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ABSTRACT

Gender-Targeted Transfers and Women's Consumption: A Structural Regression Discontinuity Approach*

Whether gender-targeted cash transfers effectively redistribute resources to women and children in poor households remains an open question. We examine Uruguay's largest social assistance program, Asignaciones Familiares (AFAM), which is directed at poor families with children and paid to women. We estimate the intra-household distribution of resources and how it is discontinuously affected by AFAM eligibility. The regression discontinuity design (RDD) embedded in structural estimations points to a significant increase in resource shares for eligible women in rural areas–where traditional gender norms likely created greater margins for improvement. In contrast, children's resource shares are already substantial ex ante and do not increase further with AFAM. Translating these findings into individual poverty outcomes, we observe that while all family members benefit from the program's income effect, the bargaining effect leads to a greater reduction in poverty for women.

JEL Classification: D12, D13, C31, I32

Keywords: cash transfers, intra-household allocation, regression discontinuity, individual poverty

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1 Introduction

The assumption that targeting cash transfers to women benefits both them and their children has heavily influenced the design of conditional cash transfer (CCT) programs, particularly in Latin America, where payments are often directed to women (Handa et al., 2009). Early studies supporting this perspective primarily relied on correlations between individual expenditure data and women's control over income streams (e.g., Thomas, 1997; Hoddinott and Skoufias, 2004) or more rarely on an exogenous change in the nature of the benefit recipient (i.e., a 'wallet to purse' reform in Lundberg et al., 1997). Recent findings are more mitigated, suggesting that households' internal decision-making rules may undermine policies intended to target specific individuals. Some analyses have combined the estimation of structural models of intra-household resource allocation and randomized CCTs paid to women, such as PROGRESA (see in particular Bobonis, 2009; Tommasi, 2019; Sokullu and Valente, 2022; De Rock et al., 2022). This approach compares resource sharing between receiving households and control households, which makes it difficult to disentangle the pure bargaining effect of the transfer from its income effect and from the role of conditionalities. Other studies exploit recipient randomization, i.e., allocating transfers to either men or women within beneficiary households. This approach rules out income effects but not hard conditionalities (Akresh et al., 2016) or soft ones, i.e. labeling effects (Benhassine et al., 2015), and results are inconclusive. Targeted transfers appear to improve women's decision-making power in India (Almås et al., 2020) and Kenya (Haushofer and Shapiro, 2016), but not in Burkina Faso (Akresh et al., 2016) or Morocco (Benhassine et al., 2015). These studies find little evidence of positive effects on child outcomes.[1]

Against this background, the effectiveness of gender-based targeting must be further tested, particularly in realistic national contexts. Given the challenges of implementing large-scale randomized controlled trials (RCTs) on recipient identity, natural experiments represent a promising alternative. We contribute to this effort by examining the eligibility threshold of a transfer program targeted at women in Uruguay, the *Asignaciones Familiares* (AFAM). This setting offers several advantages. First, it constitutes a large-scale quasi-experiment: AFAM is the largest social assistance initiative in the country, covering around 10% of the population and a large fraction of poor households with children (Bérgolo and Cruces, 2021). Second, the program's eligibility score is based on predicted poverty levels as a function of household characteristics (a proxy means test). The strong enforcement of the eligibility rule by the government and the available evidence of non-manipulation of the assignment by applicants enable us to exploit the discontinuity at the cutoff to identify AFAM effects. Third, and most importantly, we propose the estimation of resource share allocations in Uruguayan households by leveraging the recent developments of the collective model approach (Bargain and Donni, 2012; Dunbar et al., 2013). The suggested method, based on simple assumptions and the observation of exclusive goods (e.g., clothing) for identification, leads to the estimation of the complete

[1] An exception is Armand et al., 2020, which shows that redistributing to poor Macedonian women tends to yield more nutritious diets for children. Lab-in-the-field also show mixed results, with no effect (Cherchye et al., 2021; Dimova et al., 2022; Ringdal and Sjursen, 2021) or mild evidence (Schürz, 2020) of redistribution towards children when women are offered extra resources.

consumption allocation among women, men and children within households. Thus, it becomes possible to calculate individual poverty levels and analyze how they vary with key determinants of the sharing rule and in particular AFAM eligibility. This way, our approach incorporates the spirit of a regression discontinuity design (RDD) within the resource sharing specification. Estimations are carried out on the Uruguay's National Expenditure Survey (ENGIH), which also serves to replicate the AFAM eligibility score. The eligibility dummy (treatment variable) along with alternative smooth functions of the score are introduced in the resource share specification of the structural model. This allows us to measure the intention-to-treat effect of AFAM on individual resources for women, men and children, and subsequently their poverty levels. Finally, our set-up addresses some of the past concerns: the structure of the model implicitly accounts for AFAM income effects, which traditionally limit the interpretation of results in studies comparing treated and control households. We also avoid the potentially confounding role of conditionality: we focus on families with children under 14 years old, for whom education requirements are almost never binding since Uruguay has already achieved near-universal enrollment at the primary and lower-secondary education levels (Cura and Capano, 2022).

Our findings indicate that AFAM reduces poverty among all person types but with a more pronounced effect for women, certainly due to the bargaining advantages generated by the gender-targeted nature of the transfer. While the effect on women's resource shares is mild overall, it is very significant in rural households. This pattern aligns with the observation that rural areas often adhere to more traditional gender norms, providing greater potential for improvements in women's empowerment. We find no effects of AFAM on children's shares. This is consistent with high child resource shares (low child poverty) ex ante, i.e. as measured on non-eligible households, and the fact that women prioritize their children's needs under normal circumstances, but may reallocate additional cash toward themselves when resources increase (Blow et al., 2012). This is in contrast with CCTs' stated objectives to benefit children indirectly through gender-targeted transfers.[2] Thus, among the different arguments for targeting women, empowerment seems to be the most compelling, rather than improving child outcomes. While one may refrain to generalize this conclusion to other, poorer settings, we emphasize the fact that it is obtained from the quasi-experimental analysis of a large-scale social transfer and in line with experimental settings that randomized the beneficiary's gender in a diversity of low- and middle-income countries (Almås et al., 2020; Haushofer and Shapiro, 2016; Akresh et al., 2016; Benhassine et al., 2015). Finally, our results are robust to alternative specifications (smooth function of the eligibility score, control variables of the sharing rule, etc.), alternative bandwidths around the cutoff,[3] and alternative specifications of the collective model (namely a general multi-adult family model, a nuclear household model in which bargaining effects are more easily interpretable, and a model treating child consumption as a public good for adults).

[2]Gender-based targeting is often motivated by the dual aims of promoting gender equality and redirecting resources towards children (Fiszbein and Schady, 2009; Duflo, 2012; Doepke and Tertilt, 2009; Dizon-Ross and Jayachandran, 2022).

[3]Note that optimal bandwidth approaches are not applicable here, since the dependent variable–resource sharing–is latent but not observed.

This paper makes several contributions to understanding the intra-household effects of gender-targeted cash transfers. *First*, the use of a structural model allows disentangling income and bargaining effects. While the income boost from the AFAM program is implicitly captured in household expenditure levels, the bargaining effect is explicitly modeled as a potential discontinuous shift in the resource share function. Consequently, our quasi-experimental results can be compared to studies where recipient identity is exogenously changed, either through policy reforms (e.g., Lundberg et al., 1997; Ward-Batts, 2008) or randomized experiments (e.g., Armand et al., 2020; Akresh et al., 2016; Benhassine et al., 2015; Haushofer and Shapiro, 2016; Almås et al., 2018). *Second*, we avoid the interpretation challenges commonly associated with conditionalities (e.g. in Akresh et al., 2016). In most studies evaluating CCTs, the conditionality associated with schooling may directly influence expenditure patterns through mechanisms such as nudging or preference shifts. In contrast, for the population we consider, the education requirement of the AFAM program is effectively neutral, so our set-up resembles analyses of universal or unconditional cash transfers (e.g., Haushofer and Shapiro, 2016). We nonetheless test for the presence of potential labeling effects (e.g. in Benhassine et al., 2015). *Third*, many existing studies are underpowered due to the relatively small benefit amounts they examine. For instance, the transfers studied in Lundberg et al. (1997), Benhassine et al. (2015) or Akresh et al. (2016) constitute only 3%, 5% and 4-5% of the mean income of poor (or beneficiary) households, respectively. By contrast, AFAM represents a more substantial transfer, approximately 16%-25% of pre-transfer mean income for recipients close to the eligibility threshold, and it is not time-limited.[4] *Fourth*, we contribute to the limited literature that combines resource share estimation with experimental or quasi-experimental designs. Some studies have evaluated the impact of PROGRESA in this way, as discussed above, or the effect of cash transfers using Difference-in-difference (DiD) and matching methods.[5] To the best of our knowledge, the present study is the first to embed a RDD within a model of intra-household resource allocation.[6] *Fifth*, our approach provides a relatively comprehensive view of resource allocation within households compared to earlier studies that focus on specific individual outcomes. For instance, some studies use female and child clothing directly as individual welfare proxies (e.g., Lundberg et al., 1997; Almås et al., 2020; Armand et al., 2020), while we employ clothing as an assignable good that allows completing the identification of the complete resource process. Other studies alternatively use individuals' survey responses to questions about decision-making (e.g. Lépine and Strobl, 2013; Sadania, 2016), sometimes for policy evaluations (e.g., De Brauw et al., 2014 for Bolsa Familia or Handa et al., 2009 for Progresa). A particularly relevant paper in that vein is Bérgolo and Galván (2018), where 'final say' questions are used as outcome of a RDD to assess the intra-household impact of AFAM. However,

[4]The transfers analyzed in Haushofer and Shapiro (2016) and Almås et al. (2018) were larger, accounting for around 37-40% of mean annual income, but were one-time payments rather than recurring transfers.

[5]For instance in Peru, India and Ethiopia (Borga and D'Ambrosio, 2020) or Argentina (Echeverría, 2020). Other papers combine DiD and structural models to look at gender-differential effects of shocks, for instance the mancession aspect of the Great Recession when the latter affected male sectors such as construction (Bargain and Martinoty, 2019).

[6]This approach combines the advantages of estimating the complete resource-sharing process, and hence individual poverty, with those of a quasi-experimental evaluation of the policy's impact on these outcomes. For other examples of structural models that incorporate the spirit of a RDD, see Duflo et al. (2012).

while this approach is insightful, it does not provide direct implications for individual poverty, as suggested here. Nevertheless, the convergence of results between both types of approaches – i.e. subjective measures of decision-making power and resource share estimations – is reassuring and, as shown hereafter, offers a valuable methodological cross-validation (see also Bargain et al., 2022).

The rest of the paper is structured as follows. Section 2 provides institutional background information on AFAM and describes the empirical strategy and data used in the estimation of intra-household resource allocation. Section 3 presents our main results on the effect of AFAM on resource shares as well as sensitivity analyses and implications for individual poverty. Section 4 concludes.

2 Empirical Strategy

2.1 Institutional Background

Uruguay is a high-income developing country in South America, with a population of 3.3 million people. Following the severe social and economic crises of the early 2000s, it has modernized the *Asignaciones Familiares del Plan de Equidad* (Family Allowance, AFAM for its acronym in Spanish), a CCT targeted at poor households with children. AFAM is currently the most important social assistance program in the country, both in terms of coverage (10% of the population, and 37% of households with children under 18) and budget (0.35% of GDP).[7]

AFAM Formula and Targeting. AFAM is proxy means-tested: households are eligible for AFAM payments if their poverty score–detailed hereafter–exceeds a defined threshold. During the study period, beneficiary households received a monthly cash transfer equivalent to USD 50 for the first child attending primary school and USD 70 for the first child attending secondary school.[8] To prevent undesired effects on fertility, an equivalence scale is applied to calculate benefits at the household level, which imposes diminishing returns to family size. Specifically, the transfer, expressed in USD, is calculated as:

$$AFAM = \alpha.(\#Children)^{\gamma} + \beta.(\#ChildrenSecondarySchool)^{\gamma}$$

with $\alpha \approx 50$, $\beta \approx 20$ and $\gamma = 0.6$ for the years studied. Note that the benefit is relatively generous, as the monthly minimum transfer (USD 50) represents around 36% of the World Bank's poverty line for Uruguay (USD 5.5 per day per person). Importantly, the transfer is assigned to the mother (or another woman in the household in her absence).[9] As discussed above, this targeting aligns with

[7]We refer to Amarante et al. (2010), Amarante and Vigorito (2012), and Bérgolo and Galván (2018) for a more complete description of the policy.

[8]The larger amount is seen as an extra incentive to enroll in secondary schools. The difference is not necessarily large enough, however, to study heterogeneity across family configurations, especially given sample size limitations.

[9]Exceptions include cases where there is no mother or female caregiver present in the household, such as single-parent households headed by men. These cases are rare and not considered in our empirical analysis. National statistics report that 93.3% of the benefit recipients were women during the years studied.

the program's objectives to empower women and enhance child welfare by entrusting women with the management of these funds. Whether it actually shifts intra-household allocation is the focus of our investigation.

Conditionalities. The program has two theoretical conditionalities: children's school attendance and regular health check-ups. The health conditionality has never been enforced (Amarante et al., 2010; Bérgolo et al., 2016; Rivero et al., 2020). For the education requirement, authorities verify school attendance once or twice a year for children in primary and secondary education. This conditionality is not binding in our sample. As justified below, our empirical work focuses on families with children under 14 years old. For these children, primary and lower secondary education is both free and compulsory. The country has made significant progress in school enrollment over recent decades, particularly following educational reforms and policy initiatives implemented in the early 2000s (Amarante et al., 2010). These efforts have resulted in very high enrollment rates for these age groups, namely 99% for children aged 6-11 years (primary school) and 98% for adolescents aged 12-14 years (lower secondary education). Past studies confirm that AFAM has no effect on school attendance within these age ranges (Bérgolo et al., 2016; Rivero et al., 2020).[10] Since the education conditionality is almost never a binding requirement for the population of families under study, we can interpret our results as if AFAM was a quasi-universal transfer. Nonetheless, we will check whether potential labeling effects are at play (see also Benhassine et al., 2015).

Eligibility Score. The eligibility rule is based on a proxy means test that entails computing a predicted poverty score for each applicant household. The score is calculated using their baseline socioeconomic characteristics at the time of application, which include the educational level of household adults, household assets (television, car or computer), the property and construction materials of the dwelling, overcrowding, sewerage, and household size. Importantly, the score formula is complex and not easily manipulated, enabling us to use it as a running variable for a RDD. The exact formula and weightings used in the score are actually not publicly disclosed to prevent manipulation–we have access to it thanks as members of the Instituto de Economá (IECON).[11] We nonetheless perform the usual manipulation tests hereafter. Households with a score above a certain threshold are eligible for the cash transfer, which creates a strong discontinuity in the probability of being assigned to the program.

Fuzzy Design. Note that mistargeting may occur, although its extent is expected to be limited (Amarante and Vigorito, 2012; Nicolau, 2023). Specifically, some eligible households may not receive the program because they did not apply. Non-take-up behavior is influenced by various factors, such as lack of awareness, perceived stigma, complex application procedures, or administrative barriers, which depend on unobserved household heterogeneity. To the extent that this heterogeneity is

[10] Positive effects are observed only for higher secondary schooling.

[11] IECON has actually been involved in the design of AFAM. The Institute frequently collaborates with the government agencies involved in the design and administration of social programs–including AFAM– namely the Banco de Previsión Social (BPS) and the Ministerio de Desarrollo Social (MIDES). These collaborations also include an access to detailed administrative data and joint methodological work.

correlated with intra-household decision-making processes, endogeneity may be a concern. Therefore, we use assignment to treatment, i.e., eligibility status, as our policy variable, rather than the treatment itself (benefit receipt). Not claiming benefits when eligible means that some households deemed poor are not recipients. Conversely, slow reassessments of eligibility scores mean that some households may have become nonpoor over time while still receiving AFAM. For both reasons, our setting corresponds to a fuzzy RDD. Consequently, our estimate will be interpreted as an *Intent-to-Treat* (ITT), which can be seen as a lower bound of the AFAM intra-household effect.

Income Effect. As a first check, we test whether the program generates a significant increase in household income in our data. We estimate a simple RDD using the log of total household income as the outcome and a smooth function of the running variable (poverty score) along with the eligibility status. Appendix Table A1 reports the coefficient associated with the eligibility dummy (ITT) for alternative specifications of the running variable's smooth function. We also present the coefficients from a model with heterogeneous effects for urban and rural households. The results indicate a positive and statistically significant average effect of AFAM on log household income, ranging from 19.7% to 33% across specifications. The effect is larger in rural households (25.8%-39.1%) but not statistically different from urban households. This suggests that poor rural and urban households do not have household compositions so distinct as to result in differing AFAM payments and income effects. Finally, we have argued that AFAM constitutes a substantial transfer, unlike many recent CCTs analyzed in the literature. In fact, our estimates indicate that for eligible households receiving AFAM, the transfer accounts for 16%-25% of their total resources.

2.2 Resource Allocation Model and Identification

Main Principles and Assumptions. The approach we use is inherited from the literature on collective models of household decision-making (Bourguignon and Chiappori, 1992). These models have been designed to account for the bargaining process underlying household decisions and, ultimately, to recover the intra-household resource allocation. The approach initially rested on the assumption that households make efficient decisions, which allows for the decentralization of the decision process leading to a sharing rule interpretation (Chiappori, 1988). Our setup builds on this tradition but does not necessarily need the efficiency assumption. As in recent studies (e.g., Bargain et al. 2022), we only need to assume that total expenditure is shared among household members according to some rule, which we identify and estimate.[12] In this literature, the most general framework rests on many years of expenditure data to identify resource allocation and economies of scale among childless couples (Browning et al., 2013) or couples with children (Bargain et al., 2023). Simpler approaches requiring only cross-sectional expenditure data have also been proposed to recover resource allocation in families with children (Bargain and Donni, 2012; Dunbar et al., 2013) and

[12]Note that the efficiency paradigm is the most commonly accepted way to justify decentralization, but it is probably not the only one supporting a sharing process. Efficiency is discussed extensively in the literature (Baland and Ziparo, 2017), and the type of method we use can actually be extended to settings with an explicit departure from efficiency (Lewbel and Pendakur, 2022).

possibly in complex, multi-adult families with children (see applications in Brown et al., 2021; Calvi et al., 2021; Penglase, 2021; Bargain et al., 2022). In this case, identification requires a first assumption known as *independence of the base* (IB hereafter), which states that the resource sharing function does not depend on total expenditure. This assumption has been tested – and not rejected – using direct observations of resource shares in Bargain et al. (2022) and Menon et al. (2012). A second assumption pertains to the observation of exclusive goods, i.e., goods consumed only by specific types of individuals (e.g., toys for children), or assignable goods, i.e. goods whose expenditure can be assigned to specific person types (e.g., male, female and child clothing). Identification hinges on alternative assumptions in terms of *preference stability* for these exclusive/assignable goods. The intuition is simple. For multi-person household, observed budget shares for these goods allow estimating person-specific Engel curves, e.g., women's clothing budget shares. If we can additionally recover some of its parameters thanks to preference stability assumptions, it may be possible to identify the location of this person on her Engel curve and, hence, her level of individual consumption.

Formal Set-up. Assuming the existence of a sharing rule that governs the distribution of resources in the household, we want to estimate how it potentially shifts with AFAM eligibility. Denote x the log of total private expenditure in the household and $\eta_{i,s}$ the share of total private expenditure $\exp(x)$ accruing to each person of type $i = f, m, c$, i.e. women, men and children, for a household of composition s. The composition corresponds to the number of individuals in each of the three person groups, denoted by s_f, s_m and s_c, respectively, and stacked in vector $s = (s_f, s_m, s_c)$. Resource shares $\eta_{i,s}(z^r, p)$ depend on several determinants including a vector z^r of socio-demographic characteristics and a policy variable p representing the AFAM eligbility score (but it does not vary with total expenditure, i.e., the IB assumption). Each person of type i in a family of composition s is endowed with an amount of private resources written in log terms as $x_{i,s} = x + \ln \eta_{i,s}(z^r, p)$. If identified, this individual resource level can be used to calculate person-specific poverty rates. Shares are identified only for broad person types (all men, women and children), not for specific individuals. This limitation arises from data availability but is not a major impediment since we focus on whether the bargaining power potentially associated with AFAM improves overall women's and children's resources.[13]

Individual and Household Engel Curves. We adopt a semi-parametric identification as in Dunbar et al. (2013). It is based on the assumption of Piglog indirect utility functions, which conveniently yield Engel curves that are linear in the log of total expenditure (see Deaton and Muellbauer 1980).

[13]A more granular sharing rule could be identified if we had more detailed exclusive/assignable goods. For instance, identifying resource shares of younger versus older children would require observing assignable goods for each of these subgroups. This data limitation is not a big issue for several reasons. First, as said, we are mainly interested by overall differences between male versus female versus child poverty here. Moreover, we can specify the sharing function in a heterogeneous way, for instance introducing children's age to check if their resource share tends to vary with age. Arguably, however, a limitation of this framework pertains to the interpretation of potential AFAM effects on women's share when there are several adult women in the household. To address this point, we will provide additional results for the subset of nuclear households with children (i.e., households where $s_f = 1$ and $s_m = 1$), which allows for more direct interpretations in terms of the potential bargaining effects of the cash transfer.

Specifically, the budget share of a good k consumed by a person i using her (log) personal resources $x_{i,s}$ can be written:

$$w_{i,s}^{k} = \delta_{i,s}(z^p) + \beta_{i,s}(z^p) \cdot x_{i,s}(z^r, p) \tag{1}$$

with preference shifters z^p and sharing rule determinants as indicated above. As a function of (log) individual expenditure, expression (1) defines *individual Engel* curves. We suppose we can observe exclusive/assignable goods, which we index k_c, k_f, k_m for goods specific to children, women, and men, respectively. For example, in a nuclear household, if k_f corresponds to woman's clothing, $w_{f,s}^{k_f}$ represents the proportion of her own resources spent on clothing. Multiplying this individual budget share by $\eta_{f,s} = \exp(x_{f,s})/\exp(x_s)$, we obtain female clothing expenditures as a fraction of total household expenditure, i.e., the *household* Engel curve $W_s^{k_f} = \eta_{f,s}.w_{f,s}^{k_f}$ for female clothing. Thus, we can write a system of household budget shares for exclusive/assignable goods k_i, $i = f, m, c$, as:

$$\begin{aligned} W_s^{k_f} &= \eta_{f,s}(z^r) \cdot (\delta_{f,s}(z^p) + \beta_{f,s}(z^p) \cdot (x + \ln \eta_{f,s}(z^r, p))) \\ W_s^{k_c} &= \eta_{c,s}(z^r) \cdot (\delta_{c,s}(z^p) + \beta_{c,s}(z^p) \cdot (x + \ln \eta_{c,s}(z^r, p))) \\ W_s^{k_m} &= \eta_{m,s}(z^r) \cdot (\delta_{m,s}(z^p) + \beta_{m,s}(z^p) \cdot (x + \ln \eta_{m,s}(z^r, p))) \end{aligned} \tag{2}$$

where the left-hand terms are observed in standard expenditure surveys.[14]

Preference Restrictions and Identification. The central question is whether we can retrieve key elements from the estimation of a reduced form of the above system, i.e., from the estimation of household budget shares on log expenditure. First, we may write men's resource shares as the complement to one of women's and children's shares, i.e., $\eta_{m,s} = 1 - \eta_{f,s} - \eta_{c,s}$. Next, we can write the derivatives of the system above with respect to log expenditure as:

$$\begin{aligned} \partial W_s^{k_f}/\partial x &= \eta_{f,s}(z^r, p) \cdot \beta_{f,s}(z^p) \\ \partial W_s^{k_c}/\partial x &= \eta_{c,s}(z^r, p) \cdot \beta_{c,s}(z^p) \\ \partial W_s^{k_m}/\partial x &= (1 - \eta_{f,s}(z^r, p) - \eta_{c,s}(z^r, p)) \cdot \beta_{m,s}(z^p) \end{aligned} \tag{3}$$

for each s out of a total of S different family compositions. The left-hand derivatives are observed, provided that household Engel curves are not flat and the IB assumption holds. In this case, the system above corresponds to $3S$ equations and $5S$ unknowns ($\eta_{f,s}, \eta_{c,s}$, $\beta_{f,s}$, $\beta_{c,s}$ and $\beta_{m,s}$ for each s). Thus, identification requires additional restrictions in the form of *preference stability* assumptions on the term β. We rely on the Similarity Across People (SAP) assumption suggested by Dunbar et al. (2013), which states that for exclusive/assignable goods, the shape of individual Engel curves is similar across persons $i = f, m, c$ within each household type s. Formally, SAP is written: $\beta_{f,s} = \beta_{m,s} = \beta_{c,s} = \beta_s$ for each s. This reduces the number of unknowns to $3S$ ($\eta_{f,s}$, $\eta_{c,s}$ and β_s for each s), enabling exact identification. Note that SAP is a commonly used preference restriction in the

[14] In households with multiple adult women, $\eta_{f,s}$ denotes the share of household resources allocated collectively to them. The per-women average share is then given by $\eta_{f,s}/s_f$.

demand literature and a weaker version of shape-invariance defined by Pendakur (1999) and Lewbel (2010). It is nonetheless a relatively strong assumption, although recent empirical verifications tend not to reject it.[15]

2.3 Specification of the Resource Allocation Model with an embedded RDD

Specification and RDD. In terms of specification, the semi-parametric approach suggested above provides a log-linear form for Engel curves (as derived from Piglog preferences and written in Equation 1). Additionally, we model resource shares using logistic functions to guarantee that the shares are below one and sum up to one. To estimate the model, we add error terms to household Engel curves for women's, men's and children's exclusive goods in the demand system (2), while imposing the SAP condition. Thus, we estimate the following system:

$$\begin{aligned} W_s^{k_f} &= \eta_{f,s}(z^r,p)\cdot(\delta_{f,s}(z^p)+\beta_s(z^p)(x+\ln\eta_{f,s}(z^r,p)))+\epsilon_{f,s} \\ W_s^{k_c} &= \eta_{c,s}(z^r,p)\cdot(\delta_{c,s}(z^p)+\beta_s(z^p)(x+\ln\eta_{c,s}(z^r,p)))+\epsilon_{c,s} \\ W_s^{k_m} &= \eta_{m,s}(z^r,p)\cdot(\delta_{m,s}(z^p)+\beta_s(z^p)(x+\ln\eta_{m,s}(z^r,p)))+\epsilon_{m,s} \end{aligned} \quad (4)$$

with $\eta_{f,s}=E_f/(1+E_f+E_c), \quad \eta_{c,s}=E_c/(1+E_f+E_c), \quad \eta_{m,s}=1/(1+E_f+E_c),$
$E_f=\exp(\alpha_f+\gamma_f z^r+\psi_f T(p)+\rho_f(p)), \quad E_c=\exp(\alpha_c+\gamma_c z^r+\psi_c T(p)+\rho_c(p)).$

Engel curve parameters $\delta(z^p)$ and $\beta(z^p)$ vary with preference shifters z^p, which include household composition (namely s_f, s_m, s_c) and an urban dummy. For the sharing rule, we specify the logistic form with a set z^r of variables equivalent to z^p (household composition and urban) plus child average age and the education levels of men and women. We also include information on employment, as explained below. Finally, and most importantly, the sharing rule depends on the AFAM eligibility score p in two ways to mirror a classic RDD specification. We include a binary variable for the assignment to treatment, $T(p)=1(p>\overline{p})$, which captures the potentially discontinuous effect associated with AFAM eligibility (the score is adjusted so that the threshold $\overline{p}$ is normalized to zero). We also incorporate a smooth function of the score, $\rho(p)$, and propose alternative specifications to verify that our conclusions do not depend on the chosen functional form. Estimations are carried out using a non-linear SUR method while addressing the usual concerns regarding the endogeneity of total expenditure, as detailed in Appendix I.

Potential Effects of AFAM. The structure of the model allows us to transparently discuss the different potential impacts of AFAM on individual consumption. *First*, there is the expected income effect, evidenced in Table A1. Members of an eligible household taking up AFAM will experience higher consumption levels, which is implicitly reflected in the total household expenditure x in the model. *Second*, the fact that the transfer is paid to women may result in a bargaining effect and intra-household redistribution, captured by the ψ coefficients in the model. As discussed, we model

[15] Bargain et al. (2022), using direct observations of resource shares, does not reject SAP for clothing. Indirect methods, such as those in Brown et al. (2021) and Dunbar et al. (2021), start from alternative identification approaches that do not require SAP and test it as a restriction.

only the impact of eligibility, which is possibly weaker than the actual receipt of AFAM transfers by women, but has the advantage of being entirely exogenous. *Third*, AFAM may entail labor market responses. Specifically, the potential wealth effect of AFAM could increase the likelihood of becoming inactive or engaging in informal work, with this effect being most pronounced among women. Two studies support this conclusion using RDDs around the eligibility threshold (Bérgolo and Cruces, 2021; Bérgolo and Galván, 2018).[16] If women reduce their labor market participation or earnings-particularly by shifting to the informal sector-this could weaken their bargaining power, potentially offsetting the direct benefits of AFAM on their control over household resources. We address this by including employment or, alternatively, earnings variables in the set of sharing rule determinants z^r. Precisely, we suggest three alternative measures: the proportion of formally employed women in the household, the overall proportion of formal workers in the household, or household formal income.[17] By including these controls, we assess whether movements of women (or other adult workers) out of formal employment near the eligibility threshold have indirect effects on individual consumption patterns.

2.4 Data and Selection

Expenditure Data. Our analysis is based on Uruguay's National Expenditure Survey (*Encuesta Nacional de Gasto e Ingreso de los Hogares*, ENGIH) provided by the National Institute of Statistics. It was conducted between November 2016 and November 2017 and is nationally representative, with a total sample of 6,889 households. Detailed information on socio-demographic characteristics, employment and income sources is collected. The expenditure survey is based on a 7-day booklet registering food and regular expenditures while longer-period expenditures are recorded through a specific recall questionnaire. We construct a measure of total household expenditure, which aggregates spending on food and non-food items.

Assignable Expenditure. We also retrieve spending on men's, women's and children's clothing for identification. Clothing expenses are recorded over a 3-month recall period. The use of clothing as the identifying good is pragmatic: this is one of the rare assignable goods commonly available in standard expenditure surveys. For this reason, it has been extensively used in the literature aiming to measure child costs using the Rothbarth approach (see Deaton 1997) or to estimate collective models of consumption with children (e.g., Bourguignon et al. 2009; Browning et al. 1994; Bargain and Donni 2012; Dunbar et al. 2013 among others). The use of clothing for resource share identification is

[16] In particular, Bérgolo and Cruces (2021) utilize labor administrative records and report a six percentage point reduction in registered employment among adult members of AFAM beneficiary households, particularly women. This reduction is associated with increased inactivity and informal work in equal proportions. The overall effect is primarily driven by single mothers: if the remainder of the effect were attributed to married women, it would correspond to a 3.9 percentage-point reduction in their registered employment.

[17] Incorporating these variables in the sharing rule assumes separability between consumption and labor supply decisions. A collective model addressing both dimensions simultaneously is beyond the scope of this paper; see the discussion in Bargain et al. (2023).

also supported by recent validation tests (Bargain et al., 2022).[18] Note that clothing is recorded as "children's clothing" for children under 14 years old, which set the age limit to define children in our empirical work.

Eligibility Score. All the variables required for the construction of the eligibility index p are included in the survey with the exact same wording as the application form. We use them to reproduce precisely the eligibility score. As described before, we have access, through IECON, to the exact formula, including weights and thresholds, which allows us to replicate the official rules used by the administration to determine eligibility. Eligible households as those with a positive score, i.e. $T = 1(p > 0)$ as posited in the model description (we adjust the score so that the official threshold is normalized to zero). Importantly, we compute the score for all households, regardless of whether they have applied to the program. As noted, some households with $T = 1$ may not receive AFAM if they have not claimed the benefit, while a few may still qualify despite a negative calculated score due to changes in key characteristics since their application

Sample Selection. Our ultimate objective is to assess the impact of AFAM on individual poverty within families. Selection must therefore strike a balance between representativity-ensuring the largest possible sample of households with children-and the constraints of our approach. Notably, children aged 14 or older cannot be distinguished from adults based on clothing expenditure. Consequently, we focus on households with at least one child under 14, resulting in an initial sample of n=2,185 households. We also exclude large families, i.e. households with more than 4 children (1% of the initial selection) and ignore single parents households or households where either adult men or adult women are absent (another 14.1%). Due to the difficult interpretation of intra-household redistribution between adults and children in households with older children, we necessarily exclude households with at least one teenager aged 14-17 (this important step takes another 22% of the initial selection away).[19] Male (female) children aged 18+ are treated as men (women) in the group $i = m$ ($i = f$) and not targeted by AFAM. Finally, we discard households for whom basic information is missing (e.g. income, assignable clothing, etc.), which represent less than 0.5% of the initial sample. Our final selection comprises 1,355 households, corresponding to 5,339 individuals and 4,065 individuals grouped into person types $i = f, m, c$. Alternatively, we shall focus on the subgroup of nuclear families, which implies an important reduction in sample size (selected n=984) but helps clarify who is impacted by the program. Selection steps are summarized in Appendix Table A2.

2.5 Descriptive Statistics and Descriptive RDDs

Main Variables. The mean and standard deviation of important variables are reported in Appendix Table A3, distinguishing the main selected sample (column 1) from the sub-sets of selected house-

[18] Alternative exclusive goods such as alcohol and tobacco pose problems of misreporting (Deaton, 1997) and generally do not allow the distinction between men and women. Individual expenditures on health or education are difficult to use in a collective model given the necessity to introduce time and risk dimensions.

[19] An alternative option would be to keep families with children aged 14-17 and treat the latter as adults. Such an approach would not change our main conclusions.

holds with one, two or more children respectively (columns 2, 3, and 4). The reported variables first include household socio-demographic characteristics, such as household composition, children's age, urban residence, and adult education, along with AFAM eligibility, defined by the eligibility dummy used as the key determinant of the sharing rule. AFAM eligibility covers 30% of the total sample and increases with family size. The reported rates closely align with those obtained from eligibility calculations based on administrative data available at IECON (Nicolau, 2023). We also report mean income and expenditure, followed by the average budget shares for male, female and child clothing. We observe that the presence of children reduces the budget devoted by adults to their own consumption. For instance, women with one child allocate 1.7% of household resources to their own clothing while this budget share decreases to 1.5% and 1.1% with the second and third child, respectively. This pattern is consistent with the Rothbarth's intuition as it reveals the resource shift towards children. We finally report the proportion of households with non-zero clothing expenditure. The infrequency of clothing purchases is not an issue for the estimation of our model (see Dunbar et al. 2013) but a reasonably low fraction of households with zero-clothing expenditure is reassuring. It is actually within reasonable bounds here, compared to recent studies (see the discussion Bargain et al. 2022) and given a recall period of 3 months.

Empowerment Statistics. We also examine potential heterogeneities in gender awareness and empowerment between rural and urban areas, which may both motivate and explain the heterogeneous effects of AFAM on intra-household resource distribution, as discussed later. Appendix Table A4 first presents indirect indicators of women's empowerment from our primary dataset (ENGIH 2016/17), including the couples' age ratio, women's income as a share of total household income, and women's labor market participation rate. We then provide additional statistics based on the 2018 Youth National Survey (ENAJ), again distinguishing between urban and rural households. These statistics capture women's own views on gender norms related to childcare and careers, as well as a summary gender role index. Across all measures, significant differences emerge in favor of urban women, reflecting both objective advantages-such as a larger age ratio-and more progressive attitudes. This aligns with anecdotal evidence suggesting that rural women have limited financial control, largely due to lower labor market participation and the traditional male-dominated structure of family production.[20] For these reasons, one might expect a greater potential for improvement, as AFAM grants women increased control over household finances. However, this remains an empirical question.[21]

RDD on Clothing Budget Shares. Before estimating the resource allocation model, we first conduct a reduced-form analysis of the effect of AFAM on clothing budget shares. While previous studies have used women's and children's relative clothing expenditures as proxies for welfare (Lundberg et al., 1997; Ward-Batts, 2008), our primary objective is to verify whether there is any significant variation around the eligibility threshold, given the central role of assignable clothing in our iden-

[20] See for instance: https://oig.cepal.org/en/countries/22/profile

[21] The opposite effect is also possible, if deeply rooted traditional gender norms render this control merely symbolic, with men ultimately determining how additional resources are allocated.

tification strategy. To do so, we estimate a simple RDD model, using the share of women's or children's clothing expenditures relative to men's as the main outcome variables. The model is specified with the AFAM eligibility dummy and alternative functional forms of the score. Appendix Table A5 presents the coefficient associated with eligibility status. Our findings provide quasi-systematic evidence of a positive jump in women's relative clothing expenditures across all specifications of the running variable. Consistent with our expectations, the effect is stronger in rural areas. In contrast, we find no effect for children. These results anticipate the conclusions drawn from the structural estimations.

3 Results

3.1 Baseline Estimates

Detailed estimates of the resource share function are reported in Appendix Table A6, considering various specifications of the determinants and using a linear spline function of the score. Model (2) is our baseline while model (1) a simplified version excluding adult education. The other variants are discussed later on. As specified, the baseline model includes the AFAM eligibility dummy (here reported as interacted with urban and rural for heterogeneous estimates), the function of the poverty score (not reported), household composition (i.e., the number of children, women, and men in the household), the average age of children, an urban dummy, and the mean education levels of men and women. The results confirm expected patterns: child (female) shares increase with the number of children (women) in the household, while women's resource shares rise with their relative education level. Most importantly, we can use these estimates to predict male, female and child resource shares for all households. Mean shares by person type are reported in Table 1. For verification, we also present male, female and child shares computed at sample mean,[22] enabling the calculation of standard errors via the delta method. Our findings indicate that children receive a relatively large share of household resources in Uruguay, while men's and women's shares are fairly balanced.[23]

The remainder of Table 1 examines the marginal effects of AFAM eligibility, reported across alternative specifications of the score. These effects are derived from two separate estimations: one that includes the AFAM eligibility dummy to estimate an overall effect, and another where this dummy is interacted with urban and rural indicators to capture heterogeneous effects. In both cases, we rely on the baseline specification of the resource share functions, as in Model (2) of Table A6. Our findings suggest a positive bargaining effect of AFAM on women's resource shares. However, this effect is statistically insignificant in most specifications of the running variable (except for the quadratic spline), which could be attributed to either limited statistical power or the anticipated absence of

[22] While these approaches could yield different results, given the non-linearity of resource share functions, they reassuringly lead to similar mean allocations.

[23] This aligns with international comparisons of intra-household distribution from Aminjonov et al. (2024), where Uruguay stands out as one of the least unequal countries among a broad set of developing nations.

an effect among urban households. By contrast, we observe a moderately significant effect for rural households, ranging from 6.1 to 7.4 percentage points across different specifications. In relative terms, this corresponds to a 20.1%-24.8% increase in women's resource shares. Such a redistributive effects on rural women is consistent with the reduced-form RDD estimates of clothing budget share ratios. It also aligns with the urban/rural differences in indirect gender empowerment and gender roles discussed previously. Note that the estimated bargaining effect represents an ITT effect and is likely a lower bound. While it is challenging to infer the ATT, non-take-up appears to be the primary source of mistargeting, affecting approximately 29% of all eligible households (Ghazarian, 2021; Nicolau, 2023). An upper bound for the ATT can therefore be approximated as the ITT divided by $(1 - 29\%)$, resulting in an estimated effect ranging from 28.3% to 35%.

A significant increase in women's shares means an equivalent decrease in cumulated male and children's shares. In Table 1, we cannot determine whether one group loses more than the other to account for women's gains: the effect of AFAM eligibility is negative but insignificant for both male and child shares. It is important to note that these effects pertain to resource shares, not resource levels: men and children still benefit significantly from the overall income effect of AFAM. The additional bargaining effect simply implies that women benefit disproportionately. That this bargaining effect does not extend to children is unsurprising. First, we have argued that the transfer is effectively unconditional for children under 14. Second, children in Uruguay already receive a substantial share of household resources (Aminjonov et al., 2024). Calculating per-person resource shares, we find an average of 28% per child compared to 25% per woman, meaning that women are relatively poorer than children–especially when considering larger adult needs. It is therefore plausible that women may "catch up" when additional income, such as AFAM funds, becomes available. Third, our findings align with previous evaluations showing no impact of AFAM on child nutrition (Bérgolo et al., 2016) or expenditures on child-related goods (Rivero et al., 2020). Fourth, even in contexts where child poverty is more pronounced, recent experimental evidence suggests that increasing women's bargaining power does not necessarily translate into higher spending on children (Almås et al., 2020; Akresh et al., 2016; Benhassine et al., 2015; Haushofer and Shapiro, 2016)

Table 1: Marginal Effects of AFAM Eligibility on Children, Women, and Men Resource Shares

	Children	Women	Men
Estimated resource share (linear spline smooth function of score)			
Mean shares over all households	0.44	0.30	0.31
Shares at sample mean	0.41	0.30	0.30
	(0.067)	(0.055)	(0.058)
Coeff. on eligibility dummy (for alternative smooth functions of score)			
All households			
Quadratic	-0.026	0.021	0.005
	(0.000)	(0.027)	(0.034)
Cubic	-0.005	0.031	-0.026
	(0.037)	(0.030)	(0.040)
Quartic	-0.042	0.032	0.009
	(0.041)	(0.034)	(0.044)
Spline	-0.016	0.017	-0.002
	(0.027)	(0.025)	(0.017)
Quadratic spline	-0.032	0.046 *	-0.014
	(0.033)	(0.027)	(0.031)
Rural households			
Quadratic	-0.035	0.061 *	-0.026
	(0.038)	(0.036)	(0.037)
Cubic	-0.019	0.074 *	-0.054
	(0.039)	(0.039)	(0.042)
Quartic	-0.048	0.072 *	-0.025
	(0.043)	(0.040)	(0.044)
Spline	-0.025	0.063 **	-0.038
	(0.033)	(0.032)	(0.030)
Quadratic spline	-0.039	0.074 **	-0.035
	(0.040)	(0.035)	(0.033)
Urban households			
Quadratic	-0.022	-0.011	0.033
	(0.038)	(0.031)	(0.038)
Cubic	0.001	-0.001	0.000
	(0.040)	(0.034)	(0.043)
Quartic	-0.036	-0.006	0.043
	(0.045)	(0.038)	(0.048)
Spline	-0.013	-0.011	0.024
	(0.032)	(0.025)	(0.032)
Quadratic spline	-0.032	0.007	0.025
	(0.038)	(0.036)	(0.028)
Observations		1355	

Source: authors' estimations using the Uruguayan household expenditure survey (ENGIH 2016/2017). Notes: The table first reports estimated resource shares for all children, women and men in selected samples, computed at household level and averaged over all households, or alternatively, computed at sample means to calculate standard errors (delta method). We then present marginal effects of AFAM eligibility (poverty score over zero) on resource shares of children, women, and men, evaluated at sample mean for each group (all households, rural households or urban households). Urban households refer to localities with more than 5,000 inhabitants. Standard errors in parentheses. *, **, *** indicate 10%, 5% and 1% significance level.

3.2 Robustness Analyses and Additional Results

Manipulation and Balance Tests. Manipulation occurs when potential beneficiaries deliberately alter their poverty score to influence program eligibility. However, as previously discussed, this scenario is highly unlikely. The score formula is complex and incorporates several structural variables, making direct manipulation by beneficiaries virtually impossible. Additionally, neither the score itself nor the eligibility cutoff was publicly disclosed. Nonetheless, we apply the Cattaneo et al. (2018) test, which utilizes a local polynomial density estimator without requiring data prebinning. The results, presented in Appendix Figure A1, provide no statistical evidence of systematic manipulation of the running variable p, overall and for urban or rural households. Specifically, we find no abnormal density concentration just above the cutoff. The test for discontinuous density yields a p-value of zero, reinforcing the conclusion that manipulation is not a concern. Another relevant aspect is that the outcome variable–resource shares–is not directly observable. Thus, we must examine whether the covariates of the resource share function exhibit any discontinuities around the cutoff, as such jumps could interfere with the estimated discontinuous effect of AFAM eligibility.Appendix Figure A2 provides both graphical and statistical evidence from balance tests. Reassuringly, we find no significant discontinuities at the cutoff for the main explanatory variables used in the model.

Alternative specifications. We assess the robustness of our results by testing different specifications of the resource share function. Appendix Table A6 reports estimates of models with alternative sets of determinants z^r, using a baseline spline function for the poverty score. Our findings indicate that adult education did not interfere with the type of empowerment provided by AFAM eligibility (Model 1 vs. Model 2), particularly regarding the positive effect on women's resource shares in rural households. Models (3)-(5) further demonstrate that the results remain robust when incorporating variables related to formal labor. As previously noted, Bérgolo and Cruces (2021) found that AFAM had a negative effect on women's formal employment. If lower formal employment weakens women's bargaining power, we would expect this to attenuate the bargaining effect observed in Model (2). Consequently, controlling for formal employment in Model (3) should increase the AFAM eligibility coefficient on women's resource share. However, our results do not support this hypothesis: the AFAM coefficient slightly decreases, and the formal employment variable-whether for women or overall-does not appear to influence resource shares significantly. In contrast, we find statistically significant effects for the formal income channel. Specifically, as household income increases, women's shares rise, while children's shares decline. A possible explanation is that children represent a necessary good: poorer households allocate a disproportionately large share of resources to children to ensure they meet a minimum consumption level. As formal income grows, households no longer need to dedicate a large share of resources to maintain children's well-being. In this sensitivity analysis, the bargaining effect of AFAM results in an increase in rural women's resource shares by 5.1 to 6.6 percentage points, i.e. a 17% to 22% rise.

Alternative Bandwidths. Since our outcome is not directly observable, RDDs with an optimal bandwidth (Calonico et al., 2020) are not applicable. Instead, we assess the robustness of our results to different bandwidth choices. Beginning with the broadest bandwidth, covering the entire selected sample, we progressively narrow the sample symmetrically around a zero score, reducing the sample size by 10% at each step until it is halved (reducing further would not be reasonable in terms of sample size restriction). Appendix Table A7 presents the marginal effects of AFAM on intra-household resource distribution in rural and urban households across these bandwidths.[24] Reassuringly, the results remain consistent across different bandwidths, with the bargaining effect ranging from 6.3 to 8.1 percentage points, corresponding to a rise of 21% to 27%.

Alternative Sample and Model: Nuclear Households Our baseline model was broad enough to encompass all types of households, including families with multiple men and women beyond the nuclear couple (as seen in Bargain et al., 2022; Calvi, 2020; Aminjonov et al., 2024, for instance). However, interpreting bargaining effects in these multi-adult families may be somewhat more complex than in the case of a single nuclear couple living with their children.[25] Thus, we now focus on nuclear households with children, where the bargaining effect can be fully attributed to the mother. Beyond offering a clearer interpretation of the empowerment effect, this approach also enhances comparability, as most prior research on the intra-household effects of CCTs has concentrated on nuclear households (e.g., Sokullu and Valente 2022 and Tommasi 2019 for PROGRESA). The downside is a substantial reduction in sample size (approximately -27%, resulting in $n = 984$). The model remains similar to the previous one but is now restricted to $s_f = s_m = 1$. The marginal effects of AFAM eligibility are reported in the first three columns of Table 2, across alternative poverty score specifications. We still observe only limited evidence of an overall effect, with a significant increase in women's share appearing only under the spline and quadratic spline specifications of the score. However, the effects are more pronounced for rural households. What becomes particularly clear in this nuclear family context is that women's gains are not offset by losses shared equally between men and children but rather by a marked decline in male shares across most specifications.[26] In terms of magnitude, the AFAM bargaining effect ranges from 6.7 to 12.4 percentage points, corresponding to an increase in the female share of 18.1%-33.4%, depending on the specification. Sensitivity analyses–conducted using alternative specifications of the resource-sharing function (Appendix Table A8) and different bandwidths (Appendix Table A9)–confirm this order of magnitude.

[24] Results are based on the baseline specification, i.e. a linear spline form of the score and the standard set of resource share determinants. Similar patterns emerge when using alternative functional forms and alternative sets of determinants (as in Appendix Table A6).

[25] Specifically, it is uncertain whether the woman receiving AFAM redistributes resources to all the women in the family–she may actually allocate part of her resources to other men such as her adult sons, for instance.

[26] This aligns with Tommasi (2019), who found positive effects on women's share and negative effects on men's share in PROGRESA villages when focusing on nuclear households.

Table 2: Marginal Effects of AFAM Eligibility on Children, Women, and Men's Resource Shares (Nuclear Households)

Coeff. on eligibility dummy (for alternative smooth functions of score)	Nuclear households			Nuclear & children as public goods	
	Children	Women	Men	Women	Men
All nuclear households					
Quadratic	0.005	0.026	-0.031	0.062	-0.062
	(0.040)	(0.035)	(0.030)	(0.044)	(0.044)
Cubic	0.012	0.021	-0.034	0.061	-0.061
	(0.042)	(0.038)	(0.035)	(0.049)	(0.049)
Quartic	-0.019	0.041	-0.022	0.053	-0.053
	(0.049)	(0.044)	(0.038)	(0.056)	(0.056)
Spline	-0.015	0.050 *	-0.035	0.068 **	-0.068 **
	(0.034)	(0.027)	(0.027)	(0.034)	(0.034)
Quadratic spline	-0.052	0.072 *	-0.020	0.060	-0.060
	(0.044)	(0.039)	(0.027)	(0.041)	(0.041)
Rural nuclear households					
Quadratic	-0.010	0.073 *	-0.063 *	0.102 **	-0.102 **
	(0.043)	(0.040)	(0.036)	(0.047)	(0.047)
Cubic	-0.005	0.067 *	-0.062	0.094 *	-0.094 *
	(0.044)	(0.042)	(0.040)	(0.051)	(0.051)
Quartic	-0.028	0.079 *	-0.052	0.084	-0.084
	(0.049)	(0.046)	(0.045)	(0.057)	(0.057)
Spline	-0.042	0.108 ***	-0.066 **	0.117 ***	-0.117 ***
	(0.038)	(0.036)	(0.031)	(0.037)	(0.037)
Quadratic spline	-0.069	0.124 ***	-0.055 *	0.107 **	-0.107 **
	(0.047)	(0.045)	(0.034)	(0.044)	(0.044)
Urban nuclear households					
Quadratic	0.032	-0.030	-0.001	-0.010	0.010
	(0.049)	(0.043)	(0.025)	(0.052)	(0.052)
Cubic	0.037	-0.036	-0.001	-0.021	0.021
	(0.051)	(0.046)	(0.028)	(0.056)	(0.056)
Quartic	0.010	-0.021	0.010	-0.032	0.032
	(0.059)	(0.053)	(0.032)	(0.061)	(0.061)
Spline	0.006	-0.004	-0.002	0.001	-0.001
	(0.042)	(0.038)	(0.035)	(0.043)	(0.043)
Quadratic spline	-0.023	0.016	0.007	-0.009	0.009
	(0.042)	(0.045)	(0.025)	(0.048)	(0.048)
Observations		984		984	

Source: authors' estimations using the Uruguayan household expenditure survey (ENGIH 2016/2017). Notes: focusing on the sample of nuclear households, i.e. couples living with children but no other adults, the table presents marginal effects of AFAM eligibility (poverty score over zero) on resource shares of children, women, and men, evaluated at sample mean for each group (all nuclear households, rural nuclear households or urban nuclear households). Urban households refer to localities with more than 5,000 inhabitants. Standard errors in parentheses. *, **, *** indicate 10%, 5% and 1% significance level.

We also propose a variant in which children are treated as a public good for the nuclear parents. This model, detailed in Appendix II and inspired by Blundell et al. (2005), allows us to focus solely on the implicit transfer between adults. Here, women's gains in resource shares are exactly offset by men's losses, as shown in the last two columns of Table 2. We estimate an AFAM bargaining effect ranging from 8.4 to 11.7 percentage points. Compared to the average female share (58.3% of the couple's resources), this implies an increase of 14.5%-20.1% across score specifications. We find an increase of 13.5%-22% across broader robustness checks (Appendix Tables A8 and A9), which remains broadly consistent with the baseline results.

Cross-validation with Final Say Variables. Subjective decision-power variables can serve as a useful benchmark for assessing the bargaining effect induced by AFAM eligibility. We build on Bérgolo and Galván (2018), who employ a RDD using data from the *Encuesta de Seguimiento de Condiciones de Vida*, a survey specifically designed to evaluate AFAM's impact. Analyzing responses to final say questions on food expenditures on nuclear households, they find evidence of an AFAM effect on women's consumption decisions within the household. Specifically, food decisions made by men or others (i.e., not made by women) decrease by 15-22% across score specifications, while decisions made solely by the wife increase by 51-56%. Magnitudes are not expected to be directly comparable to our results for several reasons discussed by Bérgolo and Galván (2018), including the fact that food constitutes only a part of household budgets and may be more responsive than other goods to women's control over money–especially if final-say measures reflect not only genuine shifts in power but also some degree of delegated decision-making (Baland and Ziparo, 2017). Nonetheless, the observed dynamics are similar and suggest a convergence between objective, consumption-based responses and subjective final-say responses.

Potential Labeling Effects. Even when conditionality is not binding, the mere fact that AFAM is presented as a redistribution to families with children can act as a form of nudging for parents. The labeling of cash transfers is often considered a soft form of conditionality and can be just as effective, as shown by Benhassine et al. (2015). In our case, this mechanism might explain a redistribution towards children, which, however, we do not observe. Still, it is reasonable to assume that reallocating resources to children may not be detectable overall–or through identifying goods such as clothing–but could occur for specific expenditures of primary importance to parents–most notably, ensuring quality education for their children. Given the very high enrollment rates discussed earlier, such parental decisions might affect other educational margins, such as spending on school supplies or private school tuition. To investigate this, we conduct a RDD analysis using the household budget share allocated to these educational expenses as the outcome variable, controlling for household income to neutralize AFAM income effects.[27] The results, reported in Appendix Table A10, show no discontinuous increase in the budget share allocated to these education expenditures as a result of AFAM eligibility. The absence of redistribution towards children in this dimension aligns with our findings on intra-household consumption. It is also consistent with the broader context: while

[27]Note that private education in Uruguay is relatively limited. In our data, 18% of households use private schools, but this share drops to only 4% among AFAM-eligible households.

the program is labeled as a "child transfer"–a framing that conditionalities could reinforce–it was not accompanied by specific education-related interventions or behavioral guidance.[28] Finally, the issue of conditionality is linked to potential endogeneity concerns, which could arise if unobserved preferences for schooling were correlated with unobserved preferences for the assignable good (see Tommasi, 2019). However, since conditionality is not binding and our analysis is restricted to households with children under 14, this should not be a concern. Moreover, this final robustness check on education expenditures provides additional reassurance in this regard.

3.3 Implications for Individual Poverty

Having estimated individual resource shares, we can calculate individual expenditure *levels* for children, women, and men to assess their respective poverty rates. Individual expenditure is calculated as the product of total household expenditure and the estimated individual resource shares, i.e. in log: $x_{i,s} = x + \ln \eta_{i,s}(z^r, p)$. We then compare these values to poverty thresholds, using \$5.5 per person per day, i.e. the World Bank's poverty line for Uruguay (2011 PPP). Table 3 reports individual poverty levels for children, women, and men for AFAM-eligible households. The baseline scenario ("AFAM: total effect") assumes that all these households receive AFAM. As previously discussed, poorer households tend to redistribute resources toward children, resulting in lower child poverty rates compared to adults. To better understand AFAM's impact on individual poverty, we first construct a counterfactual scenario in which both its income and bargaining effects are neutralized. Compared to this counterfactual, poverty in the baseline scenario (with AFAM) decreases by 35% for women, 20% for children, and 15% for men. This total poverty reduction combines both income and bargaining effects, which explains the larger reduction for women. To disentangle these effects, we isolate each component by either removing AFAM from individual expenditure (income effect) or canceling AFAM's impact on estimated resource shares (bargaining effect). We perform a Shapley decomposition, as proposed by Shorrocks (1999), averaging the contributions of each effect over the two possible sequences (i.e. introducing income effect first or bargaining effect first). Table 3 shows that, as expected, the total change in poverty for men and children results from a combination of a positive income effect and a negative bargaining effect. For women, both effects contribute significantly and nearly equally, with 56% of the poverty reduction attributed to the income effect and 44% to the bargaining effect.

[28] Other studies suggest that most eligible households are not even aware of the conditionalities (Bérgolo et al., 2016).

Table 3: Children, Women, and Men Individual Poverty

	Children	Women	Men
Counterfactual without AFAM	0.070	0.214	0.191
	(0.255)	(0.411)	(0.394)
AFAM: income effect	0.051	0.158	0.163
	(0.221)	(0.366)	(0.370)
AFAM: bargaining effect	0.088	0.167	0.200
	(0.284)	(0.374)	(0.401)
AFAM: total effect	0.056	0.140	0.163
	(0.230)	(0.347)	(0.370)
Total change in poverty rate	-20%	-35%	-15%
Contribution (Shapley decomposition):			
income effect	183%	56%	117%
bargaining effect	-83%	44%	-17%

Source: authors' estimations using the Uruguayan household expenditure survey (ENGIH 2016/2017). Notes: the table reports individual poverty rates of men, women, and children for the sample of AFAM eligible households Individual poverty rates are based on predicted resource shares for women, men, and children and World Bank poverty line for high-income country. For counterfactual estimation of poverty without AFAM, we neutralize the AFAM income from the total household expenditure as well as the AFAM eligibility effect in the resource share function. To account for income or bargaining effect, we relax the former or latter neutralization respectively. Standard deviations in parentheses.

4 Concluding Discussion

Conditional cash transfers have been targeted to women in Latin America and other parts of the world with the explicit goal of influencing the intra-household distribution of resources and alleviating child poverty. Most prior research has focused on specific individual expenditure items, used as welfare proxies, or final say questions related to decision-making. In this paper, we examine potential shifts in household consumption allocation induced by AFAM, the largest cash transfer program in Uruguay, which is targeted at women. We integrate a fuzzy regression discontinuity design (RDD) into the structural estimation of household sharing rules to estimate the program's intention-to-treat effects on the resource shares of men, women, and children, as well as their respective poverty rates. Given that AFAM's conditionalities are not binding for our sample, our approach closely aligns with studies evaluating universal cash transfers (Almås et al., 2020; Haushofer and Shapiro, 2016). Our RDD identification strategy links our study to research that randomizes or shifts the recipient's identity (Akresh et al., 2016; Almås et al., 2020; Armand et al., 2020; Benhassine et al., 2015; Lundberg et al., 1997; Ward-Batts, 2008). However, our structural approach enables a direct measurement of bargaining effects on individual poverty. Moreover, unlike many experimental studies that analyze the impact of relatively small transfers, AFAM constitutes a substantial redistribution to households, mitigating concerns about marginal transfer effects.

We show that AFAM, by directly transferring funds to women, enhances their bargaining power within the household and increases their resource share. This finding confirms and extends the results of Bérgolo and Galván (2018), who examined women's perceived control over decision-making. Our analysis reveals that the positive effect on women is primarily driven by rural women. This suggests that AFAM had more scope to promote empowerment in rural areas, which initially faced greater disparities in gender empowerment and attitudes. In contrast, we do not find significant effects for children. This result is consistent with the broader context of substantial pre-existing redistribution to children in low-income households. Child resource shares are already high, and child poverty is relatively low, particularly when compared to adult poverty. As a result, for women, the marginal utility of additional spending on children may be lower than that of increasing their own consumption. Finally, in terms of individual poverty, we find that AFAM's income effect reduces poverty among men, women, and children in eligible households, while its bargaining effect leads to a particularly strong reduction in women's poverty.

Our results underscore the crucial role of households' internal decision-making processes in shaping the outcomes of social policies. Moreover, they highlight the significance of context in determining the potential effects of such policies, raising questions about the external validity of previous finding and suggesting that the effectiveness of cash transfers and their impact on intra-household dynamics may vary significantly across different socio-economic and cultural settings. Several potential limitations should be acknowledged in our study. *First*, our RDD analysis relies on a relatively small sample. The estimation of resource shares requires detailed expenditure data, which inherently limits sample size due to the constraints associated with household surveys. Alternative approaches, such as those based on labor supply responses, could be explored and would benefit from the availability of large administrative datasets–similar to those used in reduced-form studies like Bérgolo and Cruces, 2021. Expanding the availability of expenditure data would not only enhance statistical power but also enable more granular analyses. This would allow for a deeper examination of heterogeneity in program effects, such as differences across subgroups or the impact of differential transfers to secondary school children. *Second*, the identification of the collective model relies on several assumptions, such as the stability of preferences and the use of exclusive goods (e.g., clothing). While these assumptions are transparent and relatively straightforward to operationalize with standard expenditure data–which typically include information on assignable goods such as male, female, and child clothing–further research is needed to assess how potential deviations from these assumptions might affect the robustness of our findings. Additionally, further validation of recent approaches to model identification would be valuable (see Bargain et al., 2022). *Third*, while our study acknowledges the role of traditional gender norms in rural areas, it does not deeply explore how these norms might interact with or moderate the program's effectiveness beyond the observed results. Further research, potentially integrating both qualitative and quantitative methods, would be valuable in examining these dynamics more thoroughly.

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Appendix

I. Non-linear SUR Estimations

We present here the detailed estimation technique. Since the error terms of the model are likely to be correlated across equations, each system is estimated using Non-Linear Seemingly Unrelated Regressions, as in Bargain et al. (2022) and Calvi (2020) for instance. The SUR estimator is iterated until the estimated parameters and error covariance matrices converge.[29] The likely correlation between the error terms in each budget-share function and the log total expenditure is a frequent source of endogeneity, especially if total expenditure suffers from measurement errors. We therefore proceed as in Banks et al. (1997) or Blundell and Robin (1999). Each budget share equation is augmented with the Wu-Hausman residuals obtained from a reduced-form estimation of x on all exogenous variables used in the model plus some instruments, namely the log pre-transfer household income and its quadratic term. Note that these instruments predict the log of expenditure very strongly (the F statistic on the excluded instruments is well above the usual threshold in all cases).

II. Model with Children as Public Goods

We present the collective model with children treated as public goods. This model is inspired by Blundell et al. (2005)'s original proposition that children can be considered as public goods within the households to evaluate the effect of a gender-targeted cash transfer. The specification is based on further work by Tommasi and Wolf (2016) and Manzur and Pendakur (2023) who incorporate this idea in the context of Dunbar et al. (2013). Household resources are assumed to be distributed between adults in the household (wife and husband) and we denote only two types of individuals in the household $i = f, m$, i.e. women and men. The system of household budget shares for the exclusive goods become:

$$\begin{aligned} W_s^{k_f} &= \eta_{f,s}(z^r) \cdot (\delta_{f,s}(z^p) + \beta_{f,s}(z^p) \cdot (x + \ln \eta_{f,s}(z^r, p))) \\ W_s^{k_m} &= \eta_{m,s}(z^r) \cdot (\delta_{m,s}(z^p) + \beta_{m,s}(z^p) \cdot (x + \ln \eta_{m,s}(z^r, p))) \end{aligned} \tag{5}$$

where the left-hand terms are observed. Applying the same assumptions as in the full model (SAP) and recalling that the shares add up to one, the derivatives with respect to log expenditure of the above system are:

$$\begin{aligned} \partial W_s^{k_f} / \partial x &= \eta_{f,s}(z^r, p) \cdot \beta_s(z^p) \\ \partial W_s^{k_c} / \partial x &= (1 - \eta_{f,s}(z^r, p)) \cdot \beta_s(z^p) \end{aligned} \tag{5}$$

for each s out of a total of S different family compositions. Since we only consider nuclear households here, the family composition is simply defined by the number of children ($s = s_c$). The specification derives from this setting in the same way as before and the estimation method is the same.

[29] The iterated SUR is equivalent to a maximum likelihood approach with multivariate normal errors.

III. Additional Results

Table A1: AFAM Effect on Log Household Income (Fuzzy RDD)

Smooth function of the eligibility score:	Quadratic	Cubic	Quartic	Spline	Quadratic Spline
Eligible	0.330 ***	0.266 ***	0.274 **	0.238 ***	0.197 *
	(0.087)	(0.093)	(0.115)	(0.082)	(0.120)
R-squared	0.304	0.304	0.305	0.302	0.305
Eligible rural	0.391 ***	0.325 ***	0.330 ***	0.302 ***	0.258 **
	(0.102)	(0.108)	(0.126)	(0.098)	(0.131)
Eligible urban	0.306 ***	0.245 ***	0.249 **	0.215 **	0.174
	(0.089)	(0.095)	(0.117)	(0.084)	(0.122)
R-squared	0.304	0.306	0.306	0.303	0.306
Observations			1,355		

Source: authors' estimations using the household expenditure survey (ENGIH 2016/2017). Notes: The table reports the RDD estimates corresponding to AFAM eligibility on (log) total household income. Columns refer to different specifications of the smooth function of the score (running variable). No other controls are included. We focus on households with adults (at least on man and one woman) and 1 to 4 children. Urban households refer to localities with more than 5,000 inhabitants. Standard errors in parentheses. *, **, *** indicate 10%, 5% and 1% significance levels.

Table A2: Sample Selection

	Sample size	% of total sample
Complete sample	6 889	
Households with at least one child under 14 (a)	2 185	100.0%
Households with 1 to 4 children	2 163	99.0%
Households with both male and female adults	1 854	84.9%
Excluding teenage adults (b)	1 365	62.5%
No missing, expenditure trimming / **main selected sample**	**1 355**	62.0%
Sample of nuclear households	984	45.0%

Source: authors' sample selection from the household expenditure survey (ENGIH 2016/2017). Notes: (a) 14 years of age is the limit below which a child is classified as such in clothing expenditures, used for identification of resource sharing between children, men and women. (b) Excludes children between 14 and 17 (whose clothing expenditure is not differentiated from adults').

Table A3: Descriptive Statistics

	Total sample	Couple with 1 child	Couple with 2 children	Couple with 3-4 children
Proportion of sample	1.00	0.55	0.34	0.11
Number of men	1.15	1.18	1.09	1.17
	(0.410)	(0.442)	(0.327)	(0.455)
Number of women	1.21	1.26	1.15	1.17
	(0.480)	(0.521)	(0.415)	(0.430)
Number of children	1.58	1.00	2.00	3.16
	(0.732)	(0.000)	(0.000)	(0.368)
Children average age	6.36	6.42	6.17	6.66
	(3.687)	(4.255)	(3.062)	(2.035)
Urban	0.75	0.78	0.73	0.71
	(0.432)	(0.418)	(0.446)	(0.454)
Years of education (women)	10.42	10.23	10.90	9.84
	(3.930)	(3.681)	(4.164)	(4.246)
Years of education (men)	9.54	9.36	9.96	9.15
	(3.745)	(3.522)	(3.991)	(3.938)
Eligible household	0.30	0.22	0.32	0.65
	(0.459)	(0.412)	(0.469)	(0.478)
Household income (PPP 2011)	2 340	2 229	2 538	2 279
	(2024)	(1702)	(2317)	(2449)
Household formal income (PPP 2011)	1 338	1 312	1 439	1 162
	(1376)	(1178)	(1607)	(1490)
Household expenditure (PPP 2011)	1 817	1 676	1 973	2 030
	(1393)	(1149)	(1554)	(1838)
Budget shares: clothing expenditure				
Men	0.013	0.014	0.012	0.008
	(0.018)	(0.019)	(0.016)	(0.013)
Women	0.016	0.017	0.015	0.011
	(0.020)	(0.021)	(0.019)	(0.015)
Children	0.024	0.020	0.028	0.033
	(0.023)	(0.020)	(0.023)	(0.030)
Proportion of non-zero clothing expenditure				
Men	0.614	0.619	0.617	0.580
	(0.487)	(0.486)	(0.487)	(0.495)
Women	0.702	0.716	0.706	0.620
	(0.458)	(0.451)	(0.456)	(0.487)
Children	0.913	0.892	0.935	0.947
	(0.282)	(0.310)	(0.247)	(0.225)
Observations	1 355	743	462	150

Source: authors' statistics from the selected sample drawn from the household expenditure survey (ENGIH 2016/2017). Notes: the selection corresponds to households with men, women, and 1-4 children under 14 (and no children aged 14-17), to align with the classification of child clothing in the data. Standard deviations in parentheses.

Table A4: Women's Relative Characteristics and Views on Gender Roles

	Urban	Rural	Diff.
A. Women's relative characteristics (nuclear households)			
Age ratio (woman's age over man's age)	0.926	0.898	0.028 ***
	(0.147)	(0.151)	
Weight of women's income in total household income	0.368	0.278	0.090 ***
	(0.269)	(0.261)	
Women's labor market participation (%)	0.736	0.626	0.110 ***
	(0.441)	(0.485)	
B. Women's views on gender roles (ENAJ data)			
Raising children should be the primary task of women	0.260	0.356	-0.096 ***
	(0.439)	(0.480)	
Women should choose careers that don't interfere with a future family project	0.238	0.353	-0.115 ***
	(0.426)	(0.479)	
If I could I would stop working to dedicate myself exclusively to my family	0.330	0.439	-0.109 ***
	(0.470)	(0.497)	
If my partner's wage was higher, I would stop working	0.213	0.279	-0.066 ***
	(0.409)	(0.449)	
Even if my household income was sufficient, I would not stop working to maintain my autonomy. (Disagree)	0.220	0.351	-0.131 ***
	(0.414)	(0.478)	
Gender role index	0.292	0.393	-0.101 ***
	(0.250)	(0.280)	

Source: Panel A: authors' statistics using the household expenditure survey (ENGIH 2016/2017). Panel B. authors' statistics using the Youth National Survey (ENAJ) 2018. Notes: Panel A presents results for nuclear households with children under 14 years of age (n=984). Panel B presents opinions regarding gender norms and attitudes for women between 25 and 35 (n=2582). Statistics correspond to the percentage of agreement with each statement (completely agree or agree), except for the fifth question (disagreement). Gender role index is the average of the answers to all questions. Urban households refer to localities with more than 5,000 inhabitants. Standard deviations in parentheses. *, **, *** indicate 10%, 5% and 1% significance levels of the t-test.

Table A5: AFAM Effect on Female/Children's Clothing Expenditure Relative to Men's

Smooth function of the eligibility score:	Quadratic	Cubic	Quartic	Spline	Quadratic Spline
Women's clothing over men's					
Eligible	1.585 **	1.562 **	1.383	1.622 ***	1.552 *
	(0.638)	(0.733)	(0.904)	(0.620)	(0.846)
R-squared	0.006	0.006	0.006	0.006	0.006
Eligible rural	1.477 **	1.457 **	1.266	1.521 **	1.443 *
	(0.634)	(0.702)	(0.839)	(0.627)	(0.803)
Eligible urban	1.881 **	1.860 *	1.676	1.918 **	1.846 *
	(0.883)	(1.010)	(1.197)	(0.856)	(1.119)
R-squared	0.007	0.007	0.007	0.007	0.007
Children's clothing over men's					
Eligible	1.229	1.754	0.775	1.443	1.643
	(1.112)	(1.219)	(1.398)	(1.148)	(1.319)
R-squared	0.021	0.022	0.024	0.021	0.021
Eligible rural	1.276	1.796	0.812	1.487	1.690
	(1.230)	(1.325)	(1.389)	(1.275)	(1.388)
Eligible urban	1.100	1.634	0.682	1.312	1.515
	(1.271)	(1.368)	(1.765)	(1.262)	(1.543)
R-squared	0.021	0.022	0.024	0.021	0.022

Source: authors' estimations using the household expenditure survey (ENGIH 2016/2017). Notes: The table reports the RDD estimates corresponding to AFAM eligibility on female and children clothing expenditure relative to men´s. Columns refer to different specifications of the smooth function of the score (running variable). No other controls are included. We focus on households with adults (at least on man and one woman), 1 to 4 children, and nonzero shares of men´s clothing. Urban households refer to localities with more than 5,000 inhabitants. Standard errors in parentheses. *, **, *** indicate 10%, 5% and 1% significance levels.

Table A6: Marginal Effects of Sharing Rule Determinants for Alternative Specifications

	(1)		(2, baseline)		(3)		(4)		(5)	
	Children	Women	Children	Women	Children	Women	Children	Women	Children	Women
AFAM eligibility x rural	-0.029	0.066 **	-0.025	0.063 **	-0.016	0.054 *	-0.018	0.053 *	-0.017	0.051 *
	(0.033)	(0.031)	(0.033)	(0.032)	(0.036)	(0.029)	(0.036)	(0.030)	(0.035)	(0.029)
AFAM eligibility x urban	-0.013	-0.010	-0.013	-0.011	-0.005	-0.009	-0.005	-0.004	-0.008	-0.010
	(0.032)	(0.025)	(0.032)	(0.025)	(0.032)	(0.030)	(0.033)	(0.031)	(0.032)	(0.030)
Number of children	0.125 ***	-0.005	0.137 ***	-0.011	0.153 ***	-0.114 ***	0.154 ***	-0.125 ***	0.153 ***	-0.110 **
	(0.034)	(0.032)	(0.033)	(0.031)	(0.033)	(0.042)	(0.030)	(0.036)	(0.034)	(0.043)
Number of women	-0.166 ***	0.155 ***	-0.160 ***	0.150 ***	-0.125 **	0.150 ***	-0.126 **	0.168 ***	-0.121 **	0.151 ***
	(0.061)	(0.045)	(0.061)	(0.044)	(0.057)	(0.044)	(0.057)	(0.043)	(0.057)	(0.045)
Number of men	-0.040	-0.032	-0.042	-0.029	-0.039	-0.056	-0.031	-0.043	-0.032	-0.068
	(0.056)	(0.043)	(0.056)	(0.045)	(0.061)	(0.063)	(0.065)	(0.058)	(0.060)	(0.063)
Average age of children	-0.088 ***	0.071 ***	-0.102 ***	0.079 ***	-0.110 ***	0.104 ***	-0.114 ***	0.111 ***	-0.110 ***	0.103 ***
	(0.022)	(0.019)	(0.022)	(0.019)	(0.023)	(0.022)	(0.024)	(0.023)	(0.023)	(0.022)
Urban	0.053	-0.071	0.042	-0.071	0.007	0.118	0.007	0.135 *	0.006	0.114
	(0.072)	(0.059)	(0.068)	(0.059)	(0.072)	(0.082)	(0.073)	(0.079)	(0.070)	(0.080)
Years education women			-0.036	0.046 *	-0.049 *	0.049 *	-0.054 *	0.060 **	-0.041	0.050 *
			(0.028)	(0.023)	(0.029)	(0.027)	(0.029)	(0.027)	(0.028)	(0.026)
Years education men			-0.055 **	0.005	-0.046 *	0.022	-0.041	0.023	-0.032	0.009
			(0.027)	(0.022)	(0.027)	(0.026)	(0.027)	(0.026)	(0.028)	(0.026)
Women´s proportion of formal work					-0.005	0.021				
					(0.018)	(0.017)				
All member´s proportion of formal work							-0.015	0.029		
							(0.027)	(0.025)		
Household formal earnings									-0.074 ***	0.059 **
									(0.028)	(0.025)
Observations	1355		1355		1355		1355		1355	

Source: authors' estimations using the Uruguayan household expenditure survey (ENGIH 2016/2017). Notes: the table reports the marginal effects of the different sharing rule determinants on children and women's resource shares, using alternative specifications. Marginal effects refer to the efect of the variable on the resource share of the demographic group, not on the per-person shares. Marginal effects are estimated based on average household characteristics for the sample in the specified row and based on the heterogeneous effect of AFAM model sepecification. The spline specificacion is used for the running variable. AFAM eligibility is determined by poverty score over zero. The variables in years (age and education) are expressed divided between 10. Formal work refers to the presence of a formal working women (3) or member (4) un the household. Households with men, women, and 1 to 4 children. Standard errors in parentheses. *, **, *** indicate 10%, 5% and 1% significance level.

Table A7: Marginal Effects of AFAM on Children, Women, and Men Resource Shares: Alternative Bandwidths around Eligibility Cutoff

	Children	Women	Men
Bandwidth: complete sample (baseline), n=1355			
Eligible rural	-0.025	0.063 **	-0.038
	(0.033)	(0.032)	(0.030)
Eligible urban	-0.013	-0.011	0.024
	(0.032)	(0.025)	(0.032)
Bandwidth: 90% of total sample, n=1220			
Eligible rural	-0.028	0.069 *	-0.041
	(0.032)	(0.037)	(0.044)
Eligible urban	-0.022	-0.024	0.047
	(0.034)	(0.033)	(0.034)
Bandwidth: 80% of total sample, n=1085			
Eligible rural	-0.032	0.066 *	-0.033
	(0.034)	(0.037)	(0.041)
Eligible urban	-0.006	-0.020	0.026
	(0.038)	(0.034)	(0.031)
Bandwidth: 70% of total sample, n=949			
Eligible rural	-0.030	0.068 *	-0.038
	(0.036)	(0.039)	(0.043)
Eligible urban	-0.004	-0.012	0.016
	(0.038)	(0.036)	(0.028)
Bandwidth: 60% of total sample, n=814			
Eligible rural	-0.052	0.081 **	-0.029
	(0.034)	(0.040)	(0.044)
Eligible urban	-0.019	-0.011	0.031
	(0.040)	(0.035)	(0.030)
Bandwidth: 50% of total sample, n=679			
Eligible rural	-0.069 *	0.075 *	-0.006
	(0.035)	(0.041)	(0.045)
Eligible urban	-0.016	-0.006	0.022
	(0.039)	(0.036)	(0.029)

Source: authors' estimations using the Uruguayan household expenditure survey (ENGIH 2016/201
Notes: The table reports marginal effects of AFAM eligibility (poverty score over zero) on the resour shares of children, women, and men in different symetric bandwidths around the eligibility cutoff. Margir effects are estimated based on average household characteristics for the sample in the specified row. T model is specified with standard controls and the linear spline function of the score (running variable). Urb households refer to localities with more than 5,000 inhabitants. Standard errors in parentheses. *, **, * indicate 10%, 5% and 1% significance level.

Table A8: Marginal Effects of AFAM Eligibility on Children, Women, and Men's Resource Shares: Alternative Specifications of the Sharing Rule (Nuclear Households)

	Nuclear households			Nuclear & children as public goods	
	Children	Women	Men	Women	Men
Basic (Urban, Average age of children)					
Eligible rural	-0.042	0.107 ***	-0.065 **	0.116 ***	-0.116 ***
	(0.038)	(0.036)	(0.030)	(0.038)	(0.038)
Eligible urban	0.010	-0.007	-0.003	0.001	-0.001
	(0.042)	(0.036)	(0.020)	(0.043)	(0.043)
Basic specification + education of men and women (baseline)					
Eligible rural	-0.042	0.108 ***	-0.066 **	0.117 ***	-0.117 ***
	(0.038)	(0.036)	(0.031)	(0.037)	(0.037)
Eligible urban	0.006	-0.004	-0.002	0.001	-0.001
	(0.042)	(0.036)	(0.020)	(0.043)	(0.043)
Basic specification + education, women formal work					
Eligible rural	-0.042	0.109 ***	-0.068 **	0.119 ***	-0.119 ***
	(0.038)	(0.036)	(0.031)	(0.037)	(0.037)
Eligible urban	0.005	-0.003	-0.002	0.004	-0.004
	(0.042)	(0.036)	(0.022)	(0.043)	(0.043)
Basic specification + education, members formal work					
Eligible rural	-0.039	0.113 ***	-0.073 **	0.127 ***	-0.127 ***
	(0.040)	(0.037)	(0.032)	(0.038)	(0.038)
Eligible urban	0.005	0.001	-0.006	0.014	-0.014
	(0.042)	(0.036)	(0.021)	(0.044)	(0.044)
Basic specification + education, household formal income					
Eligible rural	-0.039	0.106 ***	-0.066 **	0.117 ***	-0.117 ***
	(0.037)	(0.036)	(0.031)	(0.038)	(0.038)
Eligible urban	0.002	-0.001	-0.001	0.002	-0.002
	(0.041)	(0.036)	(0.023)	(0.043)	(0.043)
Observations		984		984	

Source: authors' estimations using the Uruguayan household expenditure survey (ENGIH 2016/2017). Notes: focusing on the sample of nuclear households, i.e. couples living with children but no other adults, the table presents marginal effects of AFAM eligibility (poverty score over zero) on resource shares of children, women, and men, evaluated at sample mean for each group (all nuclear households, rural nuclear households or urban nuclear households). Urban households refer to localities with more than 5,000 inhabitants. Standard errors in parentheses. *, **, *** indicate 10%, 5% and 1% significance level.

Table A9: Marginal Effects of AFAM Eligibility on Children, Women, and Men's Resource Shares: Alternative Bandwidths around Eligibility Cutoff (Nuclear Households)

	Nuclear households			Nuclear & children as public goods	
	Children	Women	Men	Women	Men
Bandwidth: complete sample of nuclear households (n=984)					
Eligible rural	-0.042	0.108 ***	-0.066 **	0.117 ***	-0.117 ***
	(0.038)	(0.036)	(0.031)	(0.037)	(0.037)
Eligible urban	0.006	-0.004	-0.002	0.001	-0.001
	(0.053)	(0.034)	(0.044)	(0.043)	(0.043)
Bandwidth: 90% of the nuclear household sample (n=887)					
Eligible rural	-0.033	0.101 **	-0.068 *	0.099 **	-0.099 **
	(0.037)	(0.040)	(0.042)	(0.043)	(0.043)
Eligible urban	0.009	-0.018	0.009	-0.017	0.017
	(0.051)	(0.041)	(0.034)	(0.050)	(0.050)
Bandwidth: 80% of the nuclear household sample (n=786)					
Eligible rural	-0.036	0.088 **	-0.052	0.079 *	-0.079 *
	(0.033)	(0.044)	(0.043)	(0.044)	(0.044)
Eligible urban	0.038	-0.003	-0.034	0.015	-0.015
	(0.052)	(0.029)	(0.047)	(0.051)	(0.051)
Bandwidth: 70% of the nuclear household sample (n=689)					
Eligible rural	-0.032	0.100 **	-0.068	0.095 **	-0.095 **
	(0.033)	(0.047)	(0.048)	(0.048)	(0.048)
Eligible urban	0.037	-0.008	-0.030	0.016	-0.016
	(0.053)	(0.034)	(0.044)	(0.054)	(0.054)
Bandwidth: 60% of the nuclear household sample (n=590)					
Eligible rural	-0.043	0.112 **	-0.069	0.104 **	-0.104 **
	(0.034)	(0.045)	(0.052)	(0.048)	(0.048)
Eligible urban	0.040	-0.021	-0.019	-0.027	0.027
	(0.057)	(0.029)	(0.047)	(0.053)	(0.053)

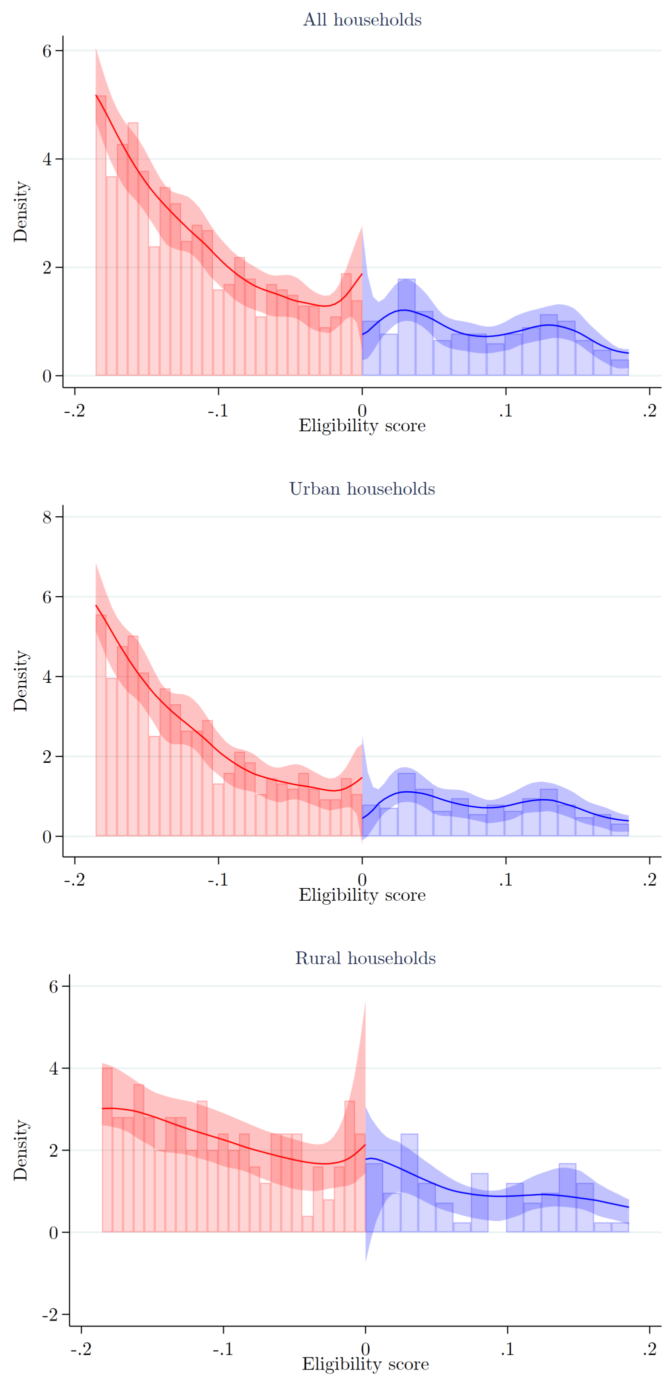
Source: authors' estimations using the Uruguayan household expenditure survey (ENGIH 2016/2017). Notes: focusing on the sample of nuclear households, i.e. couples living with children but no other adults, the table reports marginal effects of AFAM eligibility (poverty score over zero) on the resource shares of children, women, and men for different symetric bandwidths around the eligibility cutoff. Marginal effects are estimated based on average household characteristics for the sample in the specified row. The model is specified with standard controls and the linear spline function of the score (running variable). Urban households refer to localities with more than 5,000 inhabitants. Standard errors in parentheses. *, **, *** indicate 10%, 5% and 1% significance level.

Table A10: Effect of AFAM Eligibility on Education Expenditure

	Quadratic	Cubic	Quartic	Spline	Quadratic Spline
Eligible	1.219	0.561	0.491	0.253	0.200
	(1.357)	(1.460)	(1.796)	(1.273)	(1.869)
R-squared	0.290	0.291	0.291	0.290	0.291
Eligible rural	1.361	0.707	0.665	0.388	0.345
	(1.395)	(1.491)	(1.833)	(1.310)	(1.897)
Eligible urban	0.854	0.152	0.112	-0.118	-0.179
	(1.590)	(1.688)	(1.962)	(1.527)	(2.047)
R-squared	0.290	0.291	0.291	0.290	0.291
Observations			1,355		

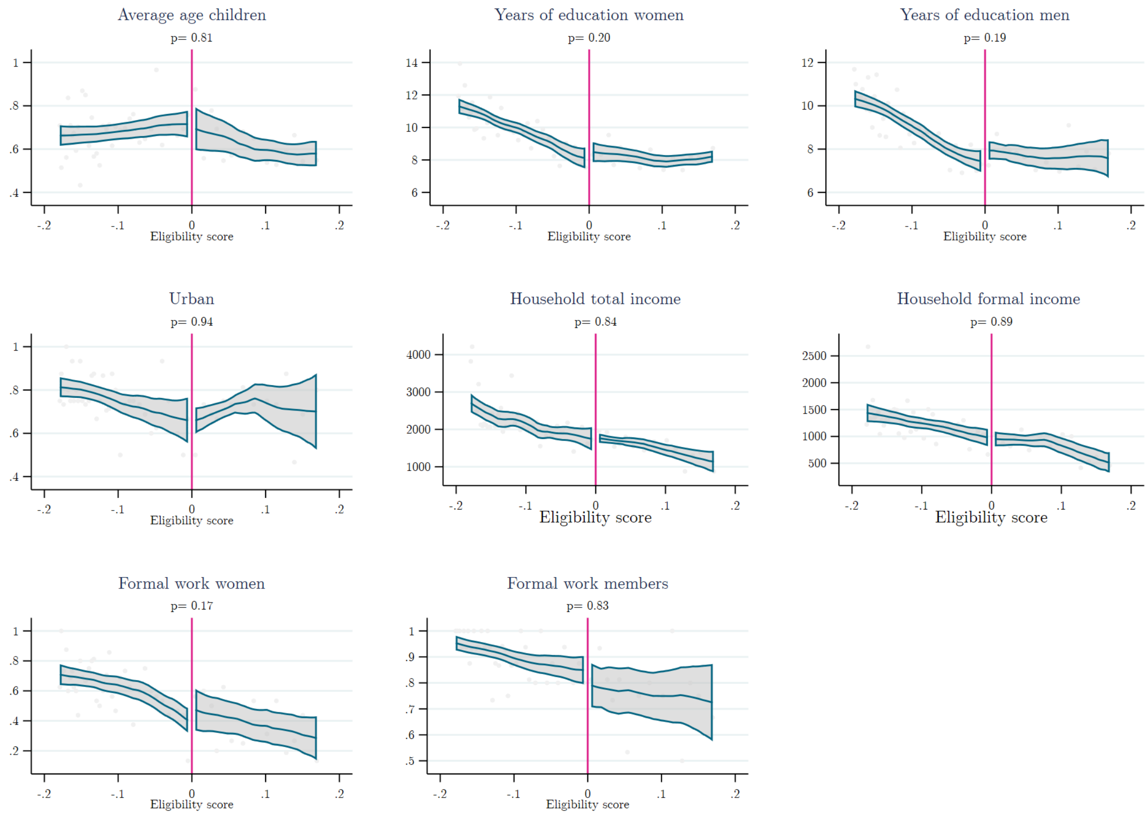
Source: authors' estimations using the Uruguayan household expenditure survey (ENGIH 2016/2017). Notes: the table reports the RDD estimates corresponding to the potential effect of AFAM eligibility on (log) education expenditures. Columns refer to different specifications of smooth function of the score (running variable). We also control for household income to neutralize AFAM income effects on education spending. We focus on households with adults (at least on man and one woman) and 1 to 4 children. Urban households refer to localities with more than 5,000 inhabitants. Standard errors in parentheses. *, **, *** indicate 10%, 5% and 1% significance level.

Figure A1: Manipulation Test



Source: authors' estimations using the data from household expenditure survey (ENGIH 2016/2017). *Notes:* Cattaneo et al. (2018) manipulation test based on a local polynomial density estimator, which does not require pre-binning of the data (optimal bandwidth). For the complete sample, the final manipulation test yields a statistic of -0.1451 with a p-value of 0.8846, indicating no statistical evidence of systematic manipulation of the running variable. p-value for Urban Households is 0.8629 and 0.1372 for Rural Households. Households with men, women, and 1 to 4 children for households as close to the eligibility cutoff as possible (bandwidth 60%, N=814). Urban households refer to localities with more than 5,000 inhabitants.

Figure A2: Balances Tests for Covariables



Source: authors' estimations using the data from household expenditure survey (ENGIH 2016/2017). *Notes:* These figures exhibit graphical evidence of the balance tests that check for systematic imbalances of covariates at each side of the cutoff. The dots represent the mean value of the covariates over 50 bins of the poverty score. The lines represent the locally weighted regressions of the variables on the poverty score and the 95% confidence intervals. Subtitles report the p-value of the RDD regression of covariates as dependent variables using the quadratic spline specification of the running variable. Households with men, women, and 1 to 4 children for households as close to the eligibility cutoff as possible (bandwidth 60%, N=814).