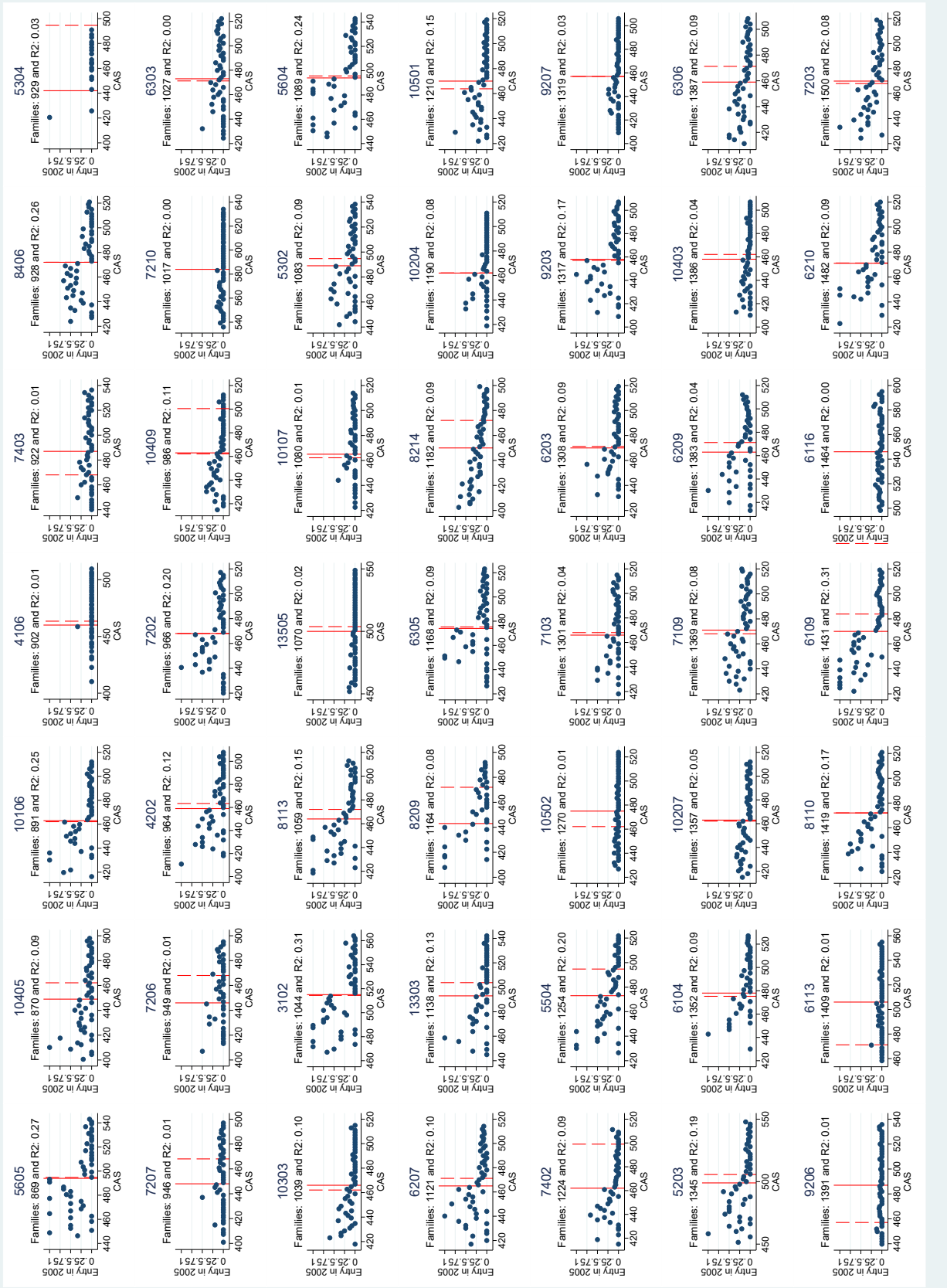


Figure C.2: Participation in CS and *effective cutoff* and *official cutoff* in illustrative municipalities (2005).

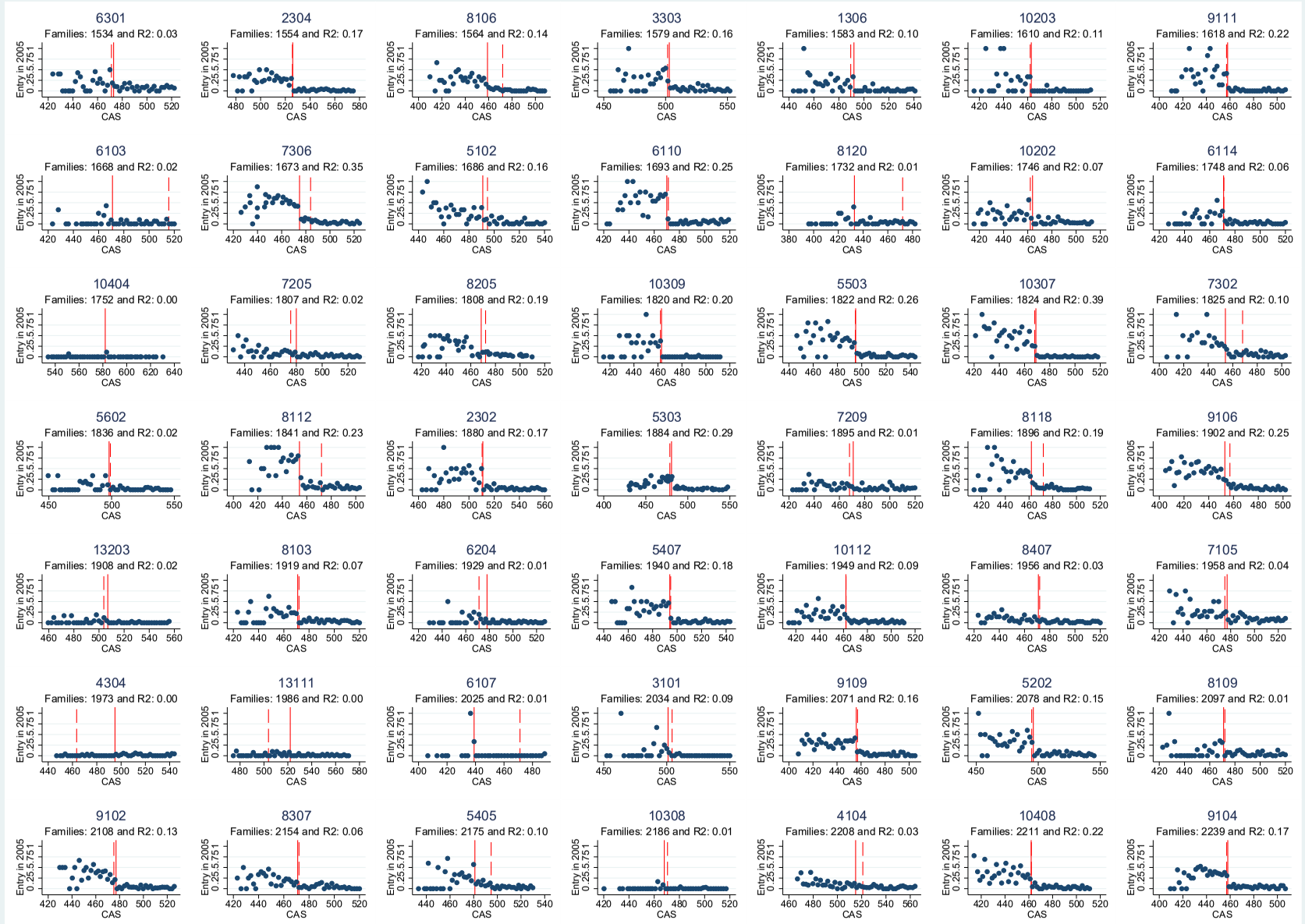


Note: The dots on the graphs are the proportion of families in municipality entering in CS in each year by intervals of CAS score of 4 points in 2005 in each municipality. The solid vertical line represents the point identified as *effective cutoff* and the dashed line is the *official cutoff*. Where only one line is shown, the two coincide. The graphs in the figure are ordered by increased size of municipality of residence in 2005. The R^2 in the top of each graph is the R^2 for the cutoff score of CAS that maximises the fit of equation (2) - the *effective cutoff*. The figures are zoomed around the *effective cutoff*, and only families at most 50-points apart from it are depicted (the CAS score varies between 380 and 770 points).

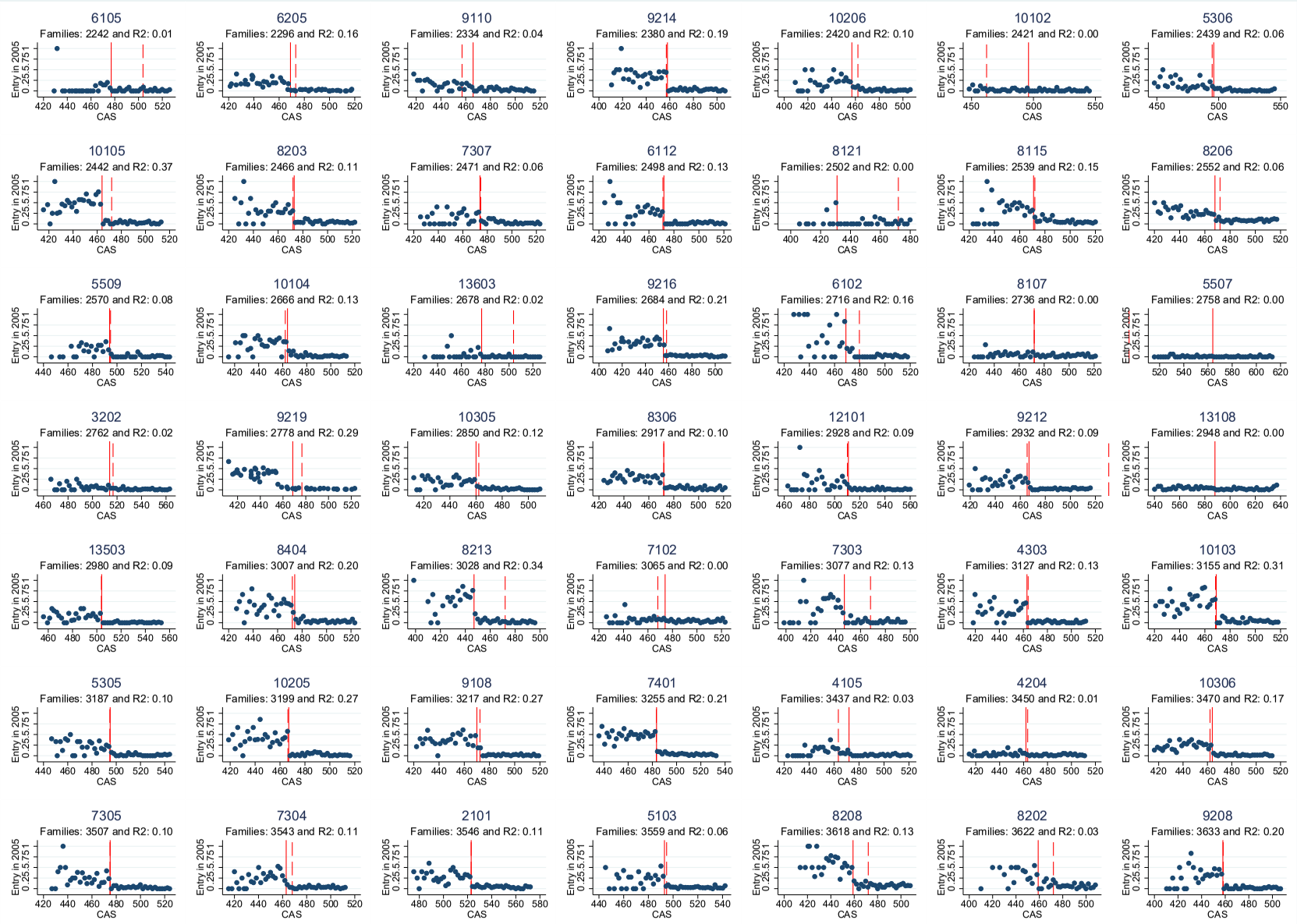
Participation in CS and *effective cutoff* and *official cutoff* in illustrative municipalities (cont.).



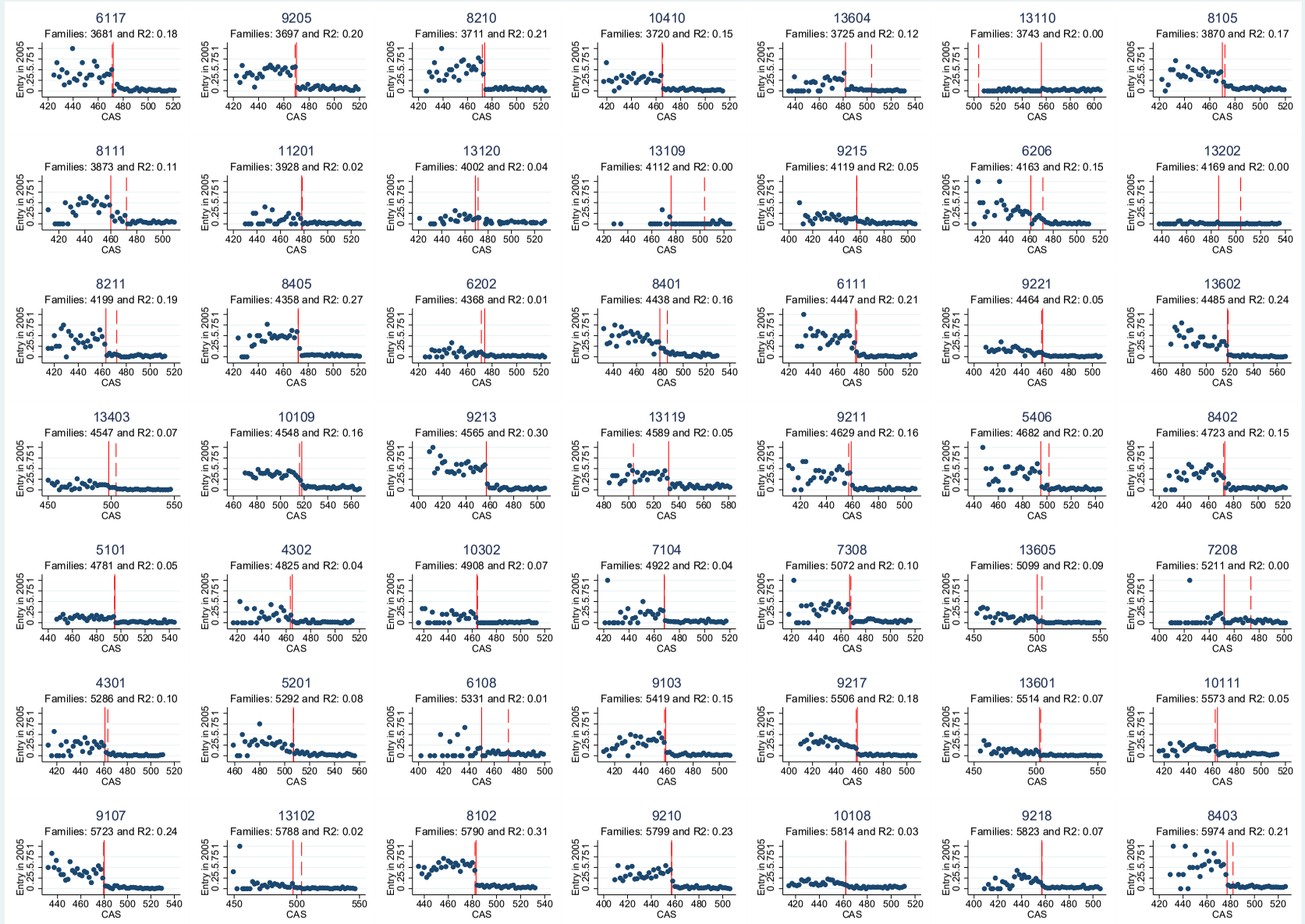
Participation in CS and *effective cutoff* and *official cutoff* in illustrative municipalities (cont.).



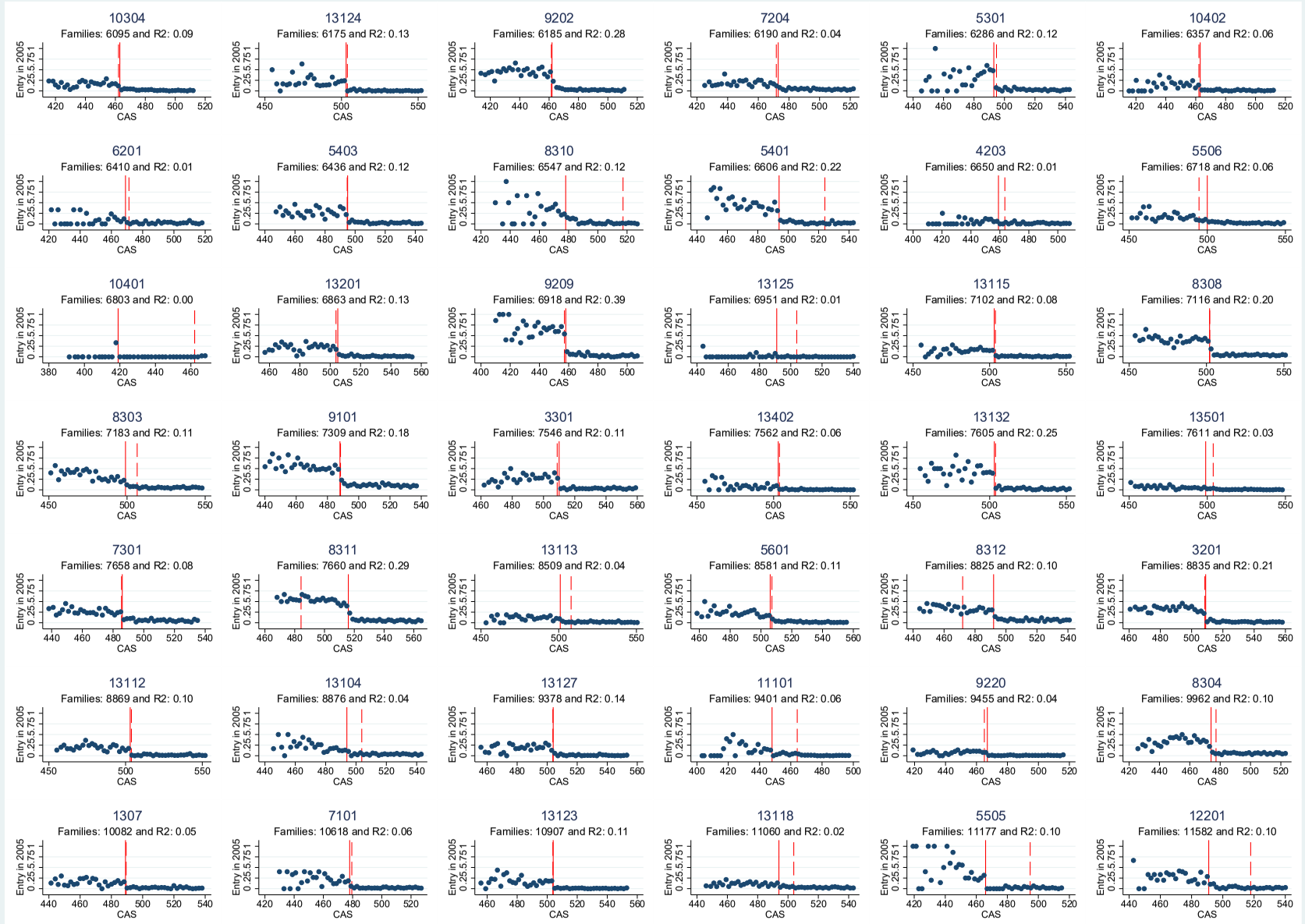
Participation in CS and *effective cutoff* and *official cutoff* in illustrative municipalities (cont.).



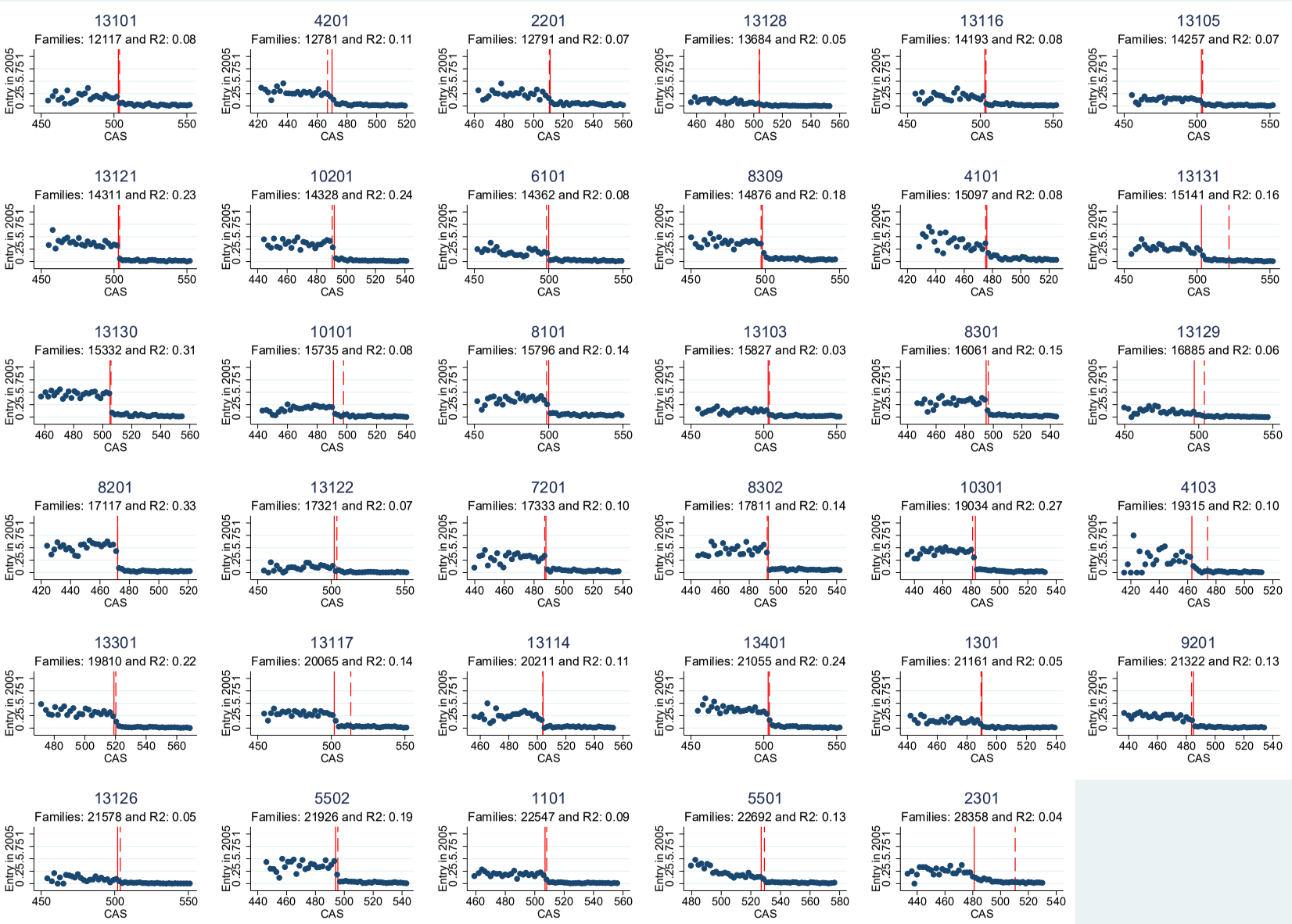
Participation in CS and *effective cutoff* and *official cutoff* in illustrative municipalities (cont.).



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



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



D Dynamic RD

One potential problem of equation (5) is that it ignores the evolution of the effective cutoffs within each municipality which followed the program roll out, and which means that families just ineligible in t may become eligible $t+1$ (equation (1)) is static, while selection into CS is sequential). One could think that, 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. However, it is also true that, over time, the take-up of CS among eligible individuals may also increase, which could mean that our static IV estimate of the impact of CS is too large.

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

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

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

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

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: $t = 1,2,3$, where CS_1, CS_2 and CS_3 are mutually exclusive. Then:

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

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

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

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

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

where we estimate β_1 from equation (8) and π^1 is a ratio of two first stage estimates: (i) the coefficient

on eligibility in $t=1$, E_1, from a regression of an indicator of entry in CS in $t=2$, CS_2, on eligibility in $t=1$, E_1, conditional on not having started CS in $t=1$, $CS_1=0$ (controlling for a function of CAS in $t=1$, which we omit above to simplify notation) and (ii) the coefficient on eligibility in $t=1$, E_1, from a regression of an indicator of entry in CS in $t=1$, CS_1, on eligibility in $t=1$, E_1 (controlling for a function of CAS in $t=1$, which we omit above to simplify notation).

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

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

Thus,

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

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

We present estimates for the βs in table D.1 and up to five years of exposure to CS. We do not present estimates for six years of exposure, because the estimation of π^1 in the case of β^6 is obtained from families entering in CS in 2007. However, in this paper we focus on the cohorts of 2002-2006, since from 2007 onwards the eligibility to the program was based on the FPS.

The coefficients of βs (adjusted) for the take-up of public subsidies are all within 90% of the θs (not adjusted) in table D.1. Table D.2 shows that π^1s are small in magnitude, and π^2, π^3 and π^4 are negative, therefore the estimates of the main results are largely unaffected by the dynamic entry into the program around the cutoff.

Table D.1: Dynamic Regression Discontinuity Estimates: 2SLS adjusted and unadjusted estimates for SUF (Cohorts 2002-2004).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years after start	2		3		4		5	
	Not adjusted	Adjusted	Not adjusted	Adjusted	Not adjusted	Adjusted	Not adjusted	Adjusted
	θ^2	β^2	θ^3	β^3	θ^4	β^4	θ^5	β^5
CS	0.083*	0.078	0.085*	0.096	0.167***	0.183	0.129**	0.176
	(0.043)		(0.048)		(0.047)		(0.052)	
90% CI	[.012;.154]	[.042;.141]	[.006;.164]	[.030;.144]	[.090;.244]	[.106;.306]	[.043;.215]	[.076;.300]
Sample	2004-2006		2005-2007		2006-2008		2007-2008	

Note: The table presents the estimated coefficients (and standard errors) for the indicator of entry in CS 2, 3, 5 and 5 years before the time at which outcome is measured in model 5. Controls excluded from table include quadratic in distance to cutoff, their interaction with the CS indicator and municipality-year effects. The municipality of residence and distance to cutoff are measured when eligibility is evaluated. In odd columns (1, 3, 5 and 7) we include robust standard errors are reported in parenthesis clustered at municipality of residence when eligibility is evaluated and the corresponding 90% confidence intervals; in even columns (2, 4, 6 and 8) we present the 90% confidence intervals obtained by block-bootstrap (the block is the family; 500 replications). * significant at 10%; ** significant at 5%; *** significant at 1%.

Table D.2: Estimates of the weights π used to obtain the adjusted estimates β in table D.1.

	(1)	(2)	(3)	(4)
Years after start	2	3	4	5
π^1	0.067	0.042	0.117	-0.008
	[.030;.114]	[.012;.072]	[.098;.149]	[-.044;.024]
π^2		-0.168	-0.182	-0.195
		[-.208;-.129]	[-.212;-.154]	[-.251;-.145]
π^3			-0.158	-0.175
			[-.196;-.123]	[-.214;-.135]
π^4				-0.151
				[-.175;-.112]

Note: The table presents estimates for the terms π used to obtained the estimates each β in the even columns of table D.1. In brackets we include 90% confidence intervals obtained by block-bootstrap (the block is the family; 500 replications).

E Data

CAS Consolidado covered about one-third of the Chilean population in each year between 2000 and 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 data include individuals surveyed between March 1998 and December 2009, covering over 14 million individuals (about 4 million families), corresponding to nearly 60 million observations). We have access to both the detailed information on the CAS and FPS forms, but also to the overall scores computed using that information. The scores are used to construct eligibility for CS and most targeted social programmes.[32]

E.1 The Ficha CAS and the CAS score

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

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

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

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

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

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

[32]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.

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 in the original database, since this was the year of transition to FPS and families surveyed in 2005 were excluded from the data; we consolidated this information adding to the CAS2006 those families surveyed during 2005). 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.

E.2 The Ficha de Proteccion Social

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

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

E.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)[33], to missing if it is too small (1000 or less) and flag observations with the same identifier[34]. 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

[33]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.

[34]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.

birth, region, province and municipality of residence, number of survey, relationship to head of family, name and surname and CAS are considered the same person, so we keep only one observation per year;

- As CAS index is assigned to the family, we dropped families with CAS varying within family;
- We found a few observations of heads of family whose parents or grandparents are younger than the head (on average 1500 out of 6 millions individuals per wave), which we flag but do not exclude from data given the small proportion of cases.

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

E.4 Administrative Data of CS Participants: The Puente Data Set

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

References

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