Welfare stigma after take-up

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Abstract

Welfare stigma (the disutility or psychological cost from receiving welfare per se) has primarily been studied as a barrier to take-up, but whether program participation induces stigma among enrolled beneficiaries remains an open question. This paper provides causal evidence that such effects exist. Exploiting regression discontinuities in Uruguay's two largest social assistance programs—Asignaciones Familiares–Plan de Equidad (AFAM-PE) and Tarjeta Uruguay Social (TUS)—and combining administrative records with survey data collected 5–10 years after enrollment, I find that welfare participation significantly increases self-reported stigma among recipients. Effect sizes are substantial, ranging from 0.46 to 0.67 standard deviations. Drawing on scales designed to capture shame and humiliation in contexts of poverty, I distinguish between experiences of shame (personal stigma) and mistreatment by others (social stigma). While both programs raise personal stigma, only TUS, which delivers benefits through a publicly visible mechanism, increases social stigma. AFAM-PE, which transfers funds discreetly, shows no such effect. These findings document that welfare participation imposes psychological costs beyond take-up, and suggest that institutional design, particularly benefit visibility, may shape both its intensity and nature.

Keywords: welfare stigma; social protection; regression discontinuity design; Asignaciones Familiares - Plan de Equidad; Tarjeta Uruguay Social.

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Cash transfers have become a cornerstone of anti-poverty policy worldwide. Nearly 1,000 programs across 200 countries cover about 2.5 billion people (Gentilini, 2022; Banerjee et al., 2024), and extensive evidence shows their effectiveness in reducing poverty and improving wellbeing (Bastagli et al., 2019). Yet as safety nets expand, so does debate over whether they foster dependency among the poor (Banerjee et al., 2017). When financed through public funds, such programs may generate taxpayer resentment (Besley and Coate, 1992) based on welfare stigma—the disutility (or psychological cost) from receiving welfare per se (Moffitt, 1983). These concerns are reflected in widespread stigmatizing attitudes: 38% of Latin Americans agree that welfare recipients are lazy (LAPOP, 2012), 42% of Europeans believe welfare promotes idleness (ESS, 2016), while welfare recipients rank as the second most negatively viewed group in the United States (ANES, 2016).

While prior research has focused on stigma as a barrier to take-up (see Ko and Moffitt (2024) for a review), far less is known about how stigma affects those who actually receive benefits. This gap matters because stigma may impose costs beyond deterring participation, including status loss, reduced self-respect, social isolation, and experiences of shame, humiliation, and discrimination (Link and Phelan, 2001; Spicker, 2011). Understanding whether and how such stigma affects beneficiaries is key to designing social protection policies that mitigate psychological costs while preserving their effectiveness.

This paper addresses this gap by providing causal evidence of welfare stigma among enrolled beneficiaries. I exploit regression discontinuities in Uruguay's two main non-contributory[1] cash transfer programs—Asignaciones Familiares–Plan de Equidad (AFAM-PE) and Tarjeta Uruguay Social (TUS), both run by the Ministry of Social Development (MIDES)—and find that program participation significantly increases stigma. Distinguishing between personal stigma (internalized shame) and social stigma (perceived mistreatment by others), I show that both programs raise personal stigma, while social stigma arises only under TUS, which delivers benefits through a publicly visible mechanism (tagged food card and a separate payment system). Together, these findings document that welfare stigma persists after enrollment, and suggest that institutional features, particularly benefit visibility, shape both its intensity and nature. Indeed, following this research, MIDES redesigned the TUS card to reduce its visible branding.

[1] That is, programs that do not require recipients to have made prior contributions to the social security system (unlike pensions, unemployment insurance, etc.).

AFAM-PE is a conditional cash transfer targeted at low-income households with children under 18.[2] Payments are delivered privately via bank transfers or in-person cash disbursements. As of 2017, the monthly benefit was US$49 for the first child (11% of the minimum wage), covering 37% of households with children in the country. Meanwhile, TUS is a "near cash transfer" that targets the most vulnerable households in the country, providing a government-issued food card restricted to essential purchases at selected retailers.[3] The card bears an official logo, making recipients publicly identifiable (Figure A2). In 2017, TUS covered 15% of households with children, with a monthly benefit of US$33 (8% of the minimum wage) for the first child.

The identification strategy exploits proxy means-test cutoffs that determine eligibility for each program. An algorithm predicts household vulnerability based on observable characteristics, creating discontinuities in participation probabilities at the eligibility thresholds (the magnitude of the jumps are 0.76 for AFAM-PE and 0.64 for TUS).[4] Higher scores indicate greater vulnerability; because TUS targets poorer households, its eligibility cutoff is higher. The combination of many variables and undisclosed rules makes manipulation unlikely, and density tests confirm no discontinuity in the score distribution at either cutoff. I thus use a fuzzy RDD, comparing applicants just above and below each threshold to obtain credible local estimates of both programs' impacts.

The analysis combines two matched data sources. Administrative records from MIDES provide detailed socioeconomic and demographic information, such as household composition, income, employment, and exact eligibility scores, covering 2008–2018 (with baseline period 2008–2010). These are linked to a follow-up survey specifically designed to evaluate program impacts (Amarante and Vigorito, 2011), conducted in two waves (2011–2012 and 2016–2018) among eligible and ineligible applicants near eligibility thresholds. The survey collects self-reported data on education, employment, health attitudes, perceptions, and stigma-related outcomes (available only in the second wave). Thus, the analysis examines medium- to long-term stigma effects, around five to ten years after households began receiving benefits.

Measuring welfare stigma is particularly challenging, as it is inherently difficult to quantify and disentangle from other determinants of wellbeing. To address this, I draw on a questionnaire

[2] Beneficiary households must comply with standard conditionalities, including children's school attendance and regular health check-ups.

[3] Authorized goods include food, cleaning, and hygiene items; alcohol and cigarettes are excluded.

[4] AFAM-PE also requires per capita formal income below a given threshold, computed from administrative records on households' formal income (e.g., earnings, pensions, and transfers) (Bérgolo and Cruces, 2021).

developed by the Oxford Poverty and Human Development Initiative (OPHI), designed to provide "direct measures of experiences of shame, humiliation, stigma, and discrimination" in the context of poverty (Zavaleta, 2007, p. 3). Using principal component analysis, I identify the questions most strongly associated with the latent constructs (personal and social stigma) and build two indices capturing internalized shame and perceived mistreatment.[5] These constitute the main outcomes of my analysis.[6]

The results reveal that welfare stigma is present among recipients of public non-contributory cash transfers, though its intensity and form differ across programs. In the preferred specification, participation in AFAM-PE and TUS increases personal stigma by 0.46 and 0.67 standard deviations, respectively. Several factors may explain the higher personal stigma associated with TUS. Unlike AFAM-PE, TUS operates outside the country's traditional social security system, which may amplify feelings of disconnection from contributory programs often viewed as more legitimate. Moreover, TUS targets a narrower and more vulnerable population, potentially more susceptible to shame. The program's purchase restrictions emphasize an inability to meet basic needs without external aid, further intensifying feelings of self-devaluation. These findings suggest that welfare beneficiaries internalize social norms valuing financial independence, fostering personal stigma.

The effects on social stigma notably differ. In the preferred specification, TUS increases social stigma by 0.65 standard deviations, while AFAM-PE has no statistically significant effect (0.14 standard deviations). Examining individual responses within both indices further shows that TUS particularly increases the frequency of being treated unfairly, discriminated against, mocked, and feeling helpless, with estimates ranging from 0.43 to 0.68 standard deviations. TUS recipients thus report experiencing greater judgment and discriminatory treatment, likely in public settings.

While fully disentangling mechanisms is beyond this study's scope, institutional features and prior research point to public visibility as a key factor. Delivered through a prominently marked food card and a separate payment system, TUS makes beneficiaries publicly identifiable, exposing them to scrutiny and judgment, a pattern supported by qualitative evidence of prejudicial treatment

[5]The shame scale asks respondents to rate the frequency of feelings such as self-consciousness, embarrassment, and helplessness on a 0–3 frequency scale. The mistreatment scale asks if respondents have been treated with disrespect, unfairly, or have been discriminated against in the past three months on a 0–3 frequency scale.

[6]For an extensive discussion of personal and social stigma, see Walker (2014) and Baumberg (2015). Related concepts in the economics literature include self- and social-image concerns (Bénabou and Tirole, 2006, 2011; Bursztyn and Jensen, 2017; Friedrichsen and Engelmann, 2018; Falk, 2021).

while shopping, from other customers and store employees (Moreno et al., 2014). This is consistent with experimental and observational evidence showing that visible markers of welfare participation exacerbate stigma by making recipiency status salient (Friedrichsen et al., 2018; Celhay et al., 2025). In contrast, AFAM-PE's private delivery shields recipients from public exposure. These differences in visibility may thus be central to explaining both the intensity and nature of social stigma experienced by beneficiaries.

Finally, shame, humiliation, and discrimination have been shown to be key predictors of life satisfaction (Hojman and Miranda, 2018), and reductions in subjective wellbeing have been interpreted as evidence of welfare stigma in prior studies (Gao and Zhai, 2017; Qi and Wu, 2018). To explore whether stigma affects subjective wellbeing, I examine the impacts of AFAM-PE and TUS on self-reported life satisfaction, measured on a scale from 1 (very dissatisfied) to 10 (very satisfied). I find no statistically significant effects for either program, though coefficients are consistently negative and larger in magnitude for TUS. While these estimates are imprecise, likely due to limited statistical power, the pattern is consistent with the hypothesis that psychological costs from stigma may partly offset the positive income effects of additional cash on life satisfaction.

This paper contributes to multiple strands of the literature. First, I provide causal evidence of welfare stigma effects after program take-up.[7] While existing research has focused on welfare stigma as a barrier to take-up,[8] its effects on individuals who receive benefits remain largely unexplored. This gap is important because welfare stigma not only discourages participation but may also affect the wellbeing of those who do participate (Besley and Coate, 1992). Existing work on post-take-up welfare stigma is limited to non-causal evidence, including qualitative studies from Chad (Della Guardia et al., 2022), quantitative analyses from China (Gao and Zhai, 2017; Qi and Wu, 2018) and the U.S. (Lapham and Martinson, 2022), and recent evidence linking program participation intensity in local networks to under-reporting in surveys (Celhay et al., 2025). To the best of my knowledge, this is the first study to provide causal evidence of welfare stigma after enrollment.

Second, I distinguish between personal and social stigma, contributing to the broader literature on image concerns. While prior research has extensively explored how norms and identity shape

[7] Overall, stigma has been widely studied in contexts such as social behavior, norms, and identity (Lindbeck et al., 1999; Hungerman, 2013; Bursztyn et al., 2020; Ghosal et al., 2022), labor markets (Chakraborty et al., 2018; Osman and Speer, 2023), and welfare programs (Stuber and Kronebusch, 2004; Stuber and Schlesinger, 2006; Kleven and Kopczuk, 2011; Bargain et al., 2012; Bhargava and Manoli, 2015; Friedrichsen et al., 2018; Kline and Tartari, 2016).

[8] Other take-up determinants include information asymmetries and transaction costs (Currie, 2004).

behavior (Akerlof and Kranton, 2000; Bénabou and Tirole, 2006, 2011; Kranton, 2016; Falk, 2021; Butera et al., 2022; Celhay et al., 2025), differentiating between self and social dimensions has proven challenging (Bursztyn and Jensen, 2017). Using survey measures of shame and discrimination (Zavaleta, 2007), I link these distinct experiences to self- and social-image concerns, shedding light on how institutional features, such as visibility and delivery mechanisms, affect stigma. This suggests that self- and social-image concerns can operate as complements rather than substitutes, an open question in the image literature (e.g., Bursztyn et al., 2017).

Third, I contribute to the literature on cash transfers by examining psychological costs beyond standard wellbeing measures. While existing research has focused on life satisfaction (e.g., Haushofer and Shapiro, 2016; Haushofer et al., 2019, 2020; Romero et al., 2021), I measure shame and mistreatment or discrimination separately, underscoring the importance of capturing multiple dimensions to understand how institutional design affects distinct outcomes. This distinction reveals that programs can impose psychological costs even when overall life satisfaction remains unchanged, suggesting offsetting effects between income gains and stigma costs.[9]

The remainder of the paper is organized as follows. Section 1 provides an overview of the institutional context and theory of change. Sections 2 and 3 describe the data and empirical strategy. Section 4 presents the results, while Section 5 concludes and discusses policy recommendations.

1 Background

Institutional context. Child allowances in Uruguay have evolved significantly since their origins in 1943 as a contributory benefit system. Initially targeting formal workers, successive reforms in the 1990s and 2000s added a non-contributory pillar,[10] expanding eligibility to vulnerable households previously excluded from the formal system. In response to the country's severe economic crisis of the early 2000s, the government introduced temporary aid programs in 2005, including the Plan de Atenci'on Nacional a la Emergencia Social (PANES) and Tarjeta Alimentaria, targeting the

[9] The dramatic rise of cash transfers in Latin America makes this an interesting context to study welfare stigma since they have been implemented by national governments with increased social expenditure and often framed outside traditional social security systems (Atuesta and Cecchini, 2017), creating conditions where welfare stigma may be particularly salient. For a review of cash transfer impacts in Latin America, see Molina-Millan et al. (2016). Previous specific studies for Uruguay have examined health and birth outcomes, employment, and political support (Manacorda et al., 2011; Amarante et al., 2016; Bérgolo and Galván, 2018; Bérgolo and Cruces, 2021; Tenenbaum and Vigorito, 2025).

[10] Meaning that no formal work requirement is set for households to access these allowances.

poorest households in the country (see Manacorda et al. (2011); Amarante et al. (2016) for details). These temporary measures laid the foundation for the permanent programs studied here.[11]

Asignaciones Familiares - Plan de Equidad. AFAM-PE, which officially started in 2008, is Uruguay's largest Conditional Cash Transfer (CCT) program, targeting low-income households with children under 18 or with pregnant women. Its dual objectives are to provide monetary assistance to vulnerable populations and promote school attendance and medical check-ups among children. As of 2017, the program covered approximately 200,000 households, with a monthly transfer amount of US$49 for the first beneficiary (equivalent to 11% of the minimum wage). Eligibility is determined through a two-step means-test process: households must be below an income threshold based on formal earnings reported in social security records and then pass a proxy means-test using the Índice de Carencias Críticas (ICC).[12] The ICC is an algorithm designed to predict household poverty based on indicators such as educational attainment, housing quality, and ownership of durable goods. Benefits are conditional on compliance with educational and health requirements, although systematic enforcement began only in 2013. Cash transfer payments are delivered discreetly via bank accounts or payment centers.

Tarjeta Uruguay Social. The program originated from the Tarjeta Alimentaria introduced in 2005 and was redesigned in 2009 to use the ICC. In 2011, following a recalculation of ICC weights and re-certification of most beneficiaries, the program was officially renamed TUS. It is a near-cash transfer aimed at covering basic nutritional needs for households in extreme poverty. It targets a narrower subset of the population than AFAM-PE (approximately 60,000 households as of 2017) and provides a monthly transfer of US$33 for the first beneficiary (equivalent to 8% of the minimum wage).[13] Eligibility is determined using the ICC but at a higher threshold, reflecting its focus on the extreme poor (for scores above an additional cutoff, the voucher amount doubles). The assistance is delivered through a government-issued card, restricted to food, cleaning and hygiene item purchases at authorized retailers (*Comercios Solidarios*).[14] While TUS and AFAM-PE are managed independently, their coverage overlaps substantially, with 77% of TUS households

[11]See Arim et al. (2009) for an overview of the history and structure of Uruguay's social security system.

[12]Refer to Amarante and Vigorito (2011) for additional details on the ICC's construction.

[13]The monthly installment additionally includes the full discount of the Value Added Tax (VAT) liability of purchases made with TUS.

[14]For a detailed description of the retailer network refer to Aguirre et al. (2022).

also receiving AFAM-PE benefits (88% among households with children).[15] The card prominently displays a government logo and remarks its associated to a "social plan," making recipients publicly identifiable as beneficiaries (Figure A2 shows the layout of both cards).

Attitudes and beliefs. Despite Uruguay's relatively low poverty and inequality rates among Latin American countries,[16] negative public attitudes toward poverty and welfare recipients are widespread, ranking among the highest across the continent. The attribution of poverty to personal failings, specifically the belief that people are poor because they are "lazy or lack willpower" rose sharply from 12% in 1996 to 45% in 2011 (Figure A1). While most Uruguayans support government assistance through employment (94%) or food (83%), only half favor helping the poor directly with cash (ELBU, 2016-2017).[17] Moreover, Uruguay registers the highest share in Latin America believing that "welfare recipients are lazy," with 52% agreeing compared to a regional average of 38% (LAPOP, 2012). Existing qualitative evidence (see Appendix B) further underscores these stigmatizing narratives, highlighting the enduring notion of the "undeserving poor." Together, these prevalent negative beliefs about welfare recipients and its robust welfare state make Uruguay a compelling case for analyzing welfare stigma.

Theory of change. Welfare participation may affect beneficiaries through multiple channels. Increased household income should improve material wellbeing and potentially reduce stigma by enabling households to meet basic needs, but receiving social assistance may simultaneously generate welfare stigma costs that vary by program design. Both AFAM-PE and TUS share features that may trigger personal stigma: non-contributory status, targeting based on poverty status, and means-testing by economic vulnerability. However, TUS targets a more vulnerable population (covering 6% of households compared to AFAM-PE's 37%), operates entirely outside the traditional social security system, and restricts purchases to basic necessities, emphasizing recipients' inability to meet fundamental needs autonomously. These additional features suggest TUS should generate stronger personal stigma than AFAM-PE, with both programs expected to increase self-reported

[15] TUS was originally targeted at households with children under 18, but later expanded to cover other vulnerable populations, including extreme poor adults, transgender individuals, homeless populations, and people with chronic diseases.

[16] Uruguay, a small country with 3.3 million inhabitants, ranks among the region's leaders in key socioeconomic indicators, such as GDP per capita, life expectancy, Human Development Index, poverty headcount, and the Gini coefficient.

[17] Relatedly, only 42% believe the government should assist all poor people, compared to 58% who think help should be limited to those who have "made an effort in life," while 48% agree that non-poor individuals often make poor people feel bad (ELBU, 2016-2017).

shame relative to non-recipients just below eligibility thresholds.

The programs differ markedly in their potential to generate social stigma, which arises from external judgment. AFAM-PE delivers benefits discreetly through bank transfers or cash, making participation invisible to the public. In contrast, TUS operates through a prominently marked food card that must be used at authorized retailers, making welfare status publicly visible during each transaction and exposing recipients to potential scrutiny and judgment by others. For instance, others can observe and judge the products recipients choose when grocery shopping. Thus, TUS recipients should report substantially higher levels of mistreatment than AFAM-PE recipients, for whom social stigma effects should be minimal due to discreet benefit delivery.

2 Data

The empirical analysis combines two matched data sources. First, administrative records from MIDES provide detailed information on applicant households from 2008–2018. Second, a follow-up survey conducted in two waves (2011–2012 and 2016–2018) was specifically designed to evaluate the programs' impacts (Amarante and Vigorito, 2011).[18]

MIDES administrative records. These data provide detailed baseline and longitudinal information on applicant households. The baseline application records include the socioeconomic and demographic data used to calculate each household's eligibility score, the Índice de Carencias Críticas (ICC), spanning 2008–2010 for AFAM-PE and 2011 for TUS (following the re-certification of the program).[19] Critically for the regression discontinuity design, these records contain the exact, normalized ICC value. The index is normalized such that the eligibility threshold is set to zero for each program, with positive values indicating eligibility and negative values indicating non-eligibility.[20] While the baseline information is already included in the follow-up survey, I merge the records until 2018 to track program participation throughout the study period.

[18] This survey has been previously used in several studies (see, e.g., Bérgolo and Galván, 2018; Bérgolo and Cruces, 2021; Tenenbaum and Vigorito, 2025).

[19] When the program was launched, households already listed in the PANES program were automatically enrolled. As a result, the AFAM-PE records include PANES baseline data, which was not updated for the ICC computation and predates the program's beginning.

[20] The ICC algorithm, developed by researchers at *Universidad de la República*, uses a probit model to predict the likelihood that an applicant household belongs to the lowest income quintile using variables from the national household survey (Amarante and Vigorito, 2011). Eligibility cutoffs differ for Montevideo (the capital city) and the rest of the country (interior), reflecting differences in cost of living (which explains why the index needs to be normalized to use both regions together).

ESAFAM follow-up survey. It complements the administrative registries with key self-reported data collected through in-person interviews at respondents' homes. Specifically designed to evaluate AFAM-PE's impacts, the survey sampling overrepresents households near its eligibility threshold, setting an optimal bandwidth to balance comparability with the need for sufficient sample size.[21] Additional households were drawn to represent the remaining AFAM-PE eligible population, which is where the TUS eligibility threshold lies. Importantly, the TUS evaluation sample is drawn from AFAM-PE eligible households. The resulting TUS effects are therefore conditional on AFAM-PE eligibility, meaning I estimate the impact of receiving TUS among households (with children) that have also qualified for AFAM-PE at some point during the study period.

The first wave (2011–2012) surveyed 3,832 households, while the second wave (2016–2018) retained 1,734 (45% of the original sample).[22] The final study sample, after removing observations with missing values on variables of interest, comprises 917 households (67% eligible) for the AFAM-PE evaluation and 595 households (51% eligible) for the TUS study.

Descriptive statistics show that both the AFAM-PE and TUS samples are predominantly female (89% and 93%, respectively) and have a similar geographic distribution (around 28% reside in Montevideo). Consistent with its targeting of extreme poverty, the TUS sample shows evidence of greater vulnerability: respondents have fewer years of education on average (7.6 years vs. 9.4 years), reside in larger households (4.8 members vs. 3.6 members), and are less likely to identify as white (70% vs. 82%) compared to the AFAM-PE sample. Average ages are 44.5 years and 40.4 years for the AFAM-PE and TUS samples, respectively. Conditional on participation, average tenure in each program is 5.3 years for AFAM-PE and 8.2 years for TUS.[23]

Beyond traditional economic topics like employment, income, and education, the survey contains dedicated modules on wellbeing, attitudes, and beliefs, including specific questions on shame, humiliation, and stigma in the second wave. As a result, this study assesses the medium- to long-term welfare stigma consequences, six to ten years after households become beneficiaries.[24]

OPHI survey module. This module, developed by Zavaleta (2007), seeks to capture depriva-

[21] See Amarante and Vigorito (2011) for details; the sample was drawn using a stratified random sampling method, with strata defined by eligibility status and geographic region (Bérgolo et al., 2016).

[22] Estimates from Rivero et al. (2019) and Tenenbaum and Vigorito (2025) indicate that attrition is uncorrelated with the ICC and other observable characteristics.

[23] The descriptive statistics for program tenure are computed for the period of August 2009 to September 2018.

[24] Figure A3 illustrates the timeline of the study.

tion in a “missing dimension” that is key to poverty and wellbeing: the ability to live without shame (Alkire, 2007). These questions distinguish between *shame*, a personal emotion reflecting self-devaluation (personal stigma), and mistreatment or “*external humiliation*,” which arises from interactions and perceptions of others’ judgment (social stigma) (Mills and Zavaleta, 2015). The shame proneness scale includes questions drawn from the Personal Feelings Questionnaire-2, assessing the frequency of emotions like embarrassment, helplessness, and self-consciousness. The mistreatment scale draws on questions from existing surveys such as the European Social Survey (ESS), and captures interactions where individuals feel disrespected, treated unfairly, or discriminated against. These measures enable the specific study of stigma-related feelings, providing insights into self- and social-image concerns.

The shame scale asks respondents to rate the frequency of listed feelings (self-conscious, ridiculous, embarrassed, etc.) from 0 (rarely or never) to 3 (always or almost always). The question specifically asks: *For each of the following listed feelings, please place a number from 0 to 3, reflecting how frequent the feeling is for you*”. The mistreatment scale focuses on external interactions, asking whether respondents felt treated without respect, unfairly, or with discrimination during the last three months, using the same 0–3 frequency scale. The question reads: “*Have you felt that you have been treated [in the following ways] during the last three months?*”. Table 1 shows descriptive statistics for each individual item.

To reduce data dimensionality and retain items most correlated with the latent constructs, I build two composite indices using Principal Component Analysis (PCA) (Filmer and Pritchett, 2001; Yin and Etilé, 2019). The mistreatment index includes all three items, which is sufficient for PCA. For the shame index, I retain six items (self-conscious, ridiculous, embarrassed, humiliated, laughable, and helpless), elected based on their ability to explain 62% of the total variance, high Kaiser-Meyer-Olkin (0.83) and Cronbach’s Alpha (0.77) scores, and individual PCA loadings above 0.3. More details on this procedure are provided in Appendix C. The item “feeling stupid” is excluded due to its low relevance to poverty and welfare recipiency and weak inter-item correlations. For simplicity, the indices are constructed by summing and standardizing (mean = 0, SD = 1) the retained items.[25]

Descriptive statistics (Tables 2 and 1) show left-skewed distributions, with approximately 80% of

[25] Alternative weighting methods (Filmer and Pritchett, 2001; Hoynes et al., 2016) yield consistent results.

responses at zero for most items, suggesting these questions capture intense rather than mild stigma experiences that may be difficult to disclose to interviewers (potentially prone to social desirability bias). Despite this floor interpretation, the measures show expected patterns: eligible individuals and those in the TUS sample, who face greater deprivation levels, report higher frequencies of shame and mistreatment (Walker, 2014; Hojman and Miranda, 2018).

3 Identification strategy

By leveraging the selection criteria outlined in Section 1, I estimate the causal impact of program participation using a Regression Discontinuity Design (RDD). The analysis compares households just above and below the normalized ICC eligibility thresholds for both AFAM-PE and TUS samples. Eligibility thresholds are defined separately for Montevideo and the rest of the country to account for regional differences in cost of living, infrastructure, and access to services. In the analysis, I normalize the ICC score within each region so that zero corresponds to the eligibility cutoff, and the normalized score represents distance from the threshold. [26]

The identification assumption requires that the eligibility threshold renders program assignment quasi-random within a sufficiently small segment around it. Because program eligibility does not perfectly determine participation, I implement a Fuzzy RDD using a two-stage least squares (2SLS) estimation approach, where the eligibility indicator is the instrumental variable for actual program participation.

Figure 1 shows the first-stage discontinuities for the baseline period (2008–2010).[27] The y-axis shows the household participation rate in each program, with dots representing averages over five-score bins and overlaid fitted lines.[28] For AFAM-PE, some initially ineligible households later reapplied and gained eligibility, increasing participation rates to the left of the cutoff, while compliance among eligible households on the right is perfect.

For TUS, the lower participation rates among eligible households likely reflect program capacity constraints and administrative delays that prevented all eligible households from receiving benefits

[26] For reference, the ICC index ranges from 0 to 1, with non-normalized cutoffs of roughly 0.2 and 0.6 for AFAM-PE and TUS, respectively.

[27] Figure A4 presents analogous plots using ten bins on each side of the cutoff.

[28] Following Gerardino et al. (2024), I use five bins on each side of the cutoff and report both linear and quadratic fits in all RDD figures.

at the time of application, rather than self-selection (Tenenbaum and Vigorito, 2025).[29] This supply-side non-compliance suggests the fuzzy RDD estimates represent a lower bound of stigma effects. Moreover, even if there was demand-side non-compliance (e.g., eligible households being deterred by anticipated stigma, as theory would predict), the estimates would still be conservative. In such a case, compliers would be the less stigma-sensitive households, and the average effect across all eligible households would be larger. The estimates should thus be interpreted as local average treatment effects for compliers and thus a lower bound for the underlying effect.

Bandwidth selection follows the survey design described in Amarante and Vigorito (2011). For AFAM-PE, the survey oversamples households near the eligibility cutoff to optimize the precision of local comparisons while maintaining adequate statistical power. This design effectively defines the estimation bandwidth, spanning approximately $[-0.05, 0.07]$ in the normalized ICC score.

For TUS, the relevant bandwidth is determined by the ICC range in which eligible TUS households appear on one side of the cutoff and ineligible (but AFAM-PE–eligible) households on the other. Because of this overlap, the TUS bandwidth is necessarily broader, extending roughly from $[-0.40, 0.40]$. Due to these sampling constraints, I retain these fixed bandwidths for the primary analysis but conduct robustness checks with incrementally narrower bandwidths to assess the sensitivity of the main results.[30]

Regression specifications. As a benchmark and robustness check, I begin by estimating a sharp RDD using ordinary least squares, which would be appropriate if the discontinuities at the cutoffs were deterministic. Following Lee and Lemieux (2010), the sharp RDD is estimated as:

$$Y_i = \alpha_1 + \beta_1 D_i + \gamma_1 f(ICC_i^*) + \gamma_1' D_i \times f(ICC_i^*) + \delta_1 X_i + \epsilon_i \tag{1}$$

where Y_i denotes the outcome variable, D_i is an indicator equal to one for households above the normalized cutoffs ($ICC^* \geq 0$), $f(ICC_i^*)$ is a polynomial function of the normalized eligibility score, X_i is a vector of control variables, and ϵ_i is the error term (assumed i.i.d).

Because compliance at the eligibility thresholds is incomplete (participation jumps of 0.76 for AFAM-PE and 0.64 for TUS) the main analysis relies on a fuzzy RDD estimated by 2SLS. In the

[29] This was confirmed by MIDES staff, citing operational issues, such as delays in delivering the card, or cases where the card was issued to a different person than the applicant (e.g., who then could have exited the household).

[30] I conduct robustness checks by incrementally reducing the range of the running variable by 0.001 for AFAM-PE and 0.01 for TUS on each side of the cutoff.

first stage,

$$T_i = \alpha_0 + \lambda_0 D_i + \gamma_0 g(ICC_i^*) + \gamma_0' D_i \times g(ICC_i^*) + \delta_0 X_i + \eta_i \tag{2}$$

eligibility (D_i) instruments for actual program participation (T_i). The second stage then estimates the local average treatment effect:

$$Y_i = \alpha_2 + \beta_2 \widehat{T_i} + \gamma_2 g(ICC_i^*) + \gamma_2' T_i \times \widehat{h(ICC_i^*)} + \delta_2 X_i + \mu_i \tag{3}$$

where the parameter of interest β captures the causal effect of each program on outcome Y_i.[31]

For robustness, I test four different specifications: linear and quadratic polynomial functions of the ICC* score, with and without controls. Controls include sex (woman = 1), age, region (Montevideo = 1), and ethnicity (white = 1). Errors are clustered by the ICC* score. As a complementary robustness exercise, I also compute local polynomial (semi-parametric) estimates following Cattaneo et al. (2018); Cattaneo and Titiunik (2022), which provide bias-corrected point estimates and robust standard errors (Calonico et al., 2014). Although this approach typically optimizes bandwidths by outcome, I retain the fixed survey bandwidths to maintain comparability and preserve statistical power, as observations are already concentrated near the thresholds.

RDD internal validity. A valid RDD relies on two fundamental requirements (Imbens and Lemieux, 2008): (i) no other determinants of outcomes change discontinuously at the cutoff, and (ii) crossing the cutoff affects outcomes only through program participation. These assumptions are discussed below.

A sufficient condition for the first assumption is that the density of the running variable remains continuous around the threshold (Lee and Lemieux, 2010). In this setting, manipulation of the eligibility score is unlikely. The algorithm used to compute the ICC, including the variables, their weights, and the eligibility thresholds, was confidential and periodically revised, preventing applicants or local officials from adjusting responses to manipulate eligibility (Amarante and Vigorito, 2011). Figure 2 provides visual evidence and formal density tests (McCrary, 2008). The estimated log-differences in density at the cutoffs are –0.094 (S.E. = 0.241) for AFAM-PE and 0.173 (S.E. =

[31] This resembles a Wald estimator, which can alternatively be computed by rescaling β_1 (dividing it by λ_0).

0.261) for TUS, indicating no statistically significant discontinuities.[32]

Discontinuities in covariates are tested using the same RDD specification as in Equation (3), with each covariate as the outcome variable (Table 3). For the full sample, F-tests for the joint significance of these regressions reject the null of balanced covariates (Columns (1), (2), (5), and (6)), suggesting some imbalances around the thresholds. However, these discontinuities are consistent with prior analyses using the same data (Bérgolo and Galván, 2018; Tenenbaum and Vigorito, 2025) and are unlikely to compromise identification. The imbalances are driven by a single covariate in each sample: sex for AFAM-PE and region for TUS. To assess this, I conduct subgroup analyses focusing on women (for AFAM-PE) and interior residents (for TUS), who account for 90% and 74% of each program sample, respectively. When restricting the sample to these subgroups, F-statistics become statistically insignificant across specifications (Columns (3), (4), (7), and (8) of Table 3).[33] Moreover, estimates for these subgroups closely align with the baseline results, reinforcing the robustness of the main findings and supporting the validity of the identification strategy.

The fuzzy RDD design also satisfies instrumental-variable requirements: (i) the instrument strongly affects treatment probability, and (ii) the exclusion restriction holds. Eligibility strongly predicts participation, with first-stage F-statistics of 476 for AFAM-PE and 75 for TUS, comfortably exceeding the Stock and Yogo (2005) thresholds.[34] The exclusion restriction is unlikely to be violated, as the ICC details were not publicly known, reducing the risk of manipulation, and the ICC has not been used to assign any other distinct policies than AFAM-PE and TUS.

RDD external validity. Finally, RDD estimates reflect local average treatment effects (LATE), which are specific to observations near the threshold (Lee and Lemieux, 2010). This limits their generalizability to the entire beneficiary population, since estimates only apply to households who are neither the most advantaged or the most vulnerable. Furthermore, the TUS impact reflects its marginal effect "on top" of AFAM-PE, as most TUS recipients have also received AFAM-PE. Due to the LATE framework and potential heterogeneity across the ICC* distribution, the combined effect of both programs cannot be directly calculated. While this limits external generalizability,

[32] Consistent results arise from more recent tests by Cattaneo et al. (2018) (p = 0.188 for AFAM-PE, p = 0.568 for TUS) and Bugni and Canay (2021) (p = 1.000 and 1.000, respectively), providing robust confirmation of continuity in the score distribution.

[33] For these subgroup analyses, the other RDD checks are also verified, as shown in Figures A5 and A6.

[34] First-stage F-statistics for interactions between eligibility and polynomial terms are 408 for AFAM-PE and 80 for TUS. These are based on regressions using the shame-proneness index as the dependent variable under linear polynomial specifications.

it does not compromise internal validity and remains informative for understanding how program design influences welfare stigma at the margins of eligibility.

4 Results

This section presents the main results. I find that both AFAM-PE and TUS significantly increase personal stigma, with larger effects for TUS. Social stigma increases substantially for TUS but shows no effect for AFAM-PE. I first show graphical and regression evidence of these results, then analyze individual scale items, explore potential channels and rule out alternative explanations, examine effects on subjective wellbeing, and finally conduct robustness checks for the main stigma outcomes of interest.

Effects on welfare stigma. Figure 3 graphically illustrates the impacts of AFAM-PE and TUS on the main outcomes of interest: personal and social stigma. The y-axis measures outcome residuals after controlling for the set of covariates, with quintiles of the normalized eligibility score (x-axis) shown as dots on each side of the cutoffs (marked by vertical lines at zero), and linear and quadratic fitted lines overlaid.[35] Panels (a) and (c) display results for AFAM-PE using the [-0.047; 0.073] bandwidth, while Panels (b) and (d) show TUS estimates using the [-0.400; 0.400] bandwidth.

Both programs increase personal stigma (Panels (a) and (b)), with a more pronounced effect for TUS. AFAM-PE shows no impact on social stigma (Panel (c)), while TUS beneficiaries report significantly higher levels relative to non-beneficiaries (Panel (d)). Linear fits better capture the data patterns near the cutoffs, while quadratic fits are less precise given the small sample sizes in each sample. Consequently, I choose the linear estimates with controls as the preferred specification.[36]

Table 4 presents these results in regression form. Panel A reports sharp estimates for the parameter of interest (β), following Equation (1). Panel B shows fuzzy estimates adjusting for non-compliance, following Equation (3). Columns (1)-(4) show results for AFAM-PE and (5)-(8) for TUS. Under the sharp linear specification with controls, AFAM-PE and TUS increase personal stigma by 0.34 and 0.44 SD (significant at 5% and 1% levels). Adjusting for non-compliance scales

[35] Residuals are obtained by regressing each outcome on control variables and plotting the mean residuals by quintile. Figure A7 presents robustness checks with 10 bins at each side of the eligibility thresholds.

[36] Although controls are not required for unbiased RDD estimates, including them improves precision by reducing residual variance, especially in a fuzzy RDD set up.

these effects to 0.46 and 0.67 SD respectively.[37] These magnitudes are substantial. AFAM-PE nearly doubles the ineligible mean (from 1.06 to 2.11), equivalent to moving from the 53rd to 70th percentile in the AFAM-PE sample distribution. TUS raises the ineligible mean by 150% (from 1.33 to 3.34), moving respondents from the 48th to 75th percentile.[38]

Regarding social stigma, while AFAM-PE shows no significant effect, TUS increases social stigma by 0.42 to 0.65 standard deviations (both significant at the 1% level). This effect raises the ineligible mean by 115% (from 0.96 to 2.07), moving individuals from the 48th to 65th percentile within the TUS sample. These results are robust across specifications, though the quadratic fits are less precise due to small sample size.[39] While both programs similarly affect self-image concerns, the stark contrast in social image aligns with the hypotheses. Institutional design and public attitudes likely contribute to shame in both programs. However, the publicly visible delivery of TUS exposes recipients to external judgment and mistreatment, which is absent in AFAM-PE. This is consistent with prior research on social stigma (Friedrichsen et al., 2018; Celhay et al., 2025).[40]

Further insights into social and self-image concerns emerge from analyzing individual items within the mistreatment and shame scales. Table A1 reports estimates for each item (in standard deviations); including Romano-Wolf adjusted p-values to address multiple hypothesis testing (Clarke et al., 2020). TUS significantly increases the frequency of being treated unfairly, discriminated against, humiliated, mocked, and feeling helpless, with effects ranging from 0.43 to 0.68 standard deviations. These results confirm that TUS recipients experience both external mistreatment and more pronounced internalized shame. In contrast, AFAM-PE's shame effect is primarily driven by the "self-conscious" item, which reflects a deeply internal emotion, as this is the only statistically significant item for the program.[41]

To assess the sensitivity of the composite scales, I conduct a drop-test following Hoynes et al. (2016). For TUS, excluding the most significant item ("helpless") yields a robust effect of 0.61 SD

[37]The sharp RDD identifies the intent-to-treat (ITT) effect, i.e., the causal impact of eligibility at the threshold. Because of incomplete compliance, the ITT represents a lower bound on the true cost to recipients. I thus use the fuzzy RDD to estimate the local average treatment effect (LATE), which scales the ITT by the jump in participation probability to isolate the causal effect of actual program participation on stigma.

[38]Figure C1 provides additional details on these distributional calculations.

[39]Alternative index constructions, such as using PCA-weighted averages, rotated weights, or simple sums, yield similar results in magnitude and significance.

[40]See Appendix B for selected quotes from prior qualitative studies.

[41]For instance, self-consciousness and embarrassment reflect internal emotions (Tracy and Robins, 2004; Tangney and Tracy, 2011), while mistreatment or humiliation reflects external experiences (Hartling and Luchetta, 1999; Lindner, 2007). Table A2 reports results for items excluded from the main shame scale.

(significant at the 5% level). For AFAM-PE, excluding "self-conscious" reduces the effect from 0.46 to 0.37 SD and lowers statistical significance (p = 0.088), highlighting the relevance of this item to AFAM-PE's impact on shame, though other items still meaningfully contribute to the overall scale.

Overall, these findings show that TUS has broader and more consistent effects across both external and internal dimensions of stigma, while AFAM-PE's impact is more narrowly tied to personal stigma, primarily driven by the feeling of self-consciousness.

Heterogeneity by welfare tenure. An important question is whether welfare stigma reflects a short-lived adjustment or a persistent consequence of being labeled as a welfare recipient. If stigma were transitory, it would fade as beneficiaries adapt to program participation, as transfers become normalized, or as individuals exit the programs. Examining heterogeneity by program tenure thus can help understanding whether stigma constitutes a temporary or psychological cost from welfare participation.

To investigate this, I divide each program sample at the median tenure (64 months for AFAM-PE and 98 months for TUS) and re-estimate the main regression including this split. Table 5 reports these results. Across both programs, there is no statistically significant evidence of differential effects by tenure in either personal or social stigma. These results, however, should be interpreted with caution since program tenure is not an exogenous variable, which limits a causal interpretation of these heterogeneity patterns. Nonetheless, the absence of attenuation with shorter tenure suggests that once individuals become identified as recipients, the associated feelings of shame and mistreatment appear to persist over time, and may endure well beyond initial enrollment.

Effects on alternative explanations. The main results show that welfare participation increases both personal and social stigma among beneficiaries. However, these feelings could arise from channels other than welfare stigma associated with eligibility, such as poverty stigma or resentment toward the program. If such channels were in place, eligibility would affect stigma through means other than participation, violating the exclusion restriction. I leverage the richness of the ESAFAM survey and examine additional attitudinal measures to rule out alternative explanations.

A first concern is that shame or perception of mistreatment may reflect poverty stigma rather than welfare stigma. Becoming eligible for social assistance could make one's poverty status more salient, whereas being rejected might signal that the government officially does not recognize the household as poor. In this case, eligible individuals could feel more ashamed simply because they perceive themselves as poorer, not because of welfare participation itself. To test this, I examine respondents' perceived position in the income distribution (on a scale from 1 to 10). Table 6, Row (1), shows no significant differences between eligible and ineligible individuals in either program, suggesting that the results are not driven by differences in perceived poverty.

A second concern is that ineligible individuals may experience pride in not needing government aid to get by or resentment toward beneficiaries that got into these programs. Because pride can be viewed as the opposite of shame (Tangney and Tracy, 2011), these emotions could mechanically decrease measured shame among the ineligible group. To examine this, I analyze agreement with the statement that welfare beneficiaries "should feel ashamed." As shown in Row (2) of Table 6, differences between eligible and ineligible individuals are statistically insignificant for both programs. Likewise, there are no differences in overall policy support for both programs (Row (3)). This suggests the effects are not driven by resentment toward beneficiaries or the programs themselves.

A third concern is that the observed effects may stem from social judgments about appearance rather than welfare stigma. The ESAFAM survey includes a question about feeling ashamed due to inadequate clothing. The RDD estimates, shown in Row (4), reveal no significant effect for AFAM-PE under the linear specification, while the quadratic specification finds a significant impact at the 5% level, possibly reflecting scrutiny in social settings. For TUS, coefficients are consistently close to zero. For TUS, effects are consistently near zero, indicating no added shame related to clothing or appearance. These findings reinforce the idea that welfare stigma is the primary driver of increased shame and mistreatment, rather than alternative confounding channels.

Effects on subjective wellbeing. Shame and humiliation are documented predictors of life satisfaction (Hojman and Miranda, 2018), and declines in subjective well-being have often been interpreted as a result of welfare stigma (Gao and Zhai, 2017; Qi and Wu, 2018). Figure A8 shows the correlations between these variables in the ESAFAM data. If stigma is prevalent, it may thus have broader implications for beneficiaries' wellbeing. To analyze this, I estimate the effects of AFAM-PE and TUS on self-reported life satisfaction (z-score), measured on a scale from 1 (very dissatisfied) to 10 (very satisfied). Given that life satisfaction is asked in both survey waves, I report separate estimates using responses in each wave.

Figure 4 summarizes these results, reporting the estimated OLS and 2SLS regression coefficients on the figures. Although the estimates are not statistically significant for either program, the coefficients are consistently negative and larger in magnitude for TUS, consistent with its stronger stigmatizing effects. While these results are imprecise, the pattern in both waves suggests that psychological costs from stigma (particularly when both social- and self-image concerns are present) may partly offset the positive extra cash effects on life satisfaction.

Robustness checks. Several robustness checks are conducted to assess the internal validity of the main RDD estimates, confirming the robustness of the findings on personal and social stigma.

I begin by examining whether imbalances in specific covariates (e.g., sex for AFAM-PE, region for TUS) influence the results, as discussed in Section 3.[42] Such imbalances could bias the observed effects if they drive the results rather than program participation. To test this, I re-estimate the main analyses using subsamples restricted to women (for AFAM-PE) and non-Montevideo residents (for TUS). Figure A9 presents these results, which are consistent with the baseline estimates, reinforcing the validity of the causal findings.[43]

Next, placebo tests are conducted by applying the same RDD analyses to alternative, arbitrary cutoff values to confirm that the impacts are attributable to program eligibility rather than other confounding factors.[44] None of these alternative cutoffs yield estimates significantly distinct from zero, confirming that the observed impacts occur due to the change in eligibility (Figure A11).

Another standard RDD robustness check involves varying the bandwidth to assess sensitivity. While survey constraints prevent me from expanding beyond the thresholds, I test narrower bandwidths. Table A3 presents estimates for progressively tighter bandwidths using the preferred specification. Panels A, B, and C trim the ICC* by 0.01, 0.015, and 0.02 for AFAM-PE, and 0.1, 0.15, and 0.2 for TUS. Results remain robust, with narrower bandwidths yielding higher estimates (all significant at the 5% level). As expected, effects become more pronounced closer to the cutoff.[45]

To address potential noise from observations closest to the eligibility cutoff, a donut RDD excludes these observations near each threshold from the analyses. Table A4 presents these estimates. Panel A trims 0.001 (0.01) of the ICC* for AFAM-PE (TUS), while Panel B trims 0.005 (0.05).

[42] Figures A5 and A6, and Table 3 confirm that RDD requirements are met for these subsamples.

[43] Figure A10 compares the estimated β from full and restricted samples using linear specifications with controls. Results remain consistent. For TUS, effects are robust, though social stigma significance slightly decreases ($p = 0.051$) likely due to reduced power. For AFAM-PE, the effect on personal stigma slightly increases.

[44] For AFAM-PE, alternative cutoffs range from [-0.020, ..., -0.010] to [0.010, ..., 0.020], while for TUS, these range from [-0.20, ..., -0.10] to [0.10, ..., 0.20].

[45] Figure A12 shows coefficients for additional trimmings.

The results remain consistent, with slight increases in magnitude, all statistically significant at the 5% level. These findings confirm that near-threshold observations do not introduce significant bias arising from potential sorting or manipulation near the cutoff.

I further conduct local polynomial regressions with bias-corrected estimates and robust standard errors as an alternative to parametric specifications (Calonico et al., 2014; Cattaneo et al., 2018). Table A5 confirms results are robust to this estimation method, and shows that placing greater weight on near-cutoff observations amplifies estimates.[46]

As an additional validity check, I verify that the estimated increase in social stigma among TUS beneficiaries is not driven by respondents who had left the program by the time of the second wave survey. I leverage the administrative records covering the full study period (2008–2012) to restrict the sample to beneficiaries who received benefits over all preceding three months before the survey.[47] With this restriction, the number of observations drops from 560 to 519, and the point estimate of the impact of TUS on social stigma using the preferred specification is 0.614 (p = 0.019), confirming the robustness of this result.

5 Discussion

This paper empirically examined welfare stigma after take-up by analyzing the effects of Uruguay's two largest non-contributory cash transfer programs (AFAM-PE and TUS) on personal and social stigma. Using a regression discontinuity design that combines administrative and survey data, it identified the causal effects of program participation on self-reported feelings of shame (self-image concern) and perceiving mistreatment by others (social image concern). The results provide robust reduced-form evidence that receiving public transfers entails psychological costs for beneficiaries, with magnitudes and mechanisms differing across programs.

Participation in AFAM-PE increases personal stigma, raising a standardized shame index by 0.46 standard deviations, based on the preferred specification, but has no significant effect on social stigma. In contrast, TUS generates stronger stigmatizing effects, increasing both shame and mistreatment indices by 0.67 and 0.65 standard deviations, respectively. These patterns indicate that the institutional design of welfare programs plays a critical role in shaping welfare stigma. The

[46] This is consistent with the narrower bandwidth results from Table A3.

[47] This period corresponds to the reference window in the social stigma questions ("during the last three months").

public visibility of TUS, which is distributed through a tagged card, requires the use of a separate payment system at the store counter, and is restricted to food purchases, likely exposes recipients to public scrutiny, reinforcing social stigma. This interpretation is consistent with qualitative evidence from Uruguay and with quantitative findings in other settings showing that visibility amplifies stigma.

While this study identifies the existence and nature of welfare stigma, understanding its precise mechanisms remains an open question. Future research should directly test visibility as a channel by varying the salience of welfare participation. For instance, through experimental, randomized distribution of generic, untagged cards, and the use of a standard payment system. Theoretical models that formally incorporate stigma as a welfare cost of individuals would also help quantify the net welfare effects of cash transfers once this type of psychological cost is considered. Moreover, studies using behavioral or administrative outcomes, as well as longitudinal data, could shed light on whether stigma dissipates over time or lasts even after individuals exit welfare programs.

The findings highlight important implications for policy design. Policymakers should internalize that social protection can impose unintended psychological costs that may partially offset its material benefits. Reducing program visibility, for instance, could mitigate welfare stigma. Complementary interventions targeting the internal dimension of stigma may also help. Psychological or identity-based programs, as highlighted by Ghosal et al. (2022), can increase self-worth and improve decision-making among marginalized populations, who are the target of social assistance in the first place.

Finally, how benefits are framed matters. Framing benefits as entitlements, particularly those tied to children's wellbeing, may reduce internal shame by reinforcing beneficiaries agency and legitimacy. As emphasized by Bertrand et al. (2006), small framing choices can have large behavioral consequences. Consolidating AFAM-PE and TUS into a unified, less visible transfer system could similarly reduce social stigma while preserving the benefits of targeting. Taken together, these insights underscore that the success of social protection depends not only on how much is transferred or how it is delivered, but also on how it is perceived. Designing cash transfers that preserve dignity may thus be as crucial as the cash itself.

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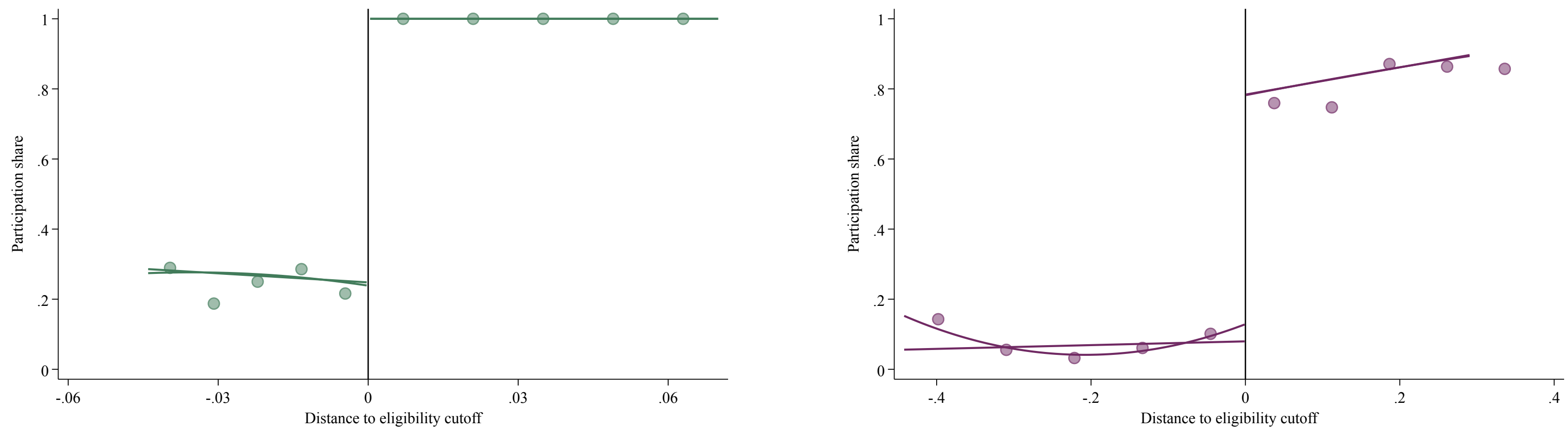
Figure 1:
First Stage Estimates

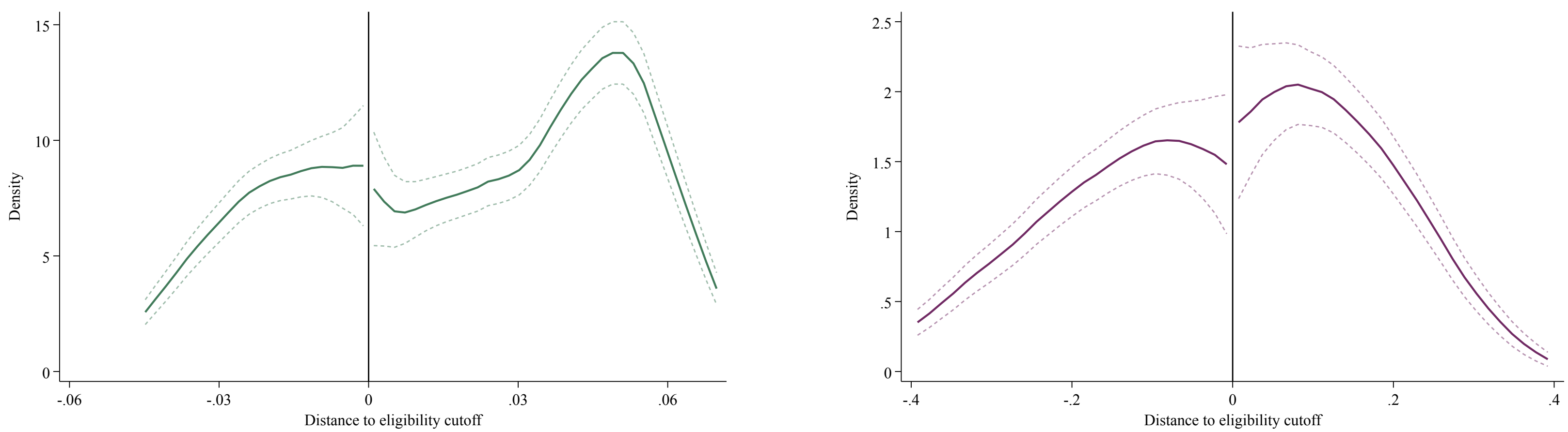
(a) AFAM-PE **(b)** TUS



Notes: These figures show first-stage estimates of program participation as a function of the normalized eligibility score, the Índice de Carencias Críticas (ICC*). Dots indicate the average share of participant households within ten-bin intervals, five on each side of the cutoff, marked by a vertical line at zero. Panel (a) reports results for AFAM-PE, and Panel (b) for TUS, using bandwidths of [–0.047, 0.073] and [–0.400, 0.400], respectively. Solid lines show fitted linear and quadratic trends. The discontinuities at the cutoff correspond to the estimated first-stage jumps in participation: approximately 0.76 for AFAM-PE and 0.64 for TUS. Figure A4 shows results using ten bins on each side of the cutoff, and Figure A5 presents analogous plots for the subsample robustness checks.

Figure 2:
McCrary Density Tests

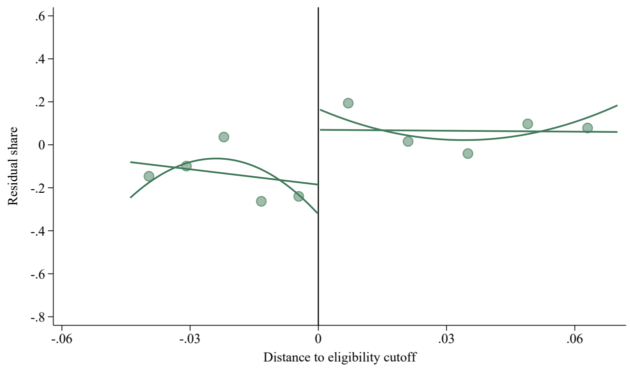
(a) AFAM-PE **(b)** TUS



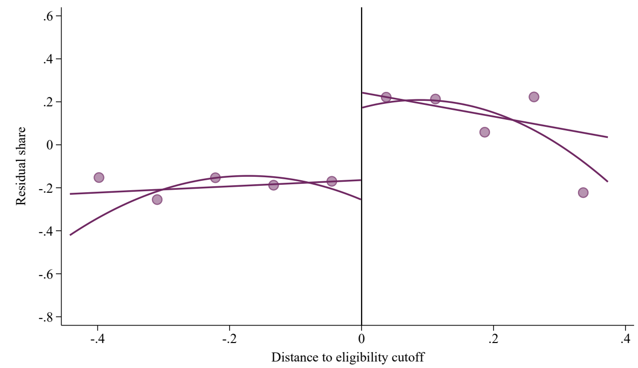
Notes: These figures show McCrary (2008) density tests to check for systematic bunching at the cutoffs (vertical black line at zero) of the AFAM-PE and TUS programs. The solid thick line is the density estimate (local linear regression with separate trends), and dashed lines represent 95% confidence intervals. Panel (a) uses a bandwidth of $[-0.047; 0.073]$ for the normalized eligibility score in the AFAM-PE sample, while Panel (b) uses a bandwidth of $[-0.400; 0.400]$ for the score in the TUS sample. The estimated log differences in density height at the cutoff are $\hat{\tau} = -0.094$ (S.E. 0.241) for AFAM-PE and $\hat{\tau} = 0.173$ (S.E. 0.261) for TUS. In both cases, the null hypothesis of continuity is not rejected. Recent tests confirm this conclusion: the p-values for the Cattaneo et al. (2018) test are 0.188 (AFAM-PE) and 0.568 (TUS), and for the Bugni and Canay (2021) test are 1.000 (AFAM-PE) and 1.000 (TUS). Figure A6 shows the same plots for the subsample analyses.

Figure 3:
RDD Impact Estimates of AFAM-PE and TUS on Stigma

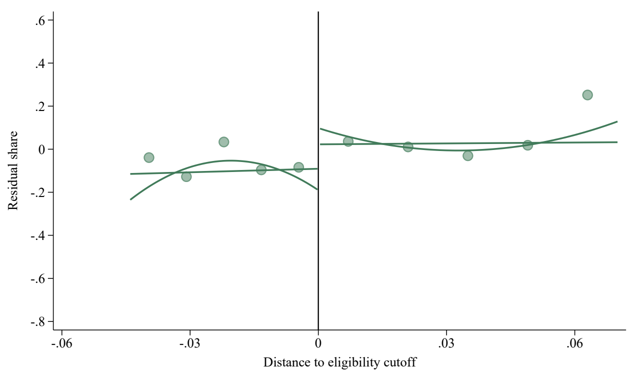
(a) AFAM-PE - Personal Stigma



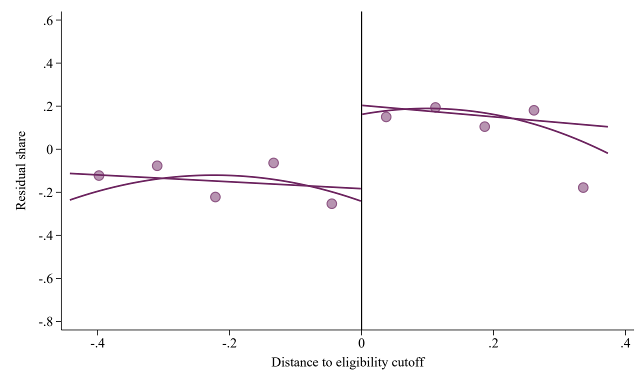
(b) TUS - Personal Stigma



(c) AFAM-PE - Social Stigma



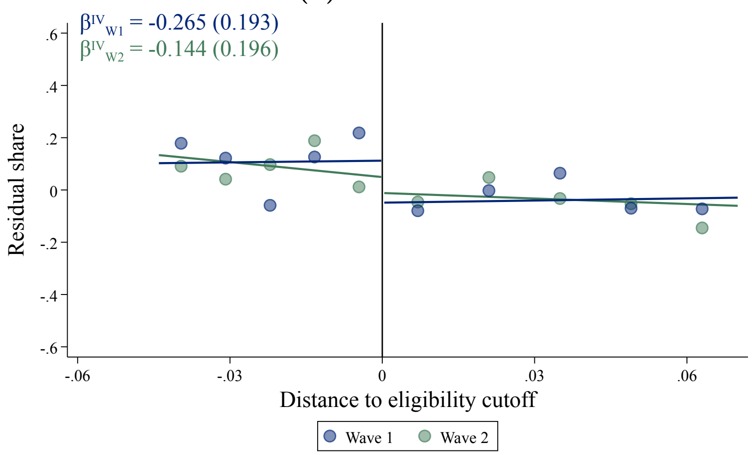
(d) TUS - Social Stigma



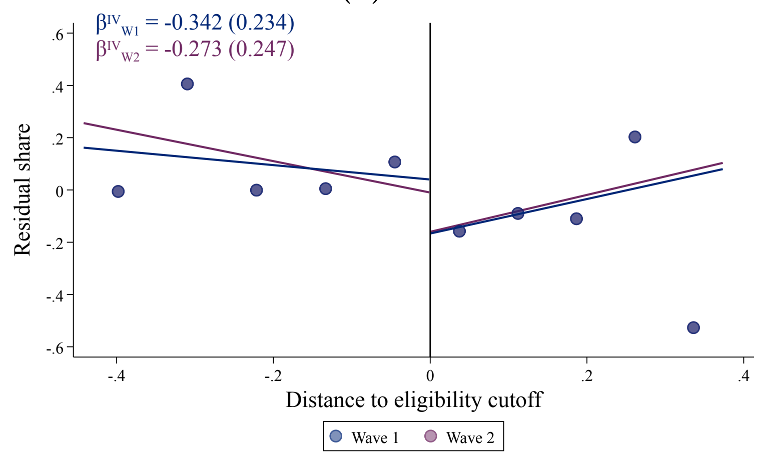
Notes: These figures show parametric RDD estimates of program impacts on the standardized indices (z-scores) of personal stigma (shame) and social stigma (mistreatment). Dots represent conditional mean values of each index across ten-bin intervals, five on each side of the normalized eligibility cutoff (marked by a vertical line at zero). Panels (a) and (c) correspond to AFAM-PE, estimated using a [-0.047; 0.073] bandwidth, while Panels (b) and (d) correspond to TUS, using a [-0.400; 0.400] bandwidth. Solid lines show linear and quadratic fitted values. Control variables include sex (woman = 1), age, region (Montevideo = 1) and ethnicity (white = 1). Table 4 reports the corresponding regression estimates. Figure A7 presents robustness checks using ten bins at each side of the cutoff.

Figure 4:
RDD Impact Estimates of AFAM-PE and TUS on Subjective Wellbeing

(a) AFAM-PE



(b) TUS



Notes: These figures show parametric RDD impact estimates for individuals' self-reported life satisfaction (z-scores). The specific question reads: "*On a scale of 1 to 10, where 1 is very dissatisfied and 10 is very satisfied, how satisfied are you in relation to to your life in general*" Dots represent conditional mean values of life satisfaction (z-score) across ten-bin intervals, five on each side of the normalized eligibility cutoff (marked by a vertical line at zero). Panel (a) corresponds to AFAM-PE, estimated using a [-0.047; 0.073] bandwidth, while Panel (b) corresponds to TUS, using a [-0.400; 0.400] bandwidth. Solid lines show linear fitted values at the time of the survey's Wave 1 (2011-2012) and Wave 2 (2016-2018). Coefficients (β) on the figures come from 2SLS regressions, following Equation (3), using the preferred linear specification with controls. Control variables include sex (woman = 1), age, region (Montevideo = 1) and ethnicity (white = 1). Standard errors are shown in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 1:
Summary Statistics of Stigma Survey Items

	For each of the following feelings, please place a number from 0 to 3 reflecting how frequent the feeling is for you	AFAM-PE			TUS		
		Obs. (1)	Mean (2)	S.D. (3)	Obs. (4)	Mean (5)	S.D. (6)
Panel A: *Shame indicators*							
Self-conscious	Feeling self-conscious	965	0.368	0.733	592	0.478	0.874
Ridiculous	Feeling ridiculous	952	0.176	0.499	587	0.286	0.703
Embarrassed	Feeling embarrassed	966	0.251	0.559	593	0.337	0.701
Humiliated	Feeling humiliated	966	0.196	0.537	593	0.250	0.635
Laughable	Feeling laughable	944	0.198	0.543	579	0.328	0.744
Stupid	Feeling stupid	951	0.183	0.507	587	0.216	0.576
Childish	Feeling childish	961	0.362	0.719	585	0.368	0.740
Blushing	Feeling blushed	969	0.733	0.908	589	0.801	0.999
Helpless	Feeling helpless, paralyzed	961	0.203	0.545	587	0.213	0.576
Disgusting	Feeling disgusting to others	937	0.085	0.374	572	0.121	0.464
	Have you felt that you have been [...] during the last three months?						
Panel B: *Mistreatment indicators*							
Disrespect	Treated without respect	982	0.426	0.783	596	0.487	0.833
Unfairness	Treated unfairly	967	0.415	0.731	594	0.51	0.877
Discrimination	Treated with discrimination	985	0.142	0.459	600	0.222	0.624

Notes: This table reports the questions from the survey module developed by Zavaleta (2007) regarding stigma, shame and humiliation feelings in contexts of poverty. Answer options are: 0 = Rarely or Never; 1 = Occasionally; 2 = Often, 3 = Always or almost always. Descriptive statistics (number of observations, means, and standard deviations) are shown separately for the AFAM-PE and TUS samples, differentiating between eligible and ineligible groups within each program.

Table 2:
Summary Statistics of Stigma Outcomes

	Obs.	Mean	S.D.	Min.	Max.	Ineligible	Eligible	Diff. Test
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: AFAM-PE								
A.1: Simple sum								
Personal stigma	917	1.373	2.304	0	17	1.062	1.528	0.466***
Social stigma	917	0.965	1.312	0	8	0.850	1.023	0.173*
A.2: Standardized sum								
Personal stigma	917	-0.003	1.000	-0.599	6.786	-0.138	0.064	0.202***
Social stigma	917	-0.015	0.981	-0.737	5.245	-0.102	0.028	0.130*
Panel B: TUS								
B.1: Simple sum								
Personal stigma	560	1.877	2.973	0	18	1.333	2.394	1.061***
Social stigma	560	1.239	1.700	0	9	0.960	1.510	0.551***
B.2: Standardized sum								
Personal stigma	560	-0.005	1.001	-0.637	5.426	-0.188	0.170	0.357***
Social stigma	560	0.010	1.016	-0.730	4.645	-0.157	0.169	0.326***

Notes: This table reports descriptive statistics for the main composite outcome variables: personal stigma (shame) and social stigma (mistreatment). For each index, both standardized (in standard deviation units) and non-standardized (simple sum) versions are reported. Panel A provides statistics for the AFAM-PE sample, and Panel B covers the TUS sample. The final three columns show the mean values for eligible (Column 6) and ineligible (Column 7) individuals within each program, with Column (8) reporting the results of a difference-in-means t-test between both groups. Descriptive statistics for the individual survey items used to construct these composite indices are provided in Table 1. $***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.10$.

Table 3:
Balance Tests

	AFAM-PE				TUS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sex (woman = 1)	0.160**	0.201***	-	-	-0.068	-0.023	-0.109	-0.136
	(0.067)	(0.062)	-	-	(0.062)	(0.099)	(0.067)	(0.119)
Region (Montevideo = 1)	-0.621***	0.001	-0.574***	0.068	0.031	0.528**	-	-
	(0.114)	(0.091)	(0.124)	(0.190)	(0.108)	(0.225)	-	-
Age	-5.259**	-1.931	-6.489***	-4.085	-4.363*	-2.993	-4.205	-2.660
	(2.131)	(2.253)	(2.248)	(3.324)	(2.2505)	(4.11)	(2.730)	(4.723)
Ethnicity (white = 1)	-0.025	-0.108	-0.017	-0.122	-0.014	0.035	0.110	0.020
	(0.137)	(0.152)	(0.153)	(0.231)	(0.212)	(0.339)	(0.216)	(0.342)
Education (years)	-1.044*	-0.662	-1.173*	-1.344	-1.178**	-1.643*	-0.748	-0.716
	(0.581)	(0.651)	(0.620)	(0.933)	(0.595)	(0.943)	(0.618)	(0.970)
Household income (log)	-0.497	-0.644	-0.063	-0.225	-1.106	-0.426	-0.932	0.112
	(0.648)	(0.699)	(0.717)	(0.991)	(0.794)	(1.227)	(0.841)	(1.424)
Labor status (employed = 1)	-0.107	0.182	0.027	-0.214	0.065	0.210	0.034	0.264
	(0.101)	(0.107)	(0.095)	(0.166)	(0.122)	(0.202)	(0.131)	(0.227)
Household size (#)	-0.045	0.019	-0.008	-0.041	-0.079	0.081	-0.304	0.094
	(0.235)	(0.258)	(0.260)	(0.400)	(0.388)	(0.580)	(0.417)	(0.627)
Number of bedrooms (#)	0.034	0.020	-0.021	0.016	-0.100	0.202	-0.034	0.081
	(0.130)	(0.145)	(0.142)	(0.210)	(0.168)	(0.252)	(0.175)	(0.274)
Sewage access (=1)	0.008	0.039	0.045	-0.028	-0.039	-0.054	-0.051	-0.163
	(0.086)	(0.102)	(0.096)	(0.134)	(0.114)	(0.185)	(0.118)	(0.200)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial degree	1	2	1	2	1	2	1	2
F-test	51.83***	16.81*	37.42***	8.07	11.96	18.17*	8.44	6.10
Subsample	Full	Full	Women	Women	Full	Full	Interior	Interior
Observations	900	900	804	804	539	539	390	390

Notes: This table tests for systematic covariate imbalances at the cutoffs, following Equation 3. Each column reports the coefficient of interest (β) from separate RDD regressions using the baselines covariate as outcomes. Control variables include sex, age, and region at baseline, and ethnicity (measured at wave 2, since it was unavailable at baseline), unless they are collinear with the outcome. F-tests report the value and significance of the joint null hypothesis that all estimated coefficients (through OLS) are zero. Columns (1) to (4) correspond to the AFAM-PE sample (bandwidth: $[-0.047; 0.073]$), and Columns (5) to (8) to TUS (bandwidth: $[-0.400; 0.400]$). Columns (3), (4), (7), and (8) report a robustness check using subsamples with less imbalances: women for AFAM-PE and interior (non-Montevideo) residents for TUS. Standard errors clustered by ICC* are shown in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 4:
RDD Impact Estimates of AFAM-PE and TUS on Stigma

	AFAM-PE				TUS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Sharp RDD estimates								
Personal stigma	0.255*	0.342**	0.490**	0.518**	0.407**	0.437***	0.428*	0.456*
	(0.165)	(0.168)	(0.234)	(0.229)	(0.191)	(0.213)	(0.234)	(0.242)
Social stigma	0.113	0.101	0.289	0.257	0.387***	0.418***	0.404*	0.398*
	(0.144)	(0.159)	(0.234)	(0.229)	(0.147)	(0.148)	(0.209)	(0.205)
Panel B: Fuzzy RDD estimates								
Personal stigma	0.337*	0.456**	0.631**	0.684**	0.624**	0.674***	0.759*	0.773*
	(0.191)	(0.213)	(0.299)	(0.305)	(0.254)	(0.260)	(0.421)	(0.410)
Social stigma	0.151	0.136	0.372	0.344	0.605***	0.654***	0.714*	0.675*
	(0.181)	(0.193)	(0.273)	(0.282)	(0.230)	(0.231)	(0.382)	(0.352)
Control variables	No	Yes	No	Yes	No	Yes	No	Yes
Polynomial degree	1	1	2	2	1	1	2	2
Observations	917	917	917	917	560	560	560	560

Notes: This table reports parametric RDD impact estimates of program impacts on the standardized indices (z-scores) of personal stigma (shame) and social stigma (mistreatment). All coefficients (β) are from separate regressions and measure the causal impact of program participation. Panel A reports sharp RDD (OLS) estimates, while Panel B reports fuzzy RDD (2SLS) estimates, corresponding to Equation (1) and Equation (3). Columns (1)-(4) show estimates for the AFAM-PE program (bandwidth: [-0.047; 0.073]) and columns (5)-(8) show estimates for the TUS program (bandwidth: [-0.400 ; 0.400]). Control variables include sex (woman = 1), age, region (Montevideo = 1) and ethnicity (white = 1). Figure 3 shows the sharp (linear and quadratic) estimates with controls in graphical form. Standard errors clustered by ICC* are shown in parenthesis. *** p<0.01, ** p<0.05, * p<0.10.

Table 5:
Heterogeneity Analysis by Program Tenure

	AFAM-PE		TUS	
	(1)	(2)	(3)	(4)
Panel A: Personal stigma				
$\widehat{T_i}$	0.389*	0.519**	0.499*	0.544*
	(0.214)	(0.224)	(0.321)	(0.321)
$T_i \times \widehat{\mathbf{1}\{\text{tenure}_i \geq \text{tenure}_{p50}\}}$	-0.085	-0.097	0.237	0.244
	(0.239)	(0.232)	(0.370)	(0.365)
F-Test	1.76	2.84*	6.14**	6.79***
Panel B: Social stigma				
$\widehat{T_i}$	0.129	0.121	0.616**	0.678**
	(0.217)	(0.223)	(0.286)	(0.282)
$T_i \times \widehat{\mathbf{1}\{\text{tenure}_i \geq \text{tenure}_{p50}\}}$	0.033	0.025	-0.124	-0.152
	(0.208)	(0.205)	(0.331)	(0.327)
F-Test	0.64	0.47	3.30*	3.65*
Control variables	No	Yes	No	Yes
Polynomial degree	1	1	1	1
Observations	917	917	560	560

Notes: This table reports heterogeneous RDD estimates of program impacts by program tenure on the standardized indices (z-scores) of shame (Panel A) and mistreatment (Panel B). The first row in each panel shows the parameter of interest (β), while the second row shows its interaction with an indicator for welfare tenure ($T_i \times \widehat{\mathbf{1}\{\text{tenure}_i \geq \text{tenure}_{p50}\}}$), which represents the additional causal effect for individuals with tenure at or above the median. Median tenure is 64 months for AFAM-PE and 98 months for TUS (between August 2009 and September 2018). Columns (1)-(2) show impact estimates for the AFAM-PE program (bandwidth: [-0.047; 0.073]) and columns (3)-(4) show estimates for the TUS program (bandwidth: [-0.400 ; 0.400]). Control variables include sex (woman = 1), age, region (Montevideo = 1) and ethnicity (white = 1). Standard errors clustered by ICC* are shown in parenthesis. *** p<0.01, ** p<0.05, * p<0.10.

Table 6:
RDD Impact Estimates of AFAM-PE and TUS on Other Perceptions

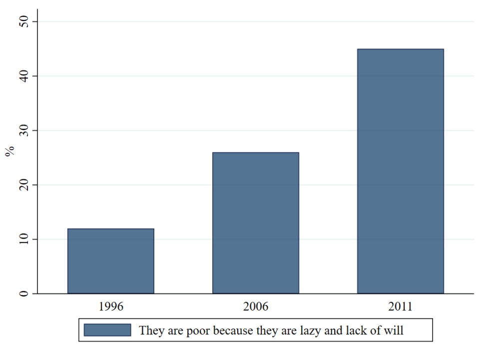
	AFAM-PE				TUS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Perceived position in income distribution	0.328	0.174	0.085	-0.047	-0.698*	-0.723*	-0.025	-0.021
	(0.280)	(0.294)	(0.673)	(0.413)	(0.404)	(0.397)	(0.973)	(0.636)
Observations	917	917	917	917	560	560	560	560
Recipients should feel ashamed of themselves	-0.007	0.008	-0.042	-0.044	-0.027	-0.034	-0.009	-0.023
	(0.043)	(0.044)	(0.062)	(0.065)	(0.032)	(0.033)	(0.054)	(0.051)
Observations	905	905	905	905	555	555	555	555
Grade of support towards the program	0.016	-0.123	0.039	-0.084	-0.264	-0.275	-0.451	-0.493
	(0.160)	(0.171)	(0.213)	(0.227)	(0.274)	(0.276)	(0.457)	(0.430)
Observations	878	878	878	878	554	554	554	554
Ashamed of appearance	0.105	0.113	0.228**	0.248**	0.055	0.056	-0.029	-0.008
	(0.078)	(0.075)	(0.109)	(0.114)	(0.109)	(0.109)	(0.178)	(0.165)
Observations	915	915	915	915	556	556	556	556
Control variables	No	Yes	No	Yes	No	Yes	No	Yes
Polynomial degree	1	1	2	2	1	1	2	2

Notes: This table reports parametric RDD impact estimates of program impacts on the standardized indices (z-scores) of personal stigma (shame) and social stigma (mistreatment). All coefficients (β) are from separate regressions and measure the causal impact of program participation. Panel A reports sharp RDD (OLS) estimates, while Panel B reports fuzzy RDD (2SLS) estimates, corresponding to Equation (1) and Equation (3). Columns (1)-(4) show estimates for the AFAM-PE program (bandwidth: [-0.047; 0.073]) and columns (5)-(8) show estimates for the TUS program (bandwidth: [-0.400 ; 0.400]). Perceived position in income distribution comes from the question: "*Imagine a scale from 1 to 10, where in 1 are the poorest and in 10 the richest people, where do you place yourself?*". Recipients should feel ashamed of themselves refers to answers to: "*Do you agree with the statement that people who receive AFAM-PE (or TUS) should be ashamed of themselves?*" (choices are 0 (No) or 1 (Yes)). Grade of support towards the program references two different questions. For AFAM-PE: "*Do you think that AFAM-PE benefits should be provided less in cash and a part should be given through a food card? (It is always the same money)*" (choice set ranges from 1 (strongly disagree) to 5 (strongly agree)). For TUS: '*'Do you think that TUS is a...?*" (choice set ranges from 1 (very bad benefit) to 5 (very good benefit)). Ashamed of appearance refers to the question: "*Have you thought about not attending or have you not attended a work, family or social event during the last month because you felt you did not have the clothes or appearance required for that venue?*" (choices are 0 (No) or 1 (Yes)). Control variables include sex (woman = 1), age, region (Montevideo = 1) and ethnicity (white = 1). Standard errors clustered by ICC* are shown in parenthesis. *** p<0.01, ** p<0.05, * p<0.10.

A Figures and Tables

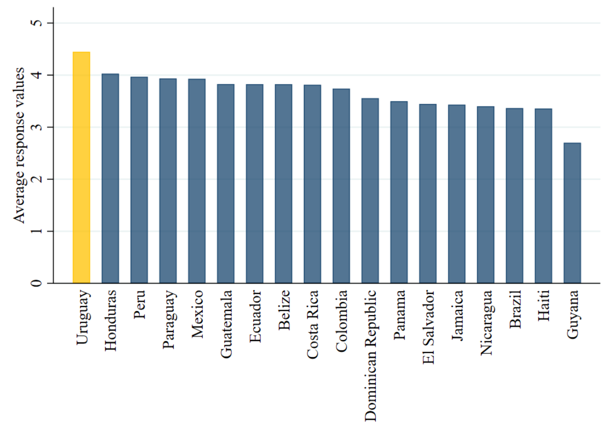
Figure A1:
Beliefs Towards Poverty and Welfare in Uruguay

(a) Why are there people in need in Uruguay?



Source: Zuasnabar (2018)

(b) Do you agree that those who are on welfare are lazy?



Source: Americas Barometer (2012)

Notes: These figure shows survey data about trends and international comparisons of Uruguayans' beliefs towards the poor and those on welfare. Panel (a) shows data from the World Value Survey (WVS) for the available three years between 1996 and 2011. It shows people´s beliefs of why there are still people in poverty, specifically the % of people in Uruguay who answer the World Value Survey's question "*Why are there people in need in Uruguay*" with the following response: "*Because they are lazy and lack of will*". Panel (b) shows data for Latin America and the Caribbean from the AmericasBarometer (LAPOP) for 2012. It displays the average answer values from each available country to this question: "*Some people say that those who receive social assistance from government programs are lazy. How much do you agree or disagree?*".

Figure A2:
Card Comparison Across Programs

(a) AFAM-PE debit card

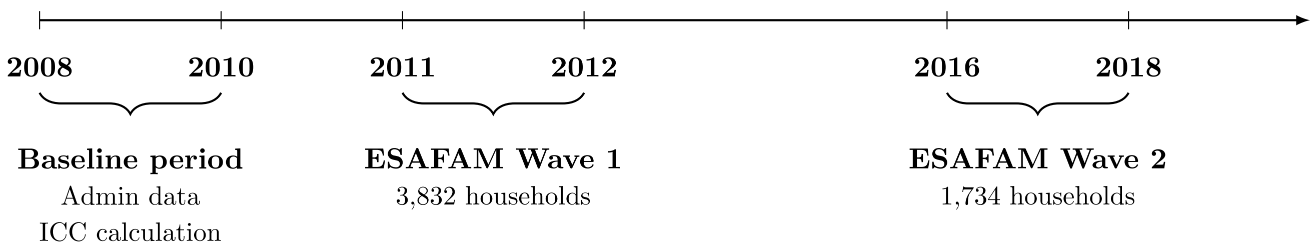
Source: BROU

(b) TUS food card

Source: BROU

Notes: These figures show the layouts of the magnetic cards through which each cash transfer is delivered to households. Panel (a) exhibits an example of a debit card for AFAM-PE. Panel (b) exhibits the only available food card for TUS at the time of the study period, showing its governmental and social plan stamp.

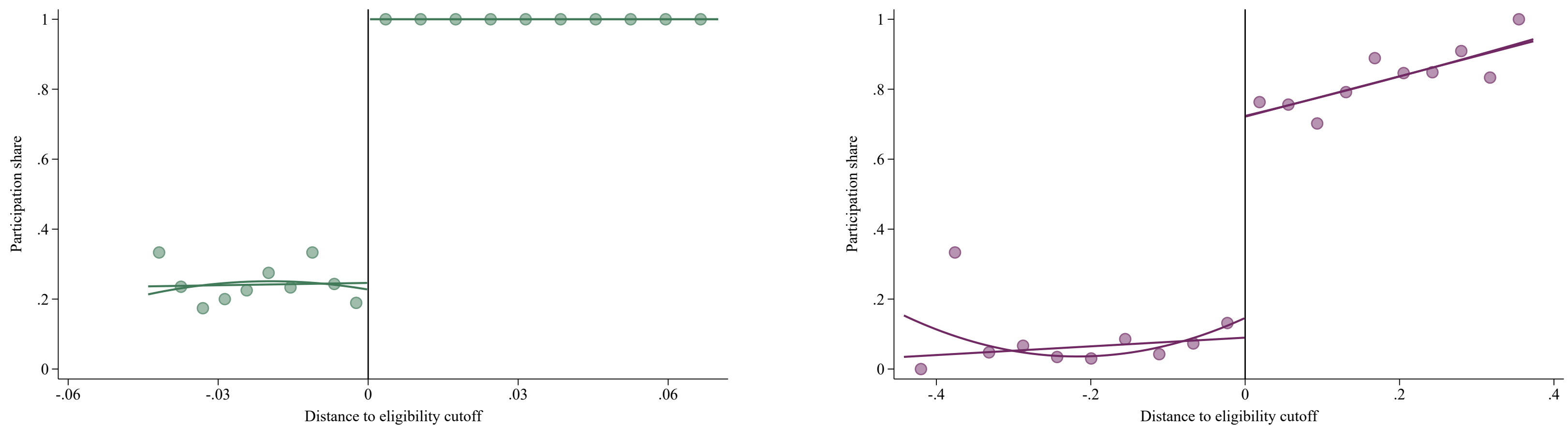
Figure A3:
Timeline of the Study



Notes: This figure illustrates the timeline of the study. The baseline period (2008–2010) provides administrative data used to determine households' eligibility index. The ESAFAM survey was conducted in two waves: Wave 1 (2011–2012, $n = 3{,}832$) and Wave 2 (2016–2018, $n = 1{,}734$). Stigma-related outcomes (shame and mistreatment/discrimination) are only available in Wave 2, allowing analysis of medium- to long-term effects 6–10 years after program enrollment. The study sample consists of 917 households for AFAM-PE and 595 for TUS after excluding observations with missing values.

Figure A4:
First Stage Estimates
(Twenty Point Bins)

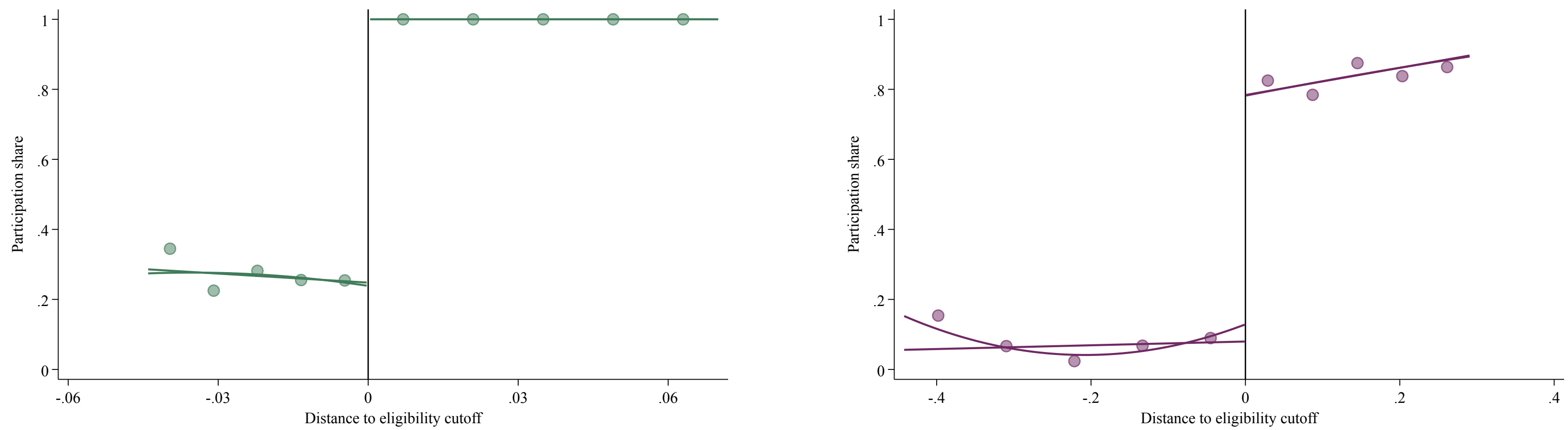
(a) AFAM-PE **(b)** TUS



Notes: These figures show first-stage estimates of program participation as a function of the normalized eligibility score, the Índice de Carencias Críticas (ICC*). Dots indicate the average share of participant households within twenty-bin intervals, ten on each side of the cutoff, marked by a vertical line at zero. Panel (a) reports results for AFAM-PE, and Panel (b) for TUS, using bandwidths of [–0.047, 0.073] and [–0.400, 0.400], respectively. Solid lines show fitted linear and quadratic trends.

Figure A5:
First Stage Estimates
(Subsample Analyses)

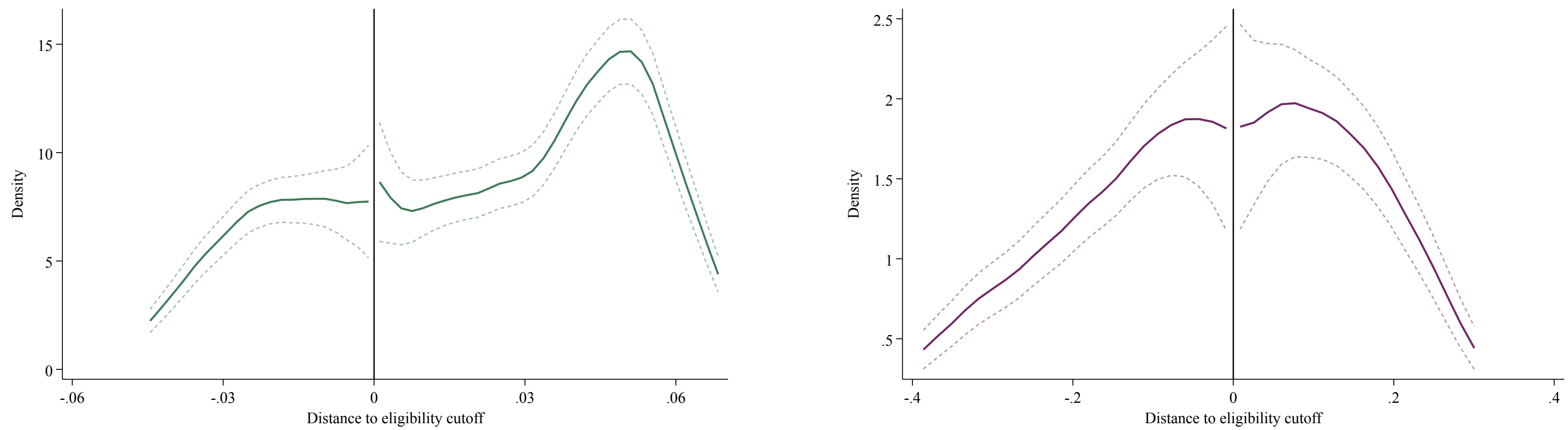
(a) AFAM-PE **(b)** TUS



Notes: These figures show first-stage estimates of program participation as a function of the normalized eligibility score, the Índice de Carencias Críticas (ICC*), for subsamples of the main analysis. Dots indicate the average share of participant households within twenty-bin intervals, ten on each side of the cutoff, marked by a vertical line at zero. Panel (a) reports results for AFAM-PE's women subsample, and Panel (b) for TUS's interior (non-Montevideo) subsample, using bandwidths of [–0.047, 0.073] and [–0.400, 0.400], respectively. Solid lines show fitted linear and quadratic trends. The discontinuities at the cutoff correspond to the estimated first-stage jumps in participation: approximately 0.74 for AFAM-PE and 0.72 for TUS.

Figure A6:
McCrary Density Tests
(Subsample Analyses)

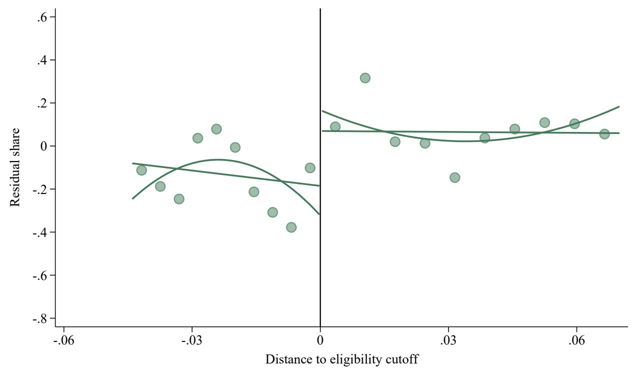
(a) AFAM-PE **(b)** TUS



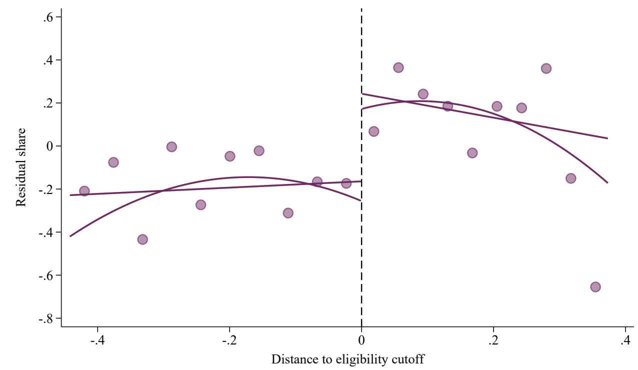
Notes: These figures show McCrary (2008) density tests for the subsample analyses to check for systematic bunching at the cutoffs (vertical black line at zero) of the AFAM-PE and TUS programs. The solid thick line is the density estimate (local linear regression with separate trends), and dashed lines represent 95% confidence intervals. Panel (a) uses a bandwidth of $[-0.047; 0.073]$ for the normalized eligibility score in the AFAM-PE's women subsample, while Panel (b) uses a bandwidth of $[-0.400; 0.400]$ for the score in the TUS's interior (non-Montevideo) subsample. The estimated log differences in density height at the cutoff are $\hat{\tau} = 0.141$ (S.E. 0.263) for AFAM-PE and $\hat{\tau} = 0.011$ (S.E. 0.291) for TUS. In both cases, the null hypothesis of continuity is not rejected. Recent tests confirm this conclusion: the p-values for the Cattaneo et al. (2018) test are 0.143 (AFAM-PE) and 0.844 (TUS), and for the Bugni and Canay (2021) test are 0.401 (AFAM-PE) and 1.000 (TUS).

Figure A7:
RDD Impact Estimates of AFAM-PE and TUS on Stigma
(Twenty Point Bins)

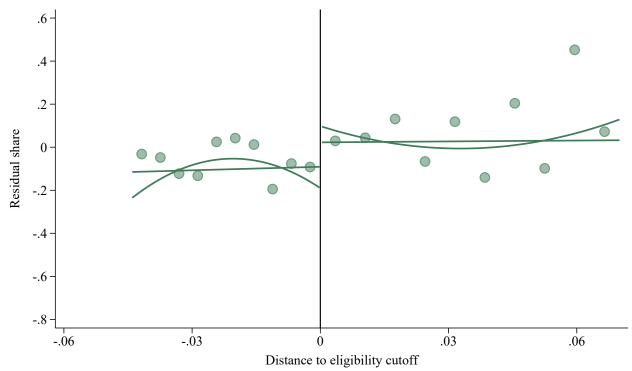
(a) AFAM-PE - Personal Stigma



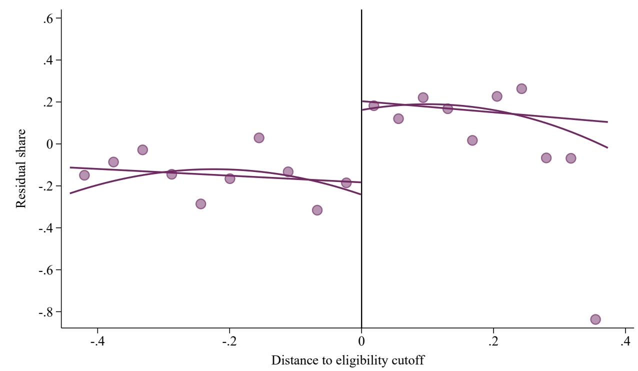
(b) TUS - Personal Stigma



(c) AFAM-PE - Social Stigma

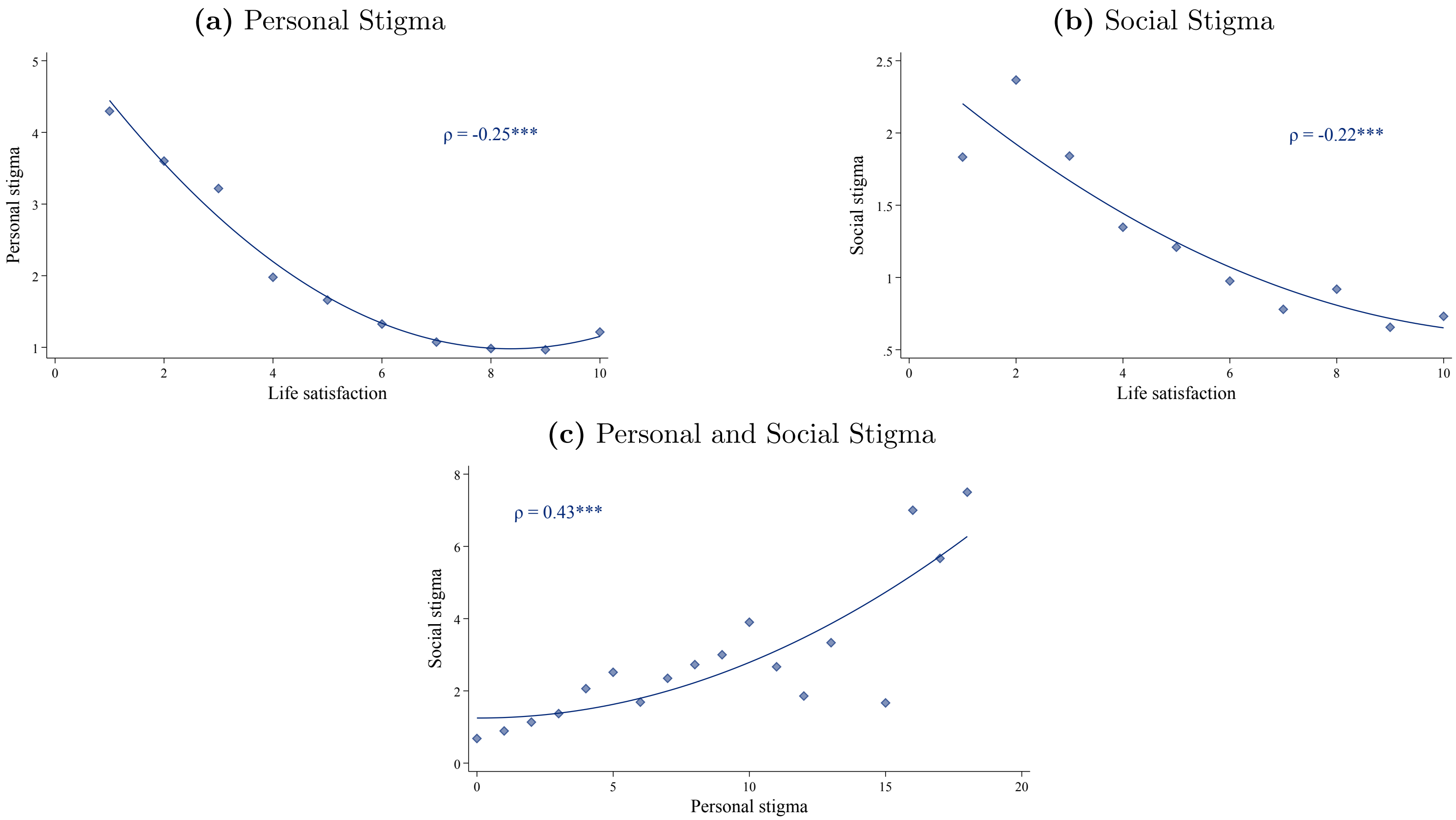


(d) TUS - Social Stigma



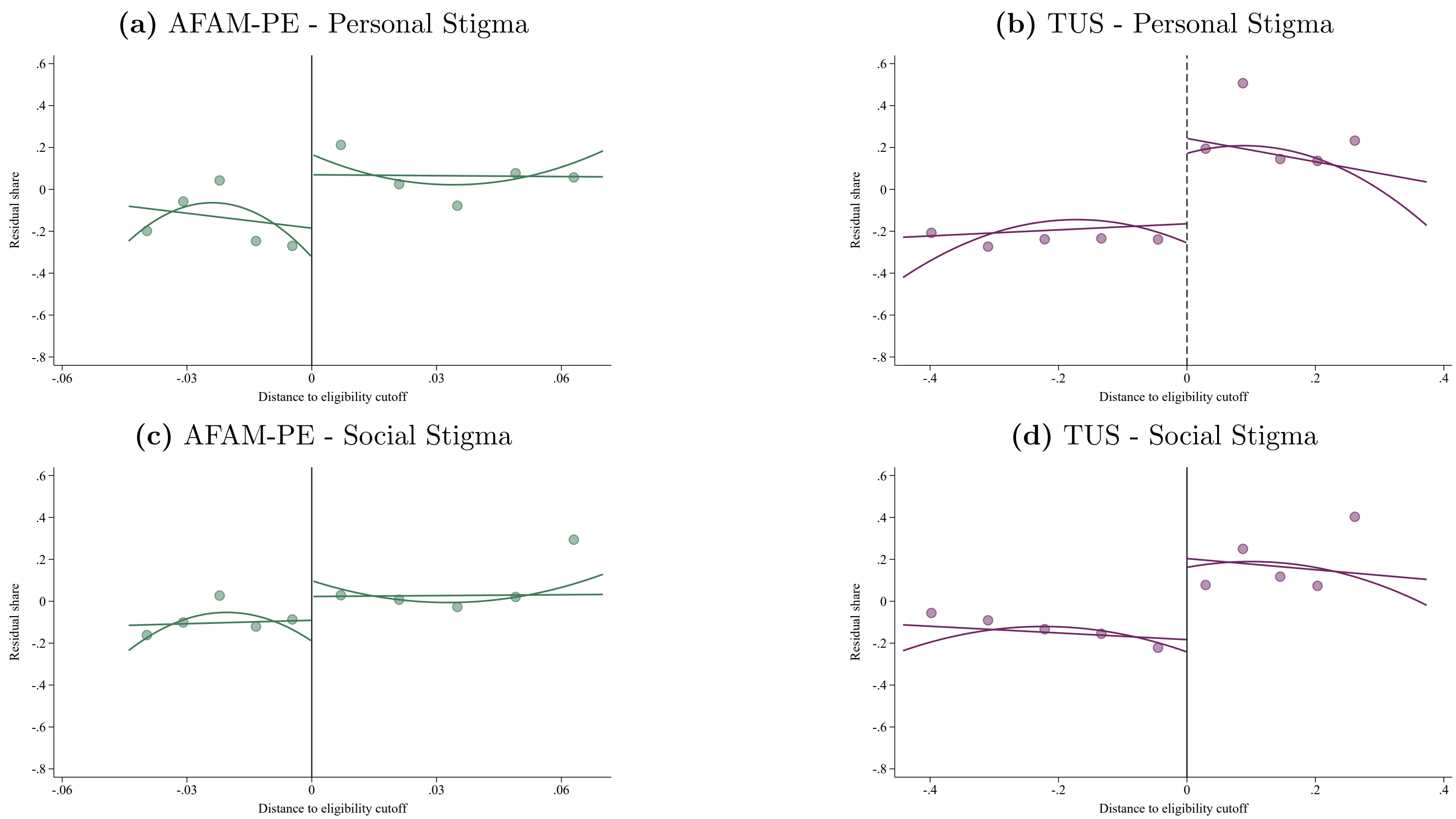
Notes: These figures show parametric RDD estimates of program impacts on the standardized indices (z-scores) of personal stigma (shame) and social stigma (mistreatment). Dots represent conditional mean values of each index across twenty-bin intervals, ten on each side of the normalized eligibility cutoff (marked by a vertical line at zero). Panels (a) and (c) correspond to AFAM-PE, estimated using a [-0.047; 0.073] bandwidth, while Panels (b) and (d) correspond to TUS, using a [-0.400; 0.400] bandwidth. Solid lines show linear and quadratic fitted values. Control variables include sex (woman = 1), age, region (Montevideo = 1) and ethnicity (white = 1).

Figure A8:
Correlation Between Stigma and Subjective Wellbeing Variables



Notes: These figures show binscatters (with an overlaid quadratic fit) between personal stigma (shame), social stigma (mistreatment) and subjective wellbeing (life satisfaction). The figures pool observations from both AFAM-PE and TUS samples. Stigma indices are constructed as simple sums of their corresponding items (non-standardized), while life satisfaction is based on responses to the question: *"On a scale from 1 to 10, where 1 means very dissatisfied and 10 means very satisfied, how satisfied are you with your life in general?"*. Correlation coefficients are reported in each panel. *** p<0.01, ** p<0.05, * p<0.10.

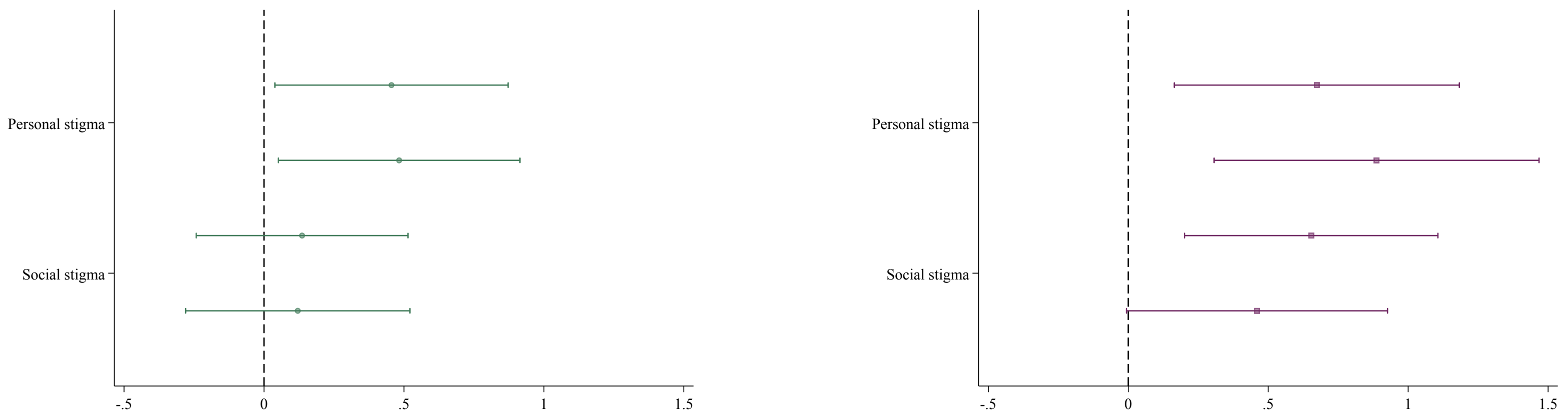
Figure A9:
RDD Impact Estimates of AFAM-PE and TUS on Stigma
(Subsample Analyses)



Notes: These figures show show robustness checks for subsample analyses. They plot parametric RDD estimates of program impacts on the standardized indices (z-scores) of personal stigma (shame) and social stigma (mistreatment). Dots represent conditional mean values of each index across ten-bin intervals, five on each side of the normalized eligibility cutoff (marked by a vertical line at zero). Panels (a) and (c) correspond to AFAM-PE's women subsample, estimated using a [-0.047; 0.073] bandwidth, while Panels (b) and (d) correspond to TUS's interior (non-Montevideo) subsample, using a [-0.400; 0.400] bandwidth. Solid lines show linear and quadratic fitted values. Control variables include sex (woman = 1), age, region (Montevideo = 1) and ethnicity (white = 1).

Figure A10:
Robustness Check: Subsample Analyses
(Subsample vs. Full Sample, RDD Impact Estimates)

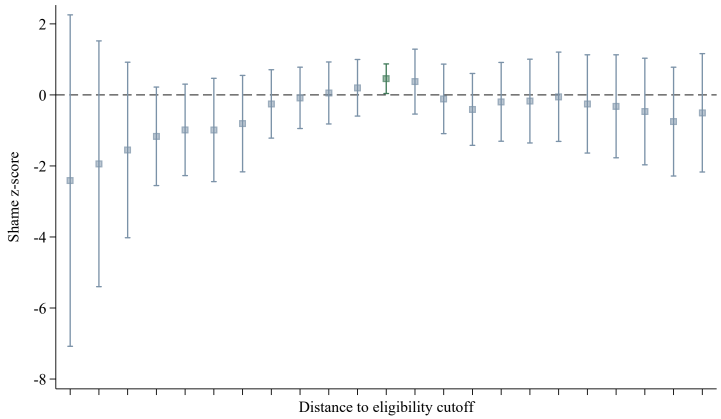
(a) AFAM-PE **(b)** TUS



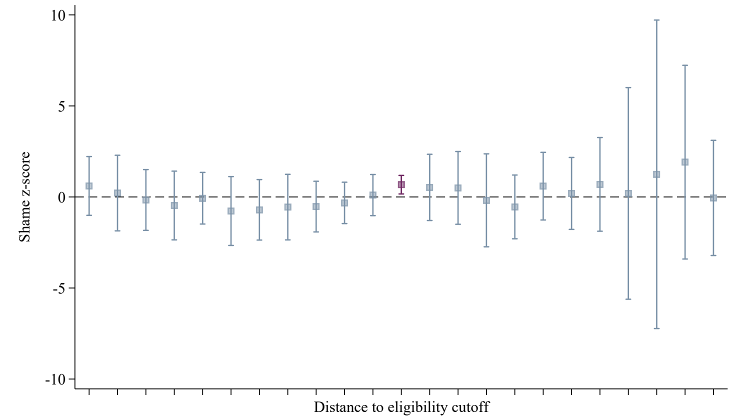
Notes: These figures show robustness checks for subsample analyses. They plot the main coefficients of interest β, which represent the effect of each program on each outcome and are obtained through the linear specification with controls of Equation 3. Results compare the full sample estimates versus estimates for the subsample, which only include women for AFAM-PE and interior (non-Montevideo) residents for TUS. The first and third coefficients in each panel correspond to the full sample estimates, while the second and fourth correspond to the subsample analyses. Control variables include sex (woman = 1), age, region (Montevideo = 1) and ethnicity (white = 1), depending on the subsample. 95% confidence intervals are displayed.

Figure A11:
Robustness Check: Placebo Estimates

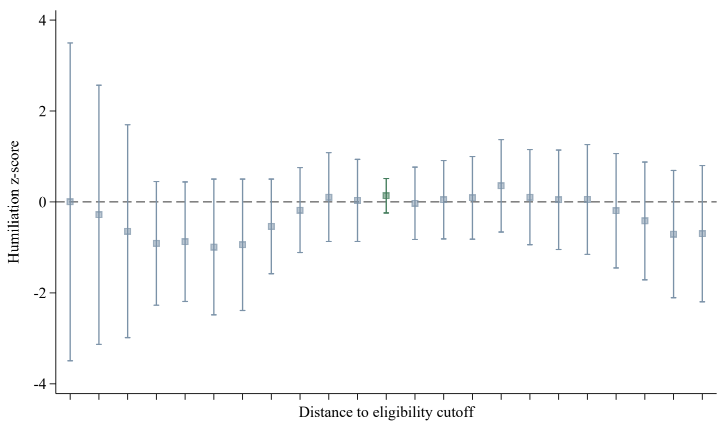
(a) AFAM-PE - Personal Stigma



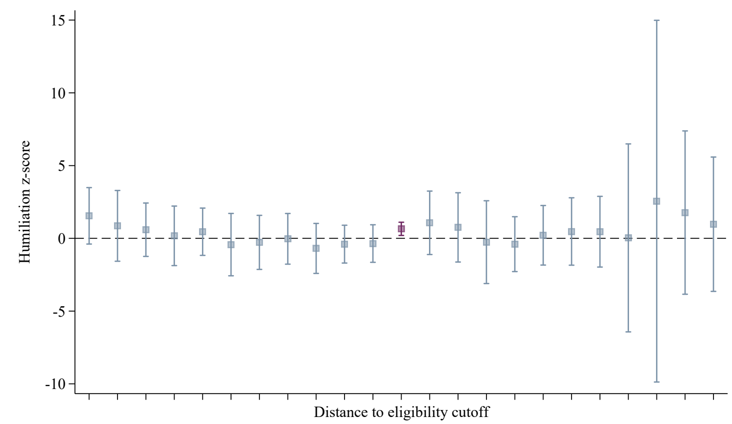
(b) TUS - Personal Stigma



(c) AFAM-PE - Social Stigma



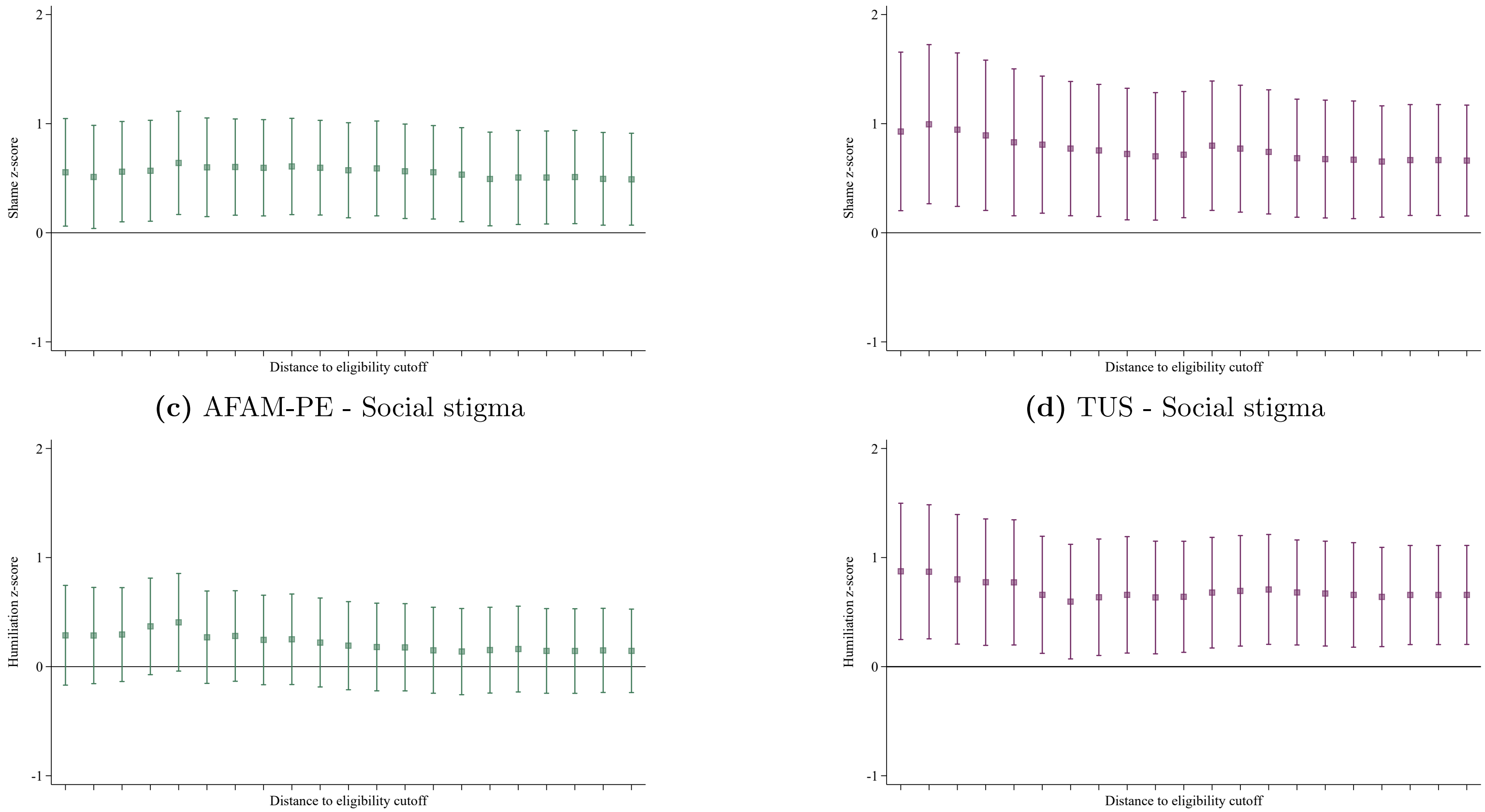
(d) TUS - Social Stigma



Notes: These figures show robustness by reporting placebo estimates for the main outcomes of interest: personal stigma (shame) and social stigma (mistreatment), for each program. They plot coefficients of interest (β), which represent the effect of each program on each outcome. The alternative cutoffs considered for AFAM-PE range from [-0.020, -0.019, ..., -0.011 -0.010] to [0.01, 0.011. ... 0.019, 0.020], while for TUS these range from [-0.20, -0.19, ..., -0.11 -0.10] to [0.10, 0.11 ... 0.19, 0.20]. These estimates are displayed in gray, while real impact estimates (at the zero cutoffs) are highlighted in their usual colors. Estimates are obtained through regressions following the preferred specification: a linear Fuzzy RDD with controls, as described in Equation 3. Panels (a) and (c) show estimates for the AFAM-PE program while Panels (b) and (d) show estimates for the TUS program. Control variables include sex (woman = 1), age, region (Montevideo = 1) and ethnicity (white = 1). Standard errors clustered by ICC*. 95% confidence intervals are displayed.

Figure A12:
Robustness Test: Alternative Bandwidth Selections

(a) AFAM-PE - Personal stigma　**(b)** TUS - Personal stigma
(c) AFAM-PE - Social stigma　**(d)** TUS - Social stigma



Notes: These figures show robustness by reporting alternative estimates for the main outcomes of interest: personal stigma (shame) and social stigma (mistreatment), for different bandwidth selections over the normalized eligibility scores for each program. They plot coefficients of interest (β), which represent the effect of each program on each outcome. Each estimate is computed by trimming the tails of the running variable by making progressive 0.001 (0.01) cuts from each side of the samples. The graph reads from right to left: in each panel, the first coefficient is the baseline estimate without any trimming. Onward, each estimate trims an additional 0.001 (0.01) cut until reaching a 0.02 (0.2) cut at each tail of the normalized running variable of each program. Coefficients are obtained through regressions following the preferred specification: a linear Fuzzy RDD with controls, as described in Equation 3. Panels (a) and (c) show estimates for the AFAM-PE program while Panels (b) and (d) show estimates for the TUS program. Control variables include sex (woman = 1), age, region (Montevideo = 1) and ethnicity (white = 1). Standard errors clustered by ICC*. 95% confidence intervals are displayed.

Table A1:
RDD Impact Estimates of AFAM-PE and TUS on Individual Survey Items

	AFAM-PE		TUS	
	(1)	(2)	(3)	(4)
Panel A: Personal stigma items				
Self-conscious	0.395	0.497	0.441	0.482
	(0.172)**	(0.177)***	(0.256)*	(0.258)*
	[0.172]	**[0.177]****	**[0.256]**	**[0.258]**
Ridiculous	0.056	0.153	0.012	0.035
	(0.186)	(0.202)	(0.232)	(0.241)
	[0.186]	**[0.202]**	**[0.232]**	**[0.241]**
Embarrassed	0.247	0.334	0.469	0.490
	(0.182)	(0.196)*	(0.248)*	(0.252)*
	[0.182]	**[0.196]**	**[0.248]**	**[0.252]**
Humiliated	0.163	0.205	0.460	0.544
	(0.168)	(0.178)	(0.240)*	(0.239)**
	[0.168]	**[0.178]**	**[0.240]**	**[0.239]***
Laughable	0.276	0.369	0.636	0.640
	(0.170)	(0.191)*	(0.228)***	(0.234)***
	[0.170]	**[0.191]**	**[0.228]****	**[0.234]****
Helpless	0.154	0.203	0.632	0.677
	(0.198)	(0.221)	(0.271)**	(0.273)**
	[0.198]	**[0.221]**	**[0.271]***	**[0.273]****
Panel B: Social stigma items				
Disrespect	0.232	0.119	0.366	0.367
	(0.174)	(0.179)	(0.263)	(0.262)
	[0.174]	**[0.179]**	**[0.263]**	**[0.262]**
Unfairness	0.009	0.049	0.499	0.563
	(0.190)	(0.201)	(0.249)**	(0.249)**
	[0.190]	**[0.201]**	**[0.249]**	**[0.249]***
Discrimination	0.028	0.115	0.433	0.472
	(0.183)	(0.200)	(0.228)*	(0.229)**
	[0.183]	**[0.200]**	**[0.228]**	**[0.229]***
Control variables	No	Yes	No	Yes
Polynomial degree	1	1	1	1
Observations	917	917	560	560

Notes: This table reports parametric RDD impact estimates of program impacts on the items comprising the main composite personal and social stigma scales. All coefficients (β) are from separate regressions and measure the causal impact of program participation. Panel A reports sharp RDD (OLS) estimates, while Panel B reports fuzzy RDD (2SLS) estimates, corresponding to Equation (1) and Equation (3). Columns (1)-(4) show estimates for the AFAM-PE program (bandwidth: [-0.047; 0.073]) and columns (5)-(8) show estimates for the TUS program (bandwidth: [-0.400 ; 0.400]). Control variables include sex (woman = 1), age, region (Montevideo = 1) and ethnicity (white = 1). Table A2 reports results for those items that are excluded from the main scale. Standard errors clustered by ICC* are shown in parenthesis. Romano-Wolf multiple hypotheses adjusted standard p-values are marked in bold and between square brackets. *** p<0.01, ** p<0.05, * p<0.10.

Table A2:
RDD Impact Estimates of AFAM-PE and TUS on Excluded Survey Items

	AFAM-PE		TUS	
	(1)	(2)	(3)	(4)
Stupid	-0.094	0.066	0.134	0.125
	(0.184)	(0.182)	(0.227)	(0.231)
	[0.184]	**[0.182]**	**[0.227]**	**[0.231]**
Childish	-0.266	-0.113	0.240	0.297
	(0.203)	(0.196)	(0.240)	(0.238)
	[0.203]	**[0.196]**	**[0.240]**	**[0.238]**
Blushing	-0.045	-0.021	0.032	0.017
	(0.182)	(0.187)	(0.252)	(0.249)
	[0.182]	**[0.187]**	**[0.252]**	**[0.249)**
Disgusting	0.220	0.282	0.499	0.534
	(0.186)	(0.192)	(0.202)**	(0.207)***
	[0.186]	**[0.192]**	**[0.202]***	**[0.207]****
Control variables	No	Yes	No	Yes
Polynomial degree	1	1	1	1
Observations	917	917	560	560

Notes: This table reports parametric RDD impact estimates of program impacts on the items excluded from the main aggregated indices through the PCA analysis. All coefficients (β) are from separate regressions and measure the causal impact of program participation. Panel A reports sharp RDD (OLS) estimates, while Panel B reports fuzzy RDD (2SLS) estimates, corresponding to Equation (1) and Equation (3). Columns (1)-(4) show estimates for the AFAM-PE program (bandwidth: [-0.047; 0.073]) and columns (5)-(8) show estimates for the TUS program (bandwidth: [-0.400 ; 0.400]). Control variables include sex (woman = 1), age, region (Montevideo = 1) and ethnicity (white = 1). Standard errors clustered by ICC* are shown in parenthesis. Romano-Wolf multiple hypotheses adjusted standard p-values are marked in bold and between square brackets. *** p<0.01, ** p<0.05, * p<0.10.

Table A3:
Robustness Check: Estimates for Alternative Bandwidth Selection

	AFAM-PE		TUS	
	(1)	(2)	(3)	(4)
Panel A:	*Cut 0.01 from tails*		*Cut 0.1 from tails*	
Personal Stigma	0.494**	0.573***	0.671**	0.716**
	(0.217)	(0.222)	(0.290)	(0.295)
Social Stigma	0.223	0.192	0.609**	0.641**
	(0.203)	(0.206)	(0.264)	(0.260)
Bandwidth	$[-0.03, 0.06]$	$[-0.03, 0.06]$	$[-0.3, 0.3]$	$[-0.3, 0.3]$
Observations	811	811	515	515
Panel B:	*Cut 0.015 from tails*		*Cut 0.15 from tails*	
Personal Stigma	0.539**	0.600***	0.761**	0.808**
	(0.229)	(0.231)	(0.315)	(0.321)
Social Stigma	0.289	0.270	0.663**	0.659**
	(0.212)	(0.216)	(0.281)	(0.274)
Bandwidth	$[-0.025, 0.055]$	$[-0.025, 0.055]$	$[-0.25, 0.25]$	$[-0.25, 0.25]$
Observations	738	738	480	480
Panel C:	*Cut 0.02 from tails*		*Cut 0.2 from tails*	
Personal Stigma	0.493**	0.554**	0.882**	0.929**
	(0.248)	(0.252)	(0.365)	(0.371)
Social Stigma	0.311	0.288	0.907***	0.873***
	(0.232)	(0.233)	(0.338)	(0.304)
Bandwidth	$[-0.02, 0.05]$	$[-0.02, 0.05]$	$[-0.2, 0.2]$	$[-0.2, 0.2]$
Observations	567	567	405	405
Control variables	No	Yes	No	Yes
Polynomial degree	1	1	1	1

Notes: This table shows robustness by reporting alternative estimates of program impacts on the standardized indices (z-scores) of personal stigma (shame) and social stigma (mistreatment). All coefficients (β) are from separate regressions and measure the causal impact of program participation. Panel A reports results for a trimmed running variable with 0.01 (0.1) cuts from each tail for the AFAM-PE (TUS) sample. Meanwhile, Panel B shows results for 0.015 (0.15) cuts, and Panel C shows results for 0.02 (0.2) for the AFAM-PE (TUS) sample. Estimates are obtained through regressions following the preferred specification: a linear fuzzy RDD, as described in Equation 3. Control variables include sex (woman = 1), age, region (Montevideo = 1) and ethnicity (white = 1). A12 shows graphical the same results in graphical form for additional cutoff trimmings. Standard errors clustered by ICC* are shown in parenthesis. *** p<0.01, ** p<0.05, * p<0.10. Figure

Table A4:
Robustness Check: Donut RDD Estimates

	AFAM-PE		TUS	
	(1)	(2)	(3)	(4)
Panel A:	$n \notin [-0.001, 0.001]$		$n \notin [-0.01, 0.01]$	
Personal stigma	0.434**	0.566***	0.568**	0.620**
	(0.195)	(0.218)	(0.257)	(0.264)
Social stigma	0.104	0.090	0.638***	0.680***
	(0.188)	(0.200)	(0.230)	(0.232)
Observations	904	904	541	541
Panel B:	$n \notin [-0.005, 0.005]$		$n \notin [-0.05, 0.05]$	
Personal stigma	0.457**	0.580**	0.767**	0.830**
	(0.214)	(0.228)	(0.327)	(0.334)
Social stigma	0.079	0.043	0.643**	0.707**
	(0.217)	(0.224)	(0.299)	(0.304)
Observations	840	840	472	472
Control variables	No	Yes	No	Yes
Polynomial degree	1	1	1	1

Notes: This table shows robustness by reporting donut RDD estimates of program impacts on the standardized indices (z-scores) of personal stigma (shame) and social stigma (mistreatment). All coefficients (β) are from separate regressions and measure the causal impact of program participation. Panel A shows results for a trimmed running variable with 0.001 (0.01) cuts right next to the threshold of AFAM-PE (TUS). Panel B shows results for 0.005 (0.05) cuts for the AFAM-PE (TUS) sample. Estimates are obtained through regressions following the preferred specification: a linear fuzzy RDD, as described in Equation 3. Control variables include sex (woman = 1), age, region (Montevideo = 1) and ethnicity (white = 1). Standard errors clustered by ICC* are shown in parenthesis. *** p<0.01, ** p<0.05, * p<0.10.

Table A5:
Robustness Check: Non-Parametric RDD Estimates

	AFAM-PE		TUS	
	(1)	(2)	(3)	(4)
Panel A: Uniform kernel				
Personal stigma	0.641**	0.686**	0.732*	0.757**
	(0.307)	(0.308)	(0.379)	(0.374)
Social stigma	0.379	0.340	0.692**	0.667**
	(0.278)	(0.277)	(0.342)	(0.336)
Panel B: Triangular kernel				
Personal stigma	0.634**	0.679**	0.785*	0.816**
	(0.308)	(0.308)	(0.411)	(0.399)
Social stigma	0.377	0.340	0.705*	0.687*
	(0.277)	(0.278)	(0.377)	(0.362)
Panel C: Epanechnikov kernel				
Personal stigma	0.641**	0.686**	0.799**	0.829**
	(0.307)	(0.308)	(0.400)	(0.391)
Social stigma	0.379	0.340	0.729**	0.705**
	(0.277)	(0.278)	(0.364)	(0.352)
Control variables	No	Yes	No	Yes
Polynomial degree	1	1	1	1
Observations	917	917	560	560

Notes: This table shows robustness by reporting non-parametric estimates Cattaneo et al. (2018) of program impacts on the standardized indices (z-scores) of personal stigma (shame) and social stigma (mistreatment). All coefficients (β) are from separate regressions and measure the causal impact of program participation. Estimates are obtained through regressions following the preferred fuzzy RDD specification, local polynomials of first order. Estimates of β are bias-corrected, following Calonico et al. (2014). Panel A shows results using a uniform kernel specification, while Panel B shows results with a triangular kernel, and Panel C with a epanechnikov kernel. Control variables include sex (woman = 1), age, region (Montevideo = 1) and ethnicity (white = 1). Robust standard errors (clustered by ICC*) following Calonico et al. (2014). *** p<0.01, ** p<0.05, * p<0.10.

B Quotes from Qualitative Studies

This appendix presents existing qualitative evidence from interviews and focus groups, with general population, as well as with recipients of AFAM-PE and TUS. The statements highlight two issues. First, these underscore the prevalent stigmatizing narratives in Uruguay, highlighting the strong notion of the "undeserving poor". Second, these testimonies describe the lived experiences of beneficiaries, highlighting the perceived discrimination they face when using the TUS food card in stores.

Firstly, anecdotal evidence of negative beliefs towards the poor and those on welfare that include these negative stereotypes about them can be found in Rivero (2020):

> *"I think that those who really need it are not helped. There are single mothers, who have four hundred children and get the allowance, but you work, you have your wage, and your daughter is not entitled to it. (...) They help the scoundrel, not the poor"*

> *"The State helps those who do nothing, not to the poor. You don't have to do formal work. If you are a single mother and you do not have a job (...) you go to MIDES, you apply, and they just give you money on the side"*

> *"MIDES gives them money not to work, they do not teach them how to work, or give them a plot to work the land (...). People are getting used to not work"*

> *"I believe that some people are poor because they want to be. If you are poor and you want to get by, and Christmas or Easter comes, you are not going to take out a loan to buy 20 Easter eggs. They have little reasoning. I think that people who go around asking for coins could very well be asking for small works instead of coins."*

Concomitantly, quotes from Moreno et al. (2014) include negative beliefs, particular to TUS recipients and the TUS program itself, as well. These follow:

> *" I went to a meeting at the communal center before the card came out and it was something else. It asked for conditionalities. Supposedly, it was going to imply work for the people...for me this [the card] does not contribute at all. You generate people who*

are scratching all day long and you give them money. Give them work instead, better living conditions...”

“ Some who get it, deserve it, others don’t deserve it. Others who have asked for it and need it, they don’t get it. There is a bit of everything.”

Thirdly, testimonies documented in Moreno et al. (2014) point out that TUS beneficiaries report feeling discriminated and treated unjustly in the groceries stores where they use the food card. Some translated testimonies are:

“The people [from the store] look at you as if you were stealing... as if he was discriminating you.”

“If the supermarket is very crowded, they say no, and you have to wait several days.”

“They make a face [grocery store employees], like, I don’t know what... they tell you: oh no, are you going to pay with the food card?”

“The guy [grocery store employee] looks at you as if you were stealing...as if he is discriminating against you.”

“Discrimination is there... It’s not that they treat you badly, but yes, you can feel it.”

“(...) At the butcher, as we go with the card we have to wait, they leave you waiting... there are 50 people in front and they leave you waiting. As we go with the card we have to wait (...)”

C Principal Component Analysis (PCA)

This appendix outlines the construction of the shame proneness index using the survey module proposed by Zavaleta (2007). To reduce dimensionality and identify latent constructs, I conduct a Principal Component Analysis (PCA), following Filmer and Pritchett (2001) and Osborne et al. (2008).

The PCA model is based on the following equation, where the goal is to identify which items load onto the latent constructs and their respective weights:

$$y_{ij} = \lambda_{i1}F_{1j} + \lambda_{i2}F_{2j} + \ldots + \lambda_{iN}F_{Nj} + \mu_{ij} \qquad j = 1, \ldots, N \qquad i = 1, \ldots, N$$

where y_{ij} represents the items, F_{ij} the latent constructs, λ_{ij} the regression coefficients ("loadings"), and μ_{ij} the error terms assumed to be independent of the constructs.

I begin by including all shame scale's items (Table 1). I first calculate a polychoric correlation matrix, and then preforme the PCA. Table C1 shows that two components had eigenvalues ≥ 1, with Component 1 explaining 51.4% of the variance. Goodness-of-fit indicators—Kaiser-Meyer-Olkin (KMO = 0.87) and Cronbach's Alpha (0.77)-indicate strong internal validity, aligning with usual benchmarks (Yin and Etilé, 2019).

Table C2 presents the loadings for Component 1, identifying items that contribute to the shame proneness construct. Items such as "stupid", "childish", and "blushing" are excluded for having loadings below 0.3, a common threshold in the literature (Yin and Etilé, 2019). Additionally, "stupid" is removed due to weak inter-item correlations (≤ 0.14) and limited relevance to the contexts of poverty and welfare recipiency. This further improves the explained variance of Component 1 (to 62%) and increased the average inter-item correlation.

After excluding low-loading items, the PCA is repeated. The revised analysis shows that Component 1 now explains 62.0% of the total variance (Table C3), with all retained items loading above 0.3 (Table C4). The updated KMO (0.83) and Cronbach's Alpha (0.77) remain strong, further validating the construct.

The final shame proneness index is constructed by aggregating the retained items. Although various methods (e.g., simple summation, PCA, factor analysis with or without rotated weights) could be used, a standardized summation is chosen for simplicity and robustness. Results are consistent across alternative methods. Figure C1 summarizes the distribution within samples of the constructed indices in their sum versions.

Table C1:
Principal Components (Initial Items)

Component	Eigenvalue	Difference	Proportion	Cumulative
Component 1	5.144	4.092	0.514	0.514
Component 2	1.052	0.320	0.105	0.620
Component 3	0.732	0.076	0.073	0.693

Notes: This table shows the results of a principal component analysis performed on the polychoric correlation matrix of the items, restricting the number of components to two. The first four components with eigenvalues higher than the unite value are shown. The first component explains 54.4% of the total variance.

Table C2:
Component Loadings (Initial Items)

Variable	Component 1	Uniqueness
Self-conscious	0.322	0.466
Ridiculous	0.370	0.294
Embarrassed	0.339	0.408
Humiliated	0.345	0.375
Laughable	0.315	0.491
Stupid	0.361	0.329
Childish		0.768
Blushing		0.682
Helpless	0.330	0.439
Disgusting		0.604

Notes: This table shows the components loadings and uniqueness obtained by means of a principal component analysis performed on the polychoric correlation matrix of the items, restricting the number of components to two. The component loadings represent the relationship between each of the items and the latent constructs. Following Yin and Etilé (2019) rule, only those loadings with values greater than 0.3 are shown. As for the uniqueness column, it represents the proportion of variance that is specific to an item and is not shared by the latent constructs.

Table C3:
Principal Components (Final Items)

Component	Eigenvalue	Difference	Proportion	Cumulative
Component 1	3.712	3.088	0.620	0.620
Component 2	0.632	0.073	0.105	0.725

Notes: This table shows the results of a principal component analysis performed on the polychoric correlation matrix of the items, restricting the number of components to two. The first four components with eigenvalues higher than the unite value are shown. The first component explains 62.0% of the total variance.

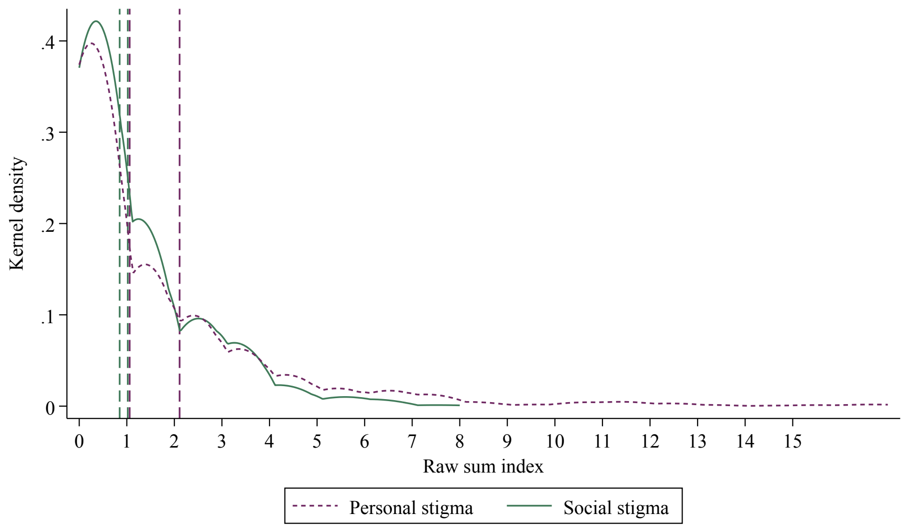
Table C4:
Component Loadings (Final Items)

Variable	Component 1	Uniqueness
Self-conscious	0.393	0.426
Ridiculous	0.447	0.256
Embarrassed	0.393	0.425
Humiliated	0.428	0.425
Laughable	0.379	0.318
Helpless	0.406	0.467

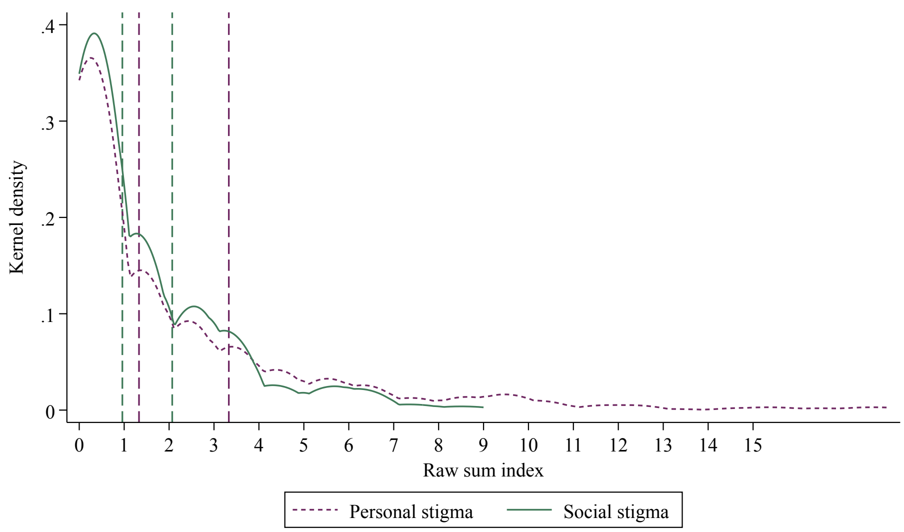
Notes: This table shows the components loadings and uniqueness obtained by means of a principal component analysis performed on the polychoric correlation matrix of the items, restricting the number of components to two. The component loadings represent the relationship between each of the items and the latent constructs. Following Yin and Etilé (2019) rule, only those loadings with values greater than 0.3 are shown. As for the uniqueness column, it represents the proportion of variance that is specific to an item and is not shared by the latent constructs.

Figure C1:
Kernel Densities of Composite Indices

(a) AFAM-PE



(b) TUS



Notes: These figures show epanechnikov kernel densities of shame (short-dashed purple) and mistreatment (solid green) proneness indices. Panel (a) shows results for the AFAM-PE sample, while Panel (b) for the TUS sample. The dashed vertical lines represent the average value for the ineligible group, while the solid vertical lines correspond to the sum of this ineligible mean plus the baseline impact estimate in each case. These lines reflect the estimated push caused by each program across the distribution of the indices. For instance, the effect of AFAM-PE on shame increases the mean for the ineligible group by 99% from 1.06 to 2.11, moving an individual from the 53^{th} to the 70^{th} percentile in the shame scale distribution in the AFAM-PE sample. Meanwhile, the effect of TUS on shame (mistreatment) increases the ineligible mean by 150% (115%) from 1.33 (0.96) to 3.34 (2.07), moving an individual from the 48^{th} (48^{th}) to the 75^{th} (65^{th}) percentile in the shame (mistreatment) scale distribution in the TUS sample. Table 2 reports the full summary statistics for these indices separately for each program, and for each of the summarized and standardized versions.