Welfare Stigma After Take-Up

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Abstract

Welfare participation may impose psychological costs on its recipients by exposing them to stigma associated with receiving public benefits. While existing research has primarily examined welfare stigma as a deterrent to take-up, less is known about whether it persists after enrollment and how large its consequences are for those who do enroll. To study these questions, I exploit eligibility discontinuities in Uruguay's two main social assistance programs: AFAM-PE, a conditional cash transfer delivered discreetly, and TUS, a more visible, government food card, accepted only at authorized retailers. Combining administrative records with survey data collected between five and ten years post enrollment, I find that, across both programs, welfare participation increases personal and social stigma by 0.59 and 0.36 standard deviations, respectively. Both programs increase personal stigma, while only TUS raises social stigma, particularly in localities with lower poverty rates, fewer beneficiaries, and fewer authorized retailers, all patterns consistent with welfare receipt being more conspicuous. Despite increasing household income, participation reduces life satisfaction by 0.64 (on a 1–10 scale) and increases the probability of reporting mental health issues by 5.5 p.p.. A mediation analysis suggests that stigma may account for most of such a decline in life satisfaction, and a back of the envelope calculation indicates substantial income equivalent wellbeing losses, with its lower endpoint corresponding to around one fifth of household income. I further develop a simple framework that helps rationalize these findings by distinguishing take-up deterring welfare stigma from the disutility experienced by those who take benefits up. Overall, this work highlights the psychological costs of welfare stigma and the potential role of program design in propagating them.

Keywords: welfare stigma; social protection; benefit visibility; subjective wellbeing; 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. More than 120 low- and middle-income countries assist poor households with cash transfer programs, while social safety nets cover about 2.5 billion people (Banerjee et al., 2024). Vast evidence documents their effectiveness in reducing poverty and improving wellbeing (Bastagli et al., 2019).[1] However, as safety nets expand, so does debate over whether they foster dependency among the poor (Banerjee et al., 2017). Such negative beliefs are widespread: about two in five people in Latin America and Europe agree that welfare recipients are lazy or that social benefits promote idleness, while welfare recipients are among the least popular groups in the United States (LAPOP, 2012; ANES, 2016; ESS, 2016). These attitudes can generate taxpayer resentment (Besley and Coate, 1992) and contribute to welfare stigma—the psychological cost of receiving welfare *per se* (Moffitt, 1983).[2]

Existing research has primarily examined welfare stigma as an ordeal that discourages take-up (Nichols and Zeckhauser, 1982). Ordeals may improve targeting by screening out less needy (undeserving) households, making their welfare implications ambiguous.[3] For inframarginal recipients, however, stigma imposes psychological costs without affecting the enrollment margin, reducing the net benefits of participation. Despite this, little is known about whether stigma persists after enrollment, the magnitude of its psychological costs, or how they depend on program design, key questions for designing social protection that mitigates these costs while preserving its effectiveness.

This paper addresses this gap by providing causal evidence on the long-run psychological costs of welfare participation among enrolled recipients. I employ regression discontinuity designs exploiting the eligibility rules of Uruguay's two main non-contributory social assistance programs:[4] Asignaciones Familiares–Plan de Equidad (AFAM-PE) and Tarjeta Uruguay Social (TUS), both administered by the Ministry of Social Development (MIDES). AFAM-PE provides standard conditional cash transfers delivered discreetly, while TUS offers inframarginal food assistance through a government-branded card accepted only at authorized retailers (Tenenbaum and Vigorito, 2025).

[1] See Molina-Millan et al. (2016) for a review of the impacts of cash transfer programs in Latin America.

[2] Cash transfers have expanded rapidly in Latin America with increased social expenditure and coverage, while they have often been organized outside traditional contributory social security systems (Atuesta and Cecchini, 2017)—all conditions that may make welfare stigma particularly prevalent.

[3] Recent research emphasizes that the welfare consequences of participation deterrence depend on underlying frictions and on the type of individuals they deter. Anders and Rafkin (2026) find that Supplemental Nutrition Assistance Program (SNAP) eligibility expansions in the U.S. increase take-up among previously eligible households by lifting information constraints (not through reduced stigma), while Naik (2026) shows that administrative barriers disproportionately screen out individuals with poor mental health, even though they value more such assistance.

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

Using pooled discontinuity regressions across programs, I find that welfare participation increases personal stigma (internalized shame) by 0.59 standard deviations (SD) and social stigma (mistreatment by others) by 0.36 SD. Both substantially increase personal stigma, while the increase in social stigma is entirely driven by TUS participation. Unlike AFAM-PE, TUS delivers benefits through a visibly branded card and a separate payment system, making receipt more conspicuous. This contrast, alongside the fact that social stigma effects are more pronounced in localities where TUS recipients are more identifiable, indicates that receipt visibility may shape both the magnitude and nature of stigma—particularly the extent to which it manifests through self-image and social image concerns. Overall, these findings document long-run psychological costs after take-up.[5]

On one hand, AFAM-PE is a conditional cash transfer targeted at vulnerable households with children under 18.[6] Benefits are disbursed discreetly through regular bank accounts or in cash on payment centers. On the other hand, TUS is a "near-cash" transfer targeted at the country's most vulnerable households, providing a food card issued by the government hat can be used for essential goods (e.g., food, hygiene items) at authorized retailers. Unlike AFAM-PE, the TUS card carries an official logo that makes benefit receipt more visible in stores (see Appendix Figure A1). In 2017, AFAM-PE and TUS provided monthly benefits of US$49 and US$33 for the first child and covered 37% and 15% of households with children in the country, respectively.[7]

The identification strategy relies on the selection criteria by MIDES for each program, based on two separate eligibility thresholds over the *Índice de Carencias Críticas* (ICC), a proxy means-test that measures household vulnerability.[8] Crossing each threshold discontinuously increases the probability of participation by 56 p.p. for AFAM-PE and 65 p.p. for TUS, respectively.[9] Since the ICC combines multiple household characteristics using a formula undisclosed to applicants, manipulation is unlikely, and density tests provide no evidence of such manipulation at either cutoff. Consequently, I use fuzzy regression discontinuity designs, comparing households just above

[5] MIDES subsequently redesigned the TUS card to partially reduce its visible branding.

[6] Households receiving the transfers must comply with standard conditionalities: having children regularly attend to school and health checkups.

[7] Since the TUS cutoff lies well within the AFAM-PE eligible range, households with children eligible for TUS are also eligible for AFAM-PE (and thus the TUS estimates capture the effects of TUS participation among AFAM-PE eligible households).

[8] The AFAM-PE score is based on applications submitted between 2008 and 2010. The TUS score was calculated using an updated formula during the program's 2011 nationwide re-certification.

[9] AFAM-PE additionally requires per capita formal income below a specified threshold, calculated from administrative records including earnings, pensions, and other transfers (Bérgolo and Cruces, 2021).

and below each threshold to estimate local average treatment effects of welfare participation.

The empirical analysis links administrative records to a follow-up survey specifically designed to evaluate AFAM-PE and TUS program impacts (Amarante and Vigorito, 2011). The administrative records provide baseline socioeconomic and demographic characteristics, eligibility scores, and 2008–2018 participation histories. These records are matched to two survey waves conducted in 2011–2012 and 2016–2018 among eligible and ineligible applicants near each program's eligibility thresholds. The survey collects stigma measures and broader socioeconomic, health, and wellbeing outcomes. Stigma outcomes are available only in the second wave, so I estimate stigma effects measured approximately five to ten years after households began receiving benefits.[10]

I measure the psychological consequences of welfare stigma using a questionnaire from 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). These questions aim to capture general experiences of shame and humiliation that are applicable to both welfare recipients and non-recipients. I use these items to construct standardized indices of personal stigma, capturing feelings of shame and self-devaluation, and social stigma, capturing perceived mistreatment by others.[11] Using principal component analysis, I keep eight of the ten personal stigma items. I then construct personal, social, and overall stigma indices by adding up and standardizing corresponding items, with the overall index combining both dimensions.

I further develop a simple framework to organize the interpretation of the results. The framework distinguishes anticipated self- and social-image concerns that deter take-up from experienced costs that affect wellbeing post enrollment, allowing recipient adults to reconsider participation.

The first set of results shows that participation in both programs substantially increases personal stigma. In the preferred specification, AFAM-PE increases personal stigma by 0.52 SD, while TUS does so by 0.72 SD. The item-level estimates reveal different patterns across programs. For AFAM-

[10]Since the second wave found around 45% of the original sample, I examine differential attrition at the eligibility cutoffs and assess the sensitivity of the main estimates to controlling for baseline predictors of attrition, applying inverse probability weighting, and conducting bounding exercises to assess the potential influence of selective attrition.

[11]The personal stigma questions asks respondents to rate how frequently they experience ten feelings, including self-consciousness, embarrassment, humiliation, and helplessness. This scale captures internal emotional experiences. The social stigma scale measures perceived mistreatment by others, asking how frequently, during the previous three months, respondents felt disrespected, treated unfairly, or discriminated against. Both sets use four response categories ranging from 0 (rarely or never) to 3 (always or almost always). Since the questions do not explicitly recall experiences of welfare receipt, their interpretation related to welfare stigma relies on the estimated causal effects of program participation. Appendix D describes the construction of the indices, assesses the dimensionality of the personal stigma items using principal component analysis, and examines sensitivity to alternative scoring methods.

PE, the strongest evidence is concentrated in the feeling of self-consciousness, an emotion closely related to shame. For TUS, the largest estimates correspond to feeling laughable, helpless, repulsive to others, and humiliated. While both programs increase personal stigma, the TUS estimates suggest a broader range of experiences associated with shame, humiliation, and self-devaluation.[12]

The pattern for social stigma notably differs across programs. In the preferred specification, TUS increases social stigma by 0.79 SD, while the AFAM-PE estimate is close to zero and statistically indistinguishable from zero. This points to the visibility of delivery as a plausible mechanism. Since TUS is delivered through a prominently marked food card and a separate payment system, it makes beneficiary status observable during purchases and potentially exposes recipients to judgment or mistreatment by others.[13] In line with this interpretation, these social stigma effects are concentrated in localities with lower poverty rates, fewer beneficiaries, and fewer authorized retailers, where receipt is likely to be more salient. Although I cannot fully disentangle visibility from other differences between programs, the combined evidence is consistent with the visibility of receipt playing an important role in social stigma.

I next analyze whether participation generates labor market responses that a planner might value. Such responses, alongside potential targeting gains emphasized by the ordeal argument, could be weighed against the psychological costs imposed on recipients. Pooling observations across both programs and survey waves, I find no statistically significant effects on employment, informality, or hours worked, although welfare participation increases the probability of searching for a job by 8.3 pp.[14] Overall, these results suggest that welfare participation encourages job search, with limited evidence of changes in other behavioral responses.

Furthermore, I examine whether the estimated psychological costs extend to broader dimensions of wellbeing.[15] Since life satisfaction and mental health outcomes are available in both survey waves, I pool observations across waves and programs to improve statistical precision. I find that, despite

[12]Several institutional features may help explain this broader pattern for TUS. Compared to AFAM-PE, TUS operates outside the traditional social security system, targets a narrower poorer population, and restricts purchases to essential goods. These features may reinforce recipients' awareness of their economic vulnerability and neediness for public assistance, potentially intensifying feelings of stigma.

[13]Qualitative evidence shows mistreatment of TUS recipients by customers, store employees (Moreno et al., 2014).

[14]The main specification includes outcomes for all household members in working age. When restricting the sample to survey respondents, the results remain qualitatively similar, with a slightly less precise job search estimate of 7.7pp.

[15]Shame, humiliation, and discrimination are important predictors of life satisfaction (Hojman and Miranda, 2018), and previous studies have interpreted reductions in subjective wellbeing among welfare recipients as indirect evidence of stigma (Gao and Zhai, 2017; Qi and Wu, 2018).

providing additional income, welfare participation reduces life satisfaction by 0.64 on a scale from 1 (very dissatisfied) to 10 (very satisfied) and increases the probability of reporting mental health difficulties by 5.5 pp. To explore the role of stigma in the life satisfaction effects, I conduct an mediation decomposition combining the estimated causal effects of welfare participation on overall stigma with its conditional association with life satisfaction in the second wave. The component associated with measured stigma accounts for approximately 95% of the estimated decline in life satisfaction. An illustrative back of the envelope calculation combines this component with the conditional association between life satisfaction and log household income in ELBU, placing its income equivalent at 58.2% of household income, with a 95% bootstrap confidence interval of [19.1%, 131.5%]. Although imprecisely estimated, even the lower endpoint corresponds to approximately one-fifth of household income, indicating substantial wellbeing costs.

This paper contributes to multiple strands of the literature. First, to the best of my knowledge, it provides the first causal evidence that welfare participation itself generates psychological costs associated with stigma among enrolled recipients. Much of the existing research has examined stigma as a barrier to take-up,[16] while evidence on recipients' experiences has been primarily qualitative or observational.[17] Closest to this paper, Celhay et al. (2025) infer stigma from the under-reporting of welfare receipt and show that reporting increases when receipt is more common and so less observable within local networks. Pukelis and Holcomb (2025) directly measure self-stigma and anticipated social judgment among SNAP participants and experimentally examine messages intended to reduce stigma. Lasky-Fink et al. (2026) develop a multidimensional measure of welfare stigma and examine its relationship with take-up and program design. More recently, McQuillan and Moore (2026) find that messages aimed at reducing free rider stigma increase unemployment insurance applications, particularly among high wage workers. And Allcott et al. (2026) show that restrictions on SNAP purchases increase recipients' perceptions of stigma and quantify the associated welfare costs. By exploiting eligibility discontinuities, I identify the causal effect of welfare participation itself on recipients' self-reported shame and experiences of mistreatment, providing evidence on the longer-run psychological costs of receiving public benefits.

[16] See, among others, Stuber and Kronebusch (2004); Stuber and Schlesinger (2006); Bhargava and Manoli (2015); Osman and Speer (2023); Anders and Rafkin (2026).

[17] Non-causal and indirect evidence from Chad (Della Guardia et al., 2022), China (Qi and Wu, 2018), and the U.S. (Lapham and Martinson, 2022).

Second, I contribute to the literature on self- and social-image concerns by measuring two distinct manifestations of stigma: internalized shame and perceived mistreatment by others (e.g., Akerlof and Kranton, 2000; Bénabou and Tirole, 2011; Ghosal et al., 2022; Bursztyn and Jensen, 2017; Butera et al., 2022). Friedrichsen et al. (2018) show experimentally that concerns about appearing as a low-skilled type individual or willing to live off others reduce transfer take-up. Concomitantly, Fafchamps and Sadou Zouleya (2026) document that making transfer applications public reduces take-up among low-income participants. I extend this literature by examining how different dimensions of stigma manifest after enrollment across programs with different characteristics, including delivery systems. The results show that personal stigma increases even under private delivery, whereas social stigma increases only under TUS, especially in localities where receipt is more conspicuous. These findings suggest that feelings of personal and social stigma can coexist together and respond differently to program design, with visibility potentially playing an key role.

Third, I contribute to the growing literature on cash transfers and subjective wellbeing (e.g., Haushofer and Shapiro, 2016; Haushofer et al., 2020; Crosta et al., 2026; Vivalt et al., 2026) by examining welfare stigma as a potential mechanism through which transfers affect recipients' welfare. While current research has mainly examined the overall psychological effects of cash transfers, I use direct measures of internal shame and external mistreatment to study if stigma contributes to changes in subjective wellbeing. The findings suggest that the psychological stigma costs from welfare participation may partly offset the material benefits of the cash disbursed, highlighting the importance of taking these costs into account in welfare evaluations of social protection.

Fourth, I add to the literature evaluating Uruguay's social protection system by documenting a previously unexplored psychological cost of participation. Previous studies have found positive effects on birth outcomes and political support (Amarante et al., 2016; Manacorda et al., 2011), reductions in formal employment among adult beneficiaries, particularly women (Bérgolo and Galván, 2018; Bérgolo and Cruces, 2021), and mixed effects on food consumption and expenditures (Tenenbaum and Vigorito, 2025). More recently, Giaccobasso (2026) documents long-run benefits of childhood exposure to conditional cash transfers, including higher adult earnings and delayed fertility among women. My findings highlight a potentially important distinction in the incidence of these effects. While children may benefit from additional income support over their childhood, adults who manage the transfers may bear the psychological costs associated with stigma.

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, expanding eligibility to informal and vulnerable households previously excluded from the formal system. In response to the country's severe economic crisis of the early 2000s, the government that took office in 2005 introduced the *Plan de Atención Nacional a la Emergencia Social* (PANES), a temporary assistance program targeting the poorest households. One of its components, *Tarjeta Alimentaria*, provided food assistance through a dedicated food card complement beginning in 2006. Following the conclusion of PANES in 2007, these interventions laid the foundation for the permanent programs studied here, which I describe next.

Asignaciones Familiares–Plan de Equidad. AFAM-PE, which officially started in 2008, is Uruguay's largest conditional cash transfer program, targeting low-income households with children under 18 or pregnant women. Its dual objectives are to provide monetary assistance to vulnerable populations and promote school attendance and medical checkups among children. As of 2017, the program covered approximately 200,000 households, with a monthly transfer of US$49 for the first beneficiary, equivalent to 11% of the minimum wage. Eligibility is determined through a two-stage means test: households must have formal income below a specified threshold, based on social security records, and pass a proxy means test using the *Índice de Carencias Críticas* (ICC).[18] The ICC is an algorithm designed to predict household poverty using indicators such as educational attainment, housing quality, and ownership of durable goods. The AFAM-PE eligibility scores used in the analysis were calculated from applications submitted between 2008 and 2010. Benefits are conditional on compliance with children's school attendance and health checkup requirements, although systematic enforcement began only in 2013. Benefits can be spent without restrictions and are disbursed through regular bank accounts or collected in cash at regular payment centers, making welfare participation less conspicuous during purchases.

Tarjeta Uruguay Social. TUS evolved from the *Tarjeta Alimentaria*, introduced in 2006. After adopting the ICC in 2009, the program was renamed TUS in 2011, when the ICC weights were

[18] See Amarante and Vigorito (2011) for additional details on the construction of the ICC.

recalculated and beneficiary households were re-certified. TUS is a near-cash transfer intended to help households in extreme poverty meet basic nutritional needs. It targets a narrower population than AFAM-PE, with around 77,000 active cards by the end of 2017, and provides US$33 per month for households with one child, equivalent to 8% of the minimum wage.[19] Eligibility is determined using the updated ICC. The analysis exploits the cutoff determining entry into TUS, while households above a second, more stringent cutoff receive twice the transfer amount. Unlike AFAM-PE, TUS is delivered through a dedicated food card only accepted at authorized retailers (*Comercios Solidarios*) and purchases are restricted to food, and cleaning and hygiene items.[20] The card displays a government logo and explicitly identifies it as part of a social plan, making recipient status visible to store employees (e.g., cashiers) and other customers. Figure A1 shows examples of the cards from both programs during the period of analysis.

The coverage of both programs overlaps substantially: 77% of TUS households also receive AFAM-PE, which rises to 88% among households with children—the population of this study.[21] In the empirical sample, the TUS eligibility cutoff lies well within the AFAM-PE eligible range, so households on both sides of the TUS threshold are eligible for (and indeed receive) AFAM-PE. So the TUS analysis identifies the incremental effect of TUS participation among households eligible for AFAM-PE.

Attitudes and beliefs. Despite Uruguay's relatively low poverty and inequality levels among Latin American countries,[22] negative public attitudes toward poverty and welfare recipients are widespread, as they are elsewhere in the region. The share of Uruguayans attributing poverty to personal failings, specifically the belief that people are poor because they are "lazy or lack willpower," has increased considerably from 12% in 1996 to 45% in 2011 (Figure A2, Panel (a)). In a longitudinal panel survey, support for government assistance is substantially higher when provided through employment (94%) or food (83%) than through direct cash transfers (about 50% of respondents) (ELBU, 2016-2017).[23] Uruguay also ranks second among Latin American and

[19] Purchases made with TUS also receive a full rebate of the Value Added Tax (VAT).

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

[21] TUS originally targeted households with children under 18 but later expanded to other vulnerable populations, including adults in extreme poverty, transgender individuals, homeless, and people with chronic diseases.

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

[23] Relatedly, only 42% believe the government should assist all poor people, compared with 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).

Caribbean countries in agreement with the statement that "welfare recipients are lazy," with a mean score of 57.5 compared with a median country score of 46.9 (on a 0–100 scale; Figure A2, Panel (b)). Existing qualitative evidence (see Appendix B for specific quotes) further underscores these stigmatizing narratives, highlighting the enduring notion of the "undeserving poor." These prevalent negative beliefs about welfare recipients despite the robust development of its social safety net make Uruguay a compelling case for analyzing welfare stigma.

Potential mechanisms. Welfare participation may affect beneficiaries through competing channels. On one hand, additional income can improve material wellbeing and alleviate feelings of deprivation. On the other hand, receiving means-tested assistance may generate psychological costs by making recipients' economic vulnerability and reliance on public support more salient. This may conflict with norms of financial independence and generate feelings of shame even when participation is not observed by others. If welfare receipt is publicly observable, then recipients may be exposed to external judgment and mistreatment opportunities. The relative importance of these channels should depend on program design.

Both AFAM-PE and TUS have features that may generate personal stigma: they are non-contributory, means-tested programs targeting economically vulnerable households. TUS targets a narrower, poorer population, operates outside the traditional social security system, and restricts purchases to basic necessities. These features may reinforce recipients' awareness of their economic vulnerability and dependence on public assistance. Accordingly, both programs may increase personal stigma, potentially to a greater extent under TUS.

The programs differ more pronouncedly in their potential to generate social stigma. AFAM-PE provides unrestricted benefits through regular bank accounts or cash, making participation relatively discreet during purchases. In contrast, TUS requires recipients to use the marked food card at authorized retailers, making their beneficiary status visible to store employees (e.g., cashiers) and potentially to other customers. This visibility may expose recipients to greater scrutiny and mistreatment, while also reinforcing feelings of shame. Consequently, one should expect TUS to generate greater social stigma, whereas such effects should be limited under AFAM-PE. These channels motivate the conceptual framework developed in the following section, which distinguishes the role of stigma in deterring take-up from its psychological costs for enrolled recipients.

2 Conceptual framework

I develop a simple model to distinguish the role of stigma in deterring take-up from its consequences after enrollment. Following Moffitt (1983) and Besley and Coate (1992), receiving assistance carries psychological costs alongside material benefits. Self-image costs resemble the shame and self-devaluation associated with personal stigma, while social image costs relate to perceived judgment and mistreatment by others associated with social stigma.

Setup. There is a continuum of mass one of parents indexed by $i \in [0,1]$ who can differ in available resources, preferences, and participation costs. I suppress the individual subscript below for simplicity. At $t = 1$, an eligible parent decides whether to enroll in a welfare program. At $t = 2$, after experiencing receipt and learning its image costs, the parent decides whether to continue. Let $d_t \in \{0,1\}$ indicate receipt in period t, with $d_2 \leq d_1$.

The parent chooses own consumption c_t, expenditures benefiting children x_t, working hours h_t, and leisure ℓ_t, subject to

$$c_t + x_t \leq y_t + wh_t + d_t b_t$$

$$h_t + \ell_t + d_t \tau_t \leq T, \qquad c_t, x_t, h_t, \ell_t \geq 0$$

where y_t is non-labor income, w denotes the wage, T is available time, b_t is the transfer value net of monetary participation costs, and τ_t is the time required to apply or comply with program rules. Administrative hassle also carries a utility cost a_t, distinct from stigma, as is standard in the literature.

Let p denote self-image and νs social image (non-negative) costs, where ν measures how "visible" receipt is to others. Utility in each period is

$$U_t = u(c_t, \ell_t) + \alpha v(x_t) - d_t(a_t + p + \nu s)$$

u and v are increasing and concave, and $\alpha > 0$ measures parents' concern for their children (Becker, 1974). Image costs are incurred during receipt in both periods. At enrollment, the parent has beliefs about p and s that may differ from subsequent, realized experience.

Choices. The parent maximizes expected discounted utility

$$\max_{d_t, c_t, x_t, h_t, \ell_t} \mathbb{E}[U_1 + \beta U_2]$$

subject to the stated constraints and $d_2 \leq d_1$, where $0 < \beta \leq 1$. The expectation reflects the parent's beliefs at enrollment. Parents decide whether to enroll based on the image costs they expect to experience. After receiving assistance, they learn the magnitude of these costs and decide whether to continue. For a given decision, interior consumption and labor solutions satisfy

$$\alpha v'(x_t) = u_c(c_t, \ell_t), \qquad u_\ell(c_t, \ell_t) = w u_c(c_t, \ell_t)$$

The first condition equates the marginal utility of spending on children with that of own consumption. The second equates the marginal utility of leisure with the utility gained from an additional hour of work.

Now, let $V_2(d)$ be the optimized second period utility before image costs, given d. An beneficiary continues if

$$p + \nu s \leq V_2(1) - V_2(0)$$

At $t = 1$, an eligible, informed parent enrolls if expected utility, accounting for image costs and the option to leave, exceeds the value of never enrolling. Appendix C derives both decisions.[24]

Implications. First, this simple framework shows that positive image costs are compatible with voluntary continued receipt. The parent remains enrolled whenever the gains from receiving assistance cover these costs. Higher experienced costs weakly reduce continuation, while a shift in beliefs toward higher costs weakly reduces initial take-up. Conditional on receipt, image costs can lower parental wellbeing without directly changing the first order conditions. Second, greater visibility raises social image costs for parents with $s > 0$, while self-image costs can persist even under discreet delivery.

Third, a loss in parents' own wellbeing does not necessarily imply a loss in their total valuation of participation. To see this, let A be each parent's own discounted gain and C the gain for children, relative to never enrolling. The full benefit is $G = A + \alpha C$, so $A < 0$ can coexist with $G \geq 0$.[25]

[24] Monetary costs, time requirements, and administrative hassle can deter take-up independently of stigma.

[25] Appendix C develops this comparison and extends the model to expenditures for children that are costly to

3 Data

The empirical analysis combines two main matched data sources: MIDES administrative records containing baseline household characteristics and program participation histories through 2018, and a follow-up survey conducted in two waves (2011–2012 and 2016–2018) to evaluate program impacts (Amarante and Vigorito, 2011).[26]

3.1 MIDES administrative records

These contain baseline socioeconomic and demographic information used to calculate each household's eligibility score, the *Índice de Carencias Críticas* (ICC), as well as longitudinal information on program participation. The baseline application records cover 2008–2010 for AFAM-PE and 2011 for TUS, following the latter program's re-certification.[27] The data include the ICC score used to determine households' eligibility, which serves as the running variable in the regression discontinuity design. I normalize the score relative to the relevant eligibility cutoff for each program and region, so that zero denotes each threshold: positive values indicate eligibility, and negative values indicate non-eligibility.[28] 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.

3.2 ESAFAM follow-up survey

The *Encuesta de Seguimiento de AFAM* (ESAFAM) survey adds self-reported data collected through in-person interviews at respondents' homes. Specifically designed to evaluate AFAM-PE's impacts, it oversamples households near the eligibility threshold to balance comparability with sample size.[29] Additional households were sampled to represent the remaining AFAM-PE

reverse.

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

[27]When AFAM-PE was launched, households already enrolled in PANES were automatically enrolled. For these households, the ICC was calculated using the original socioeconomic and demographic information collected under PANES.

[28]Developed by researchers at *Universidad de la República*, the ICC uses a probit model to predict the probability that an applicant household belongs to the lowest income quintile, based on variables from the national household survey (Amarante and Vigorito, 2011). Eligibility cutoffs differ between Montevideo and the rest of the country to reflect differences in cost of living expenses. Normalizing the score relative to the relevant cutoffs allows households from both regions to be pooled together.

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

eligible population, which includes respondents around the TUS eligibility threshold. The analysis for TUS is thus restricted to households with children that qualified for AFAM-PE at some point during the study period. Accordingly, estimates for TUS capture effects within this population.

The first wave (2011–2012) surveyed 3,832 households, of which 1,734 were re-interviewed in the second wave (2016–2018), representing 45% of the original sample. Appendix Table A1 examines differential attrition at each program's eligibility cutoff. For AFAM-PE, estimated discontinuities range from 0.9 to 9.6 percentage points and are statistically significant in only one out of four specifications. For TUS, the estimates are statistically significant across specifications but change sign: linear specifications indicate a decline in attrition of approximately 12 p.p. at the cutoff, whereas quadratic specifications indicate an increase of 16–17 p.p.. Attrition is also associated with some baseline characteristics, including geographic location and household size. These patterns motivate sensitivity analyses using controls for observed predictors of attrition, inverse probability weights for survey retention, and Lee bounds.

After excluding observations with missing values on the variables used in the main analysis, the final samples comprise 917 households for the AFAM-PE evaluation and 595 households for the TUS evaluation, of which 67% and 51%, respectively, are eligible for the program in question.

Respondents in both samples are predominantly women: 89% in the AFAM-PE sample and 93% in the TUS sample. The samples also have similar geographic distributions, with around 28% of households residing in Montevideo. Consistent with TUS's targeting of extreme poverty, respondents in the that sample have fewer years of education on average than those around AFAM-PE's cutoff (7.6 vs. 9.4 years) and live in larger households (4.8 vs. 3.6 members). The share of respondents who identify as white is 70% in the TUS sample and 82% in the AFAM-PE sample. Respondents' average ages are 40.4 and 44.5 years, respectively. Among program participants, cumulative time receiving benefits averages 5.3 years for AFAM-PE and 8.2 years for TUS.

In addition to employment, income, and education, the survey collects information on wellbeing, attitudes, and beliefs. The second wave includes questions on shame, humiliation, and stigma, which provide the outcomes used in this study.[30] These measures allow me to examine the medium- to

[30]The survey also includes a household consumption module collecting self-reported expenditures on food and non-food items. It distinguishes detailed food categories and expenditures on housing, clothing, transportation, health, education, and personal care. Food expenditures are recorded over 7-day and 30-day reference periods, while non-food expenditures use specific recall periods. Based on this information, I construct consumption measures used later in the analysis.

long-term effects of program participation on psychological outcomes related to welfare stigma, with these measured several years after households' eligibility assessments. Figure A3 illustrates the study timeline.

3.2.1 OPHI survey module

Developed by Zavaleta (2007), this module measures the ability to live without shame, a "missing dimension" of poverty and wellbeing (Alkire, 2007). It distinguishes between shame, an emotion involving self-devaluation, and mistreatment or external humiliation, arising from interactions with others (Mills and Zavaleta, 2015). The shame proneness scale draws on the Personal Feelings Questionnaire-2 and captures feelings such as embarrassment, helplessness, and self-consciousness. The mistreatment scale draws on existing surveys, e.g., the European Social Survey (ESS), and captures experiences of disrespect, unfair treatment, and discrimination. I use these two dimensions to construct measures of personal and social stigma, respectively.

For the shame proneness scale, respondents rate how frequently they experience each listed feeling, using response categories from 0 (rarely or never) to 3 (always or almost always). The question 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 uses the same response categories but specifies a three-month reference period: "*Have you felt that you have been treated [in the following ways] during the last three months?*" The listed experiences include being treated without respect, unfairly, or with discrimination. Table 1 reports the individual items and their descriptive statistics.

To reduce the dimensionality of the shame proneness scale and guide item selection, I combine principal component analysis (PCA) of the polychoric correlation matrix with a common factor model (Filmer and Pritchett, 2001; Yin and Etilé, 2019). The polychoric correlations account for the ordered categorical nature of responses. Parallel analysis supports retaining one component. The preferred personal stigma index includes eight items: self-conscious, ridiculous, embarrassed, humiliated, laughable, stupid, helpless, and disgusting. Childish and bushed are excluded due to their low communalities (0.168 and 0.191). The retained items capture a dominant dimension: the first component explains 56.1% of the variance, with an alpha of 0.89.[31] The social stigma index

[31] The shame scale has also been examined in the Uruguayan context using the *Estudio Longitudinal del Bienestar del Uruguay*, which includes the same survey items. The resulting index was negatively associated with income, education, and employment, and positively correlated with poverty and social assistance receipt (Nicolau and Vigorito, 2023).

retains the three mistreatment items specified in the original module. I thus construct the personal and social stigma indices by summing the eight retained shame items and the three mistreatment items, respectively. The overall stigma index sums all eleven, combining both dimensions. I standardize each of these sums to have mean zero and standard deviation one in the pooled AFAM-PE and TUS estimation sample. Appendix D provides further details on this analysis.

3.2.2 Summary statistics

Descriptives in Table 1 show left-skewed distributions, with around 80% of responses at zero for most individual items. This is suggestive that these questions may capture intense instead of mild stigma experiences, which may be difficult to disclose to interviewers (potentially subject to social desirability bias). Despite this "floor" interpretation, these measures show expected patterns: eligible individuals and those in the around the TUS eligibility cutoff (who face greater deprivation levels) report higher frequencies in all individual items (Walker, 2014; Hojman and Miranda, 2018).

3.3 Other data sources

3.3.1 Estudio Longitudinal del Bienestar en Uruguay

To contextualize the magnitude of the estimated effects related to welfare stigma, I use an additional dataset from the *Estudio Longitudinal del Bienestar en Uruguay* (ELBU), which follows a representative sample of children (and their households) who entered public primary school in 2004 (Instituto de Economía , IECON). The sample consists of adult respondents observed in both the third (2011–2012) and fourth (2016–2017) waves.[32] These data include the same OPHI questionnaire, together with information on socioeconomic characteristics (e.g., self-reported household income including several different sources, education, mental health, and other key variables such as life satisfaction. I construct corresponding personal, social, and overall stigma indices, as well as life satisfaction and household income, that will be later included in the analyses.

[32]The fourth wave also collects these measures for the children originally enrolled in the study, who were then transitioning from adolescence to adulthood.

3.3.2 Additional information

To examine whether welfare stigma effects vary across localities, I match the ESAFAM data to additional information on local poverty measures, TUS beneficiary prevalence, and authorized retailers by locality. The 2011 Population Census provides locality population counts and the share of households with at least one unmet basic need, *Necesidades Básicas Insatisfechas* (NBI), which I use as a measure of poverty. I then combine the population counts with MIDES records of TUS recipients in 2017 to measure beneficiary prevalence, and with public web-scrapped listings of *Comercios Solidarios* from MIDES to measure retailer density.[33]

4 Identification strategy

4.1 Identification

Using the eligibility rules described in Section 1, I estimate the causal effects of welfare participation through a regression discontinuity design (RDD). The analysis compares households just above and below the ICC eligibility threshold for each program. Eligibility thresholds are defined separately for Montevideo and the rest of the country to account for regional differences in costs of living. Thus, I normalize the ICC score relative to the relevant program and regional cutoff. The resulting running variable, X_i, measures distance from the threshold, with eligibility given by $Z_i = \mathbb{1}\{X_i \geq 0\}$.[34]

Let $Y_i(d)$ denote household i's potential outcome under participation status $d \in \{0, 1\}$. The identifying assumption relies on the continuity of expected potential outcomes at each cutoff:

$$\lim_{x \to 0^-} \mathbb{E}[Y_i(d) \mid X_i = x] = \lim_{x \to 0^+} \mathbb{E}[Y_i(d) \mid X_i = x], \qquad d \in \{0, 1\}$$

This assumption rules out a discontinuity in potential outcomes at the threshold in the absence of a change in participation, e.g., given arbitrarily defined eligibility thresholds, program assignment is quasi-random within a sufficiently small segment around them. Households immediately below the cutoffs thus serve as valid counterfactuals for those just above the cutoffs.

[33] Retailer counts are averaged across partial historical listings from 2013–2015 and an archived listing from 2022.

[34] The original ICC ranges from 0 to 1, with eligibility cutoffs of approximately 0.2 for AFAM-PE and 0.6 for TUS. For simplicity, I suppress program and regional subscripts in the notation.

Given that eligibility does not perfectly determine participation, I implement a fuzzy RDD. Now, let $D_i(z)$ be the potential participation under eligibility status z. Identification additionally requires a non-zero discontinuity in participation at the cutoffs, an exclusion restriction under which eligibility affects outcomes only through program participation, and monotonicity, $D_i(1) \geq D_i(0)$, for households near the thresholds. Under these assumptions and local continuity of the joint distribution of potential outcomes and potential participation, the ratio of the outcome discontinuity to the participation discontinuity identifies the local average treatment effect (LATE):

$$\beta_{\text{IV}} = \frac{\lim_{x \to 0^+} \mathbb{E}[Y_i \mid X_i = x] - \lim_{x \to 0^-} \mathbb{E}[Y_i \mid X_i = x]}{\lim_{x \to 0^+} \mathbb{E}[D_i \mid X_i = x] - \lim_{x \to 0^-} \mathbb{E}[D_i \mid X_i = x]}$$

$$= \mathbb{E}[Y_i(1) - Y_i(0) \mid D_i(1) > D_i(0),\, X_i = 0]$$

This estimand captures the effect of participation for households at the cutoff whose participation is induced by eligibility. I estimate it using two-stage least squares (2SLS), instrumenting the dummy for program participation with the eligibility indicator.

Figure 1 (Panels (a) and (c)) shows the first-stage discontinuities in program participation at the baseline eligibility cutoffs.[35] Eligibility is determined by each household's baseline ICC score, while participation is defined as receipt of benefits at any point during the study period up to 2018. The y-axis shows the household participation rate in each program, with dots representing averages within five bins on each side of the cutoff and overlaid fitted lines.[36] For AFAM-PE, some initially ineligible households later re-applied and gained eligibility, increasing participation rates to the left of the baseline cutoff, while all baseline-eligible households in the sample received benefits at least once during the observation period.

For TUS, the lower participation rates among eligible households likely reflect program capacity constraints and administrative barriers that prevented some eligible households from receiving benefits instead of self selection concerns (Tenenbaum and Vigorito, 2025).[37] If there was also demand-side non-compliance, with eligible households deterred by anticipated stigma (e.g., welfare stigma deterring take-up), the estimates could be conservative. In particular, if households an-

[35] Appendix Figure A4 presents analogous plots using ten bins on each side.

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

[37] MIDES staff cited operational issues, such as delays in delivering cards or cases where the card was issued to someone other than the applicant who subsequently left the household.

ticipating greater stigma are less likely to participate, and these expectations reflect larger causal effects on measured stigma, compliers would experience smaller effects than the broader eligible population would experience under participation. The estimates should thus be interpreted as local average treatment effects for compliers that are potentially conservative.

Bandwidth selection is informed by the original follow-up survey design described in Amarante and Vigorito (2011). For AFAM-PE, the survey intentionally oversamples households near the eligibility cutoff to improve the precision of local comparisons while maintaining sufficient statistical power. This design effectively defines the estimation bandwidth, spanning approximately $[-0.04, 0.07]$ in the normalized ICC score. For TUS, the sample includes eligible households above the cutoff and ineligible but AFAM-PE eligible households below it, covering a broader normalized ICC range of approximately $[-0.40, 0.40]$. I retain these fixed bandwidths for the main analysis given the scarce sample size in this region. I assess robustness to the choice of bandwidth by progressively narrowing the estimation windows. I also examine sensitivity to alternative functional forms and local polynomial specifications.

4.2 Estimation

I begin by estimating the reduced form effect of program eligibility on each outcome using OLS. This regression measures the discontinuity in outcomes at the eligibility cutoff and provides a benchmark for the participation effects estimated below. I estimate:

$$Y_i = \alpha_1 + \beta_{\text{OLS}} Z_i + \gamma_1' f(X_i) + \theta_1' \big[Z_i f(X_i) \big] + \delta_1' W_i + \varepsilon_{1i} \tag{1}$$

where Y_i denotes the outcome variable, Z_i is an indicator equal to one for households above the eligibility cutoff ($X_i \geq 0$), and $f(X_i)$ contains the nonconstant polynomial terms of the normalized ICC score: $f(X_i) = X_i$ in the linear specification and $f(X_i) = (X_i, X_i^2)'$ in the quadratic specification. Interacting these terms with eligibility allows the relationship between the score and the outcome to differ at either side of the cutoff. The vector W_i includes control variables and ε_{1i} is the error term. β_{OLS} captures the effect of eligibility at the cutoff.

Given that there is non-perfect compliance at the eligibility cutoffs, the main analysis relies on a fuzzy RDD estimated by two-stage least squares (2SLS). I instrument actual program participation

and its interactions with the normalized ICC score using baseline eligibility and its corresponding score interactions. The first-stage equation for participation is thus:

$$D_i = \alpha_0 + \pi_0 Z_i + \gamma_0' f(X_i) + \theta_0' \big[Z_i f(X_i) \big] + \delta_0' W_i + \varepsilon_{0i} \tag{2}$$

where D_i indicates program participation at any point during the observation period, π_0 captures the first-stage eligibility effect at the cutoff, and the remaining variables follow the definitions in Equation 1.[38] The second stage then estimates:

$$Y_i = \alpha_2 + \beta_{\text{IV}} \widehat{D}_i + \gamma_2' f(X_i) + \theta_2' \widehat{D_i f(X_i)} + \delta_2' W_i + \varepsilon_{2i} \tag{3}$$

with $\widehat{D}_i$ and $\widehat{D_i f(X_i)}$ being the fitted values from the respective first-stages. Here, the parameter of interest β_{IV} captures the causal participation effect at that threshold. Under the identification assumptions from above, it estimates the LATE for households whose participation is induced by eligibility. These estimates are specific to complier households near each program's eligibility cutoff. This limits their generalizability to the entire beneficiary population. Furthermore, the TUS estimates capture the effect of TUS participation among AFAM-PE eligible households, most of whom have also received AFAM-PE. Since the program-specific estimates concern different compliers at different thresholds, they cannot be added to recover the combined effect of both programs. These limitations concern external validity but not internal validity.

The preferred specification uses a linear polynomial in the normalized ICC score and includes controls for sex (woman = 1), age, region (Montevideo = 1), and ethnicity (white = 1). For robustness, I estimate linear and quadratic specifications, with and without controls. Standard errors are clustered by the normalized ICC score.[39] I also assess sensitivity to progressively narrower bandwidths, donut specifications that exclude observations closest to the cutoff, and compute local polynomial estimates following Cattaneo et al. (2018); Cattaneo and Titiunik (2022), with bias-corrected point estimates and robust bias-corrected inference (Calonico et al., 2014).[40]

[38] Each endogenous interaction between participation and a score polynomial term has its own first-stage using the same full instrument set.

[39] First-stage checks indicate strong instrument relevance in the linear specification, but weaker identification when quadratic participation×score interactions are included, particularly for AFAM-PE.

[40] For these estimates, I retain the fixed bandwidths used in the main analysis to maintain comparability

In addition, to estimate pooled effects across both programs (and across both waves when data is available), I combine the AFAM-PE and TUS samples. Let P_i indicate membership in the TUS sample, and let X_i, Z_i, and D_i denote the normalized eligibility score, baseline eligibility, and participation for the corresponding program. I estimate:

$$Y_i = \alpha_P + \beta_{\text{IV}}^{\text{pool}} D_i + \zeta P_i + \gamma_A (1 - P_i) X_i + \gamma_T P_i X_i + \theta_P D_i X_i + \delta_P' W_i + \varepsilon_{Pi} \tag{4}$$

I estimate this equation by 2SLS, instrumenting D_i and $D_i X_i$ with Z_i and $Z_i X_i$. The coefficient ζ allows the intercept to differ across programs. The specification also allows the relationship between the score and the outcome to differ across programs, while imposing common coefficients on participation and its interaction with the score. The coefficient $\beta_{\text{IV}}^{\text{pool}}$ summarizes the participation effect at the normalized cutoffs under these pooling restrictions. For outcomes available in both survey waves (such as life satisfaction and mental health) I additionally pool observations across waves and include a survey-wave indicator. Standard errors in these regressions are clustered at the individual level.

4.3 Internal validity

A valid RDD relies on two fundamental requirements (Imbens and Lemieux, 2008): (i) expected potential outcomes vary continuously at the cutoff, and (ii) crossing the cutoff affects outcomes only through program participation. These are discussed below.

Density tests provide evidence on whether households sort around the eligibility threshold (Lee and Lemieux, 2010). In this case, manipulation of the running variable is unlikely. The algorithm used to compute the ICC, including the variables, their weights, and the cutoffs, was confidential and periodically revised, limiting applicants' and local officials' ability to manipulate eligibility (Amarante and Vigorito, 2011). Panels (b) and (d) of Figure 1 provides visual evidence and formal density tests (McCrary, 2008). The estimated log-differences in density at the cutoffs are 0.140 (S.E. = 0.235) for AFAM-PE and 0.168 (S.E. = 0.266) for TUS, indicating no significant discontinuities.[41]

Discontinuities in covariates are tested using Equation (1), with each covariate as the outcome

[41] Tests from Cattaneo et al. (2018) yield p-values of 0.083 for AFAM-PE and 0.807 for TUS, while those by Bugni and Canay (2021) yield p-values of 0.401 and 1.000, respectively. Although most tests do not reject continuity, the Cattaneo test provides some evidence of a small discontinuity for AFAM-PE at the 10% level.

variable (Table A2). Panel A reports baseline household characteristics, while Panel B reports respondent characteristics. For the AFAM-PE sample, joint F-tests reject the null of balanced household characteristics in the linear specifications, with the imbalance driven by geographic location. Respondent characteristics also show some differences in age and sex across specifications. For TUS, the joint tests do not reject balance at conventional levels. To assess the role of regional composition, I conduct subgroup analyses restricting both program samples to interior residents (Table A3). This restriction compares households within the same geographic eligibility regime, removing differences in the composition of Montevideo vs. interior. Within these subsamples, the joint F-tests do not reject balance at the 5% level in either panel across specifications. These results support using the interior samples to assess the robustness of the main estimates.

The fuzzy RDD also relies on the local comparability implied by the continuity assumptions from above, together with (i) an instrument that affects treatment probability (relevance), (ii) an exclusion restriction, and (iii) monotonicity of participation with respect to eligibility. Eligibility strongly predicts participation in the preferred linear specification, with first-stage F-stats of 107.5 for AFAM-PE and 133.3 for TUS.[42] The exclusion restriction requires that eligibility affects outcomes only through participation in the program being evaluated. This restriction is plausible as the program-specific ICC cutoffs do not determine access to other distinct policies or services. For TUS, it additionally requires that AFAM-PE participation varies continuously at the TUS cutoff. Appendix Figure A5 shows that using AFAM-PE receipt is continuous around the TUS threshold. Monotonicity needs that eligibility does not reduce any household's participation relative to what it would have been without eligibility. The eligibility rules make this assumption plausible, as qualifying for a program expands access to benefits.

5 Results

5.1 Effects on the psychological costs of welfare stigma

Figure 2 graphically illustrates the impacts of AFAM-PE and TUS on the main outcomes of interest: personal, social, and overall stigma. The y-axis measures standardized outcome residuals after

[42] Sanderson–Windmeijer F-stats for the participation×score interactions are 153.7 for AFAM-PE and 202.1 for TUS. K–P tests reject under-identification for both programs ($p < 0.001$), with F-stats of 57.3 and 78.3, respectively.

controlling for sex, age, region, and ethnicity, 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.[43] Both programs increase personal stigma (Panels (a) and (d)), with a more pronounced effect for TUS. AFAM-PE shows no clear discontinuity in social stigma (Panel (b)), while TUS eligible individuals report higher levels relative to non-eligible ones near the cutoff (Panel (e)).[44] Panels (c) and (f) show the corresponding results for overall stigma. Linear fits better capture the data patterns near the cutoffs, while quadratic fits are less precise given the limited sample sizes.

Table 2 presents these results in regression form. Panels A and B report estimates for AFAM-PE and TUS, respectively, while Panel C pools both programs.[45] Under the preferred linear specification with controls, AFAM-PE and TUS eligibility increase personal stigma by 0.290 and 0.473 SD. Adjusting for non-compliance results in welfare participation effects of 0.524 and 0.724 SD, respectively. These magnitudes are substantial. For illustration, adding the AFAM-PE participation effect to the non-eligible mean shifts it from 1.23 to 2.81, corresponding approximately to a shift from the 66^{th} to the 77^{th} percentile within the AFAM-PE sample distribution. For TUS, the analogous shift is from 1.58 to 3.76, which corresponds to the 62^{th} and 78^{th} percentiles.[46]

Regarding social stigma, AFAM-PE shows no statistically significant effect, while for TUS is 0.790 SD. Again, adding this participation effect to the non-eligible mean shifts it from 0.97 to 2.18, corresponding to a shift from the 48^{th} to the 78^{th} percentile within the TUS sample. While both programs increase personal stigma, the contrast in social stigma aligns with the stated hypotheses.

The pooled estimates summarize these patterns across both program samples. Welfare participation increases personal stigma by 0.585 SD, social stigma by 0.359 SD, and overall stigma by 0.591 SD. All three estimates are statistically significant at the 5% level.

[43] Residuals are obtained by regressing each outcome on the controls. Panels (a)–(c) display results for AFAM-PE using the [-0.047; 0.073] bandwidth, while Panels (d)–(f) show TUS estimates using the [-0.400; 0.400] bandwidth. Figure A6 presents analogous plots using ten bins on each side of the cutoffs.

[44] The relatively flat slopes near the cutoffs contrast with Nicolau and Vigorito (2023), who document a negative correlation between shame proneness and household income in a broader Uruguayan population. This distinction is expected as in this case we analyze these variables over narrower range of socioeconomic circumstances within the RD samples.

[45] Appendix Table A4 reports estimates in raw sum units, providing an alternative interpretation of their magnitudes.

[46] Figure D2 compares the program specific and pooled sum distributions of these indexes.

5.1.1 Magnitude comparison with out of sample data

To contextualize the magnitude of the estimated effects, Figure 3 compares the pooled eligibility effects estimated by OLS with associations between stigma and socioeconomic characteristics in the ELBU comparison sample. It also marks the lower endpoints of the 95% confidence intervals for the pooled participation effects estimated by 2SLS. All estimates are expressed using simple sum indexes to facilitate comparison across datasets. The estimated eligibility effect on overall stigma is 1.336 points, compared with adjusted differences of 1.194 points associated with depressive symptoms and 0.816 points associated with poverty.[47] These comparisons suggest economically meaningful magnitudes, although the ELBU correlations should not be interpreted causally.

5.1.2 Heterogeneities by local context

The effects in social stigma are consistent with the programs' different delivery systems as stated before. AFAM-PE is delivered relatively privately, while the tagged TUS card makes benefit receipt observable particularly during purchases. This visibility may expose recipients to external judgment and mistreatment, consistent with prior research on social image concerns related to stigma (Friedrichsen et al., 2018; Celhay et al., 2025).[48] Table 3 examines this interpretation for TUS, allowing participation effects to differ between localities below and above the median density of authorized retailers, TUS beneficiary share, and poverty. Social stigma effects are larger in localities with fewer authorized retailers: the estimated splits are 1.628 SD below the median and 0.215 SD above it ($p = 0.014$ for equality of the effects). The corresponding estimates are 1.538 and 0.327 SD for lower and higher beneficiary shares ($p = 0.046$), and 1.613 and 0.393 SD for lower and higher poverty ($p = 0.075$).

Since these dimensions are correlated with each other, it is difficult to disentangle their separate roles. Nonetheless, all point toward greater social stigma where TUS use should be more conspicuous. Personal stigma estimates show the opposite pattern, with larger effects in localities with more retailers, beneficiaries, and poverty (although these differences are not significant at conventional levels). This pattern is consistent with participation reinforcing feelings of poverty shame in

[47] For personal stigma, the estimated eligibility effect is 1.020 points, compared with adjusted associations of 0.858 points with depressive symptoms and 0.497 points with poverty. For social stigma, the eligibility effect of 0.315 points is similar in magnitude to the corresponding associations of 0.307 and 0.348 points.

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

deprived localities, even where receipt is more common and may attract less external attention.

5.1.3 Robustness

Several robustness checks assess the sensitivity of the welfare stigma estimates to different methods of index construction.[49] I also examine robustness to estimation windows, specifications, and sample composition. Narrower bandwidths and donut specifications preserve the broad pattern of positive personal stigma effects for each program and positive social stigma effects for TUS (Appendix Tables A7 and A8).[50] Local linear estimates using alternative kernels yield qualitatively similar patterns, and all estimates remain statistically significant at the 10% level under robust bias-corrected inference (Appendix Table A9).

To examine whether covariate imbalances influence the results, I re-estimate the analyses using a subsample restricted to interior residents. The estimates remain broadly consistent, although some are less precise (Appendix Table A10). I also assess whether the TUS social stigma result is driven by recipients who had left the program before the interview, restricting receipt to a period close to the survey: this yields an estimate of 0.782 SD (SE = 0.310, $p = 0.012$).[51]

Furthermore, I check robustness to attrition between survey waves. Adding controls for observed predictors of attrition and applying inverse probability weights for survey retention yields results consistent with the main estimates (Appendix Table A11). To assess sensitivity to selective attrition beyond adjustment for observed covariates, Appendix Table A12 reports covariate adjusted Lee bounds for both eligibility and participation effects, under the additional assumption that eligibility affects retention in the same direction for all households within each program (the direction is decreasing retention for AFAM-PE and increasing retention for TUS). For AFAM-PE, the participation bounds remain positive for personal stigma, ranging from 0.409 to 1.016 SD. For TUS, the social stigma bounds remain positive, ranging from 0.109 to 1.478 SD, although the personal stigma bounds range from -0.043 to 1.472 SD. Overall stigma bounds remain positive for

[49] Appendix Table A5 compares the preferred personal stigma index with PCA- and factor loading-weighted scores, yielding similar effects for both programs. Leaving out each personal stigma item in turn also produces similar estimates, indicating that results are not driven by any single item (Appendix Figure A7). The main patterns also remain when missing items are imputed zeros (Appendix Table A6).

[50] Figure A8 shows coefficients for additional bandwidth reductions. Figure A9 reports placebo estimates at alternative cutoffs away from the eligibility thresholds.

[51] This window covers the three months preceding the interview, corresponding to the reference period of the social stigma questions.

both programs, and the eligibility bounds display the same pattern of signs.

5.1.4 Effects on individual shame and mistreatment items

One can draw further insights from analyzing the individual items within the personal and social stigma scales. Appendix Tables A13 and A14 report participation effects on the eight retained personal stigma items and the three social stigma items, respectively. Both tables report standardized effects, resampling based p-values, and Romano–Wolf adjusted p-values accounting for multiple hypothesis testing (Clarke et al., 2020). For AFAM-PE, the strongest adjusted evidence is concentrated in self-consciousness: the estimated effect is 0.537 SD, with an adjusted p-value of 0.099. For TUS, the strongest personal stigma estimates concern feeling laughable and disgusting, with adjusted p-values of 0.079 and 0.089, respectively. For social stigma, TUS increases reports of unfair treatment and discrimination by 0.605 and 0.589 SD, respectively. Overall, these results are consistent with broader experiences of shame and mistreatment under TUS, while also showing that the aggregate indices provide clearer evidence than most individual items.[52]

5.2 Other behavioral responses

I next examine whether participation generates labor market and consumption responses that a planner might value. Such responses, alongside potential targeting gains emphasized by the ordeal argument, could be weighed against the psychological costs imposed on recipients. Table 4 reports labor market estimates pooling both programs and survey waves, using observations from all household members. I find no statistically significant effects on employment, informality, or hours worked, although welfare participation increases the probability of searching for a job by 8.3 p.p. (SE = 2.6), relative to a non-eligible mean of 13.5%.[53] Overall, these results show that welfare participation encourages job search, with limited evidence of changes in other labor outcomes.

I further analyze whether TUS participation changes the composition of household food expenditures. Welfare stigma may additionally act as a self-enforcement mechanism if recipients anticipate judgment about how they spend their benefits, encouraging choices perceived as health-

[52] For completeness, Appendix Table A15 reports the corresponding estimates for feeling blushed and childish, the two items excluded from the personal stigma index.

[53] Informality and hours are conditional on employment. Appendix Table A16 reports the same specifications restricted to survey respondents. The job search estimate is 7.7 p.p. (SE = 4.9) but it is not significant at 5%.

ier or more socially desirable. As such, candy is a potentially stigmatized good in this setting, since purchasing sweets with welfare benefits may be viewed as an unneeded indulgence. Appendix Table A17 reports TUS effects on the shares of food expenditures allocated to fruits and vegetables, eggs, and candy, both overall and by personal and social stigma. Several things are worth noting. First, there are no statistically significant average effects on these expenditure shares. Second, in the social stigma split, the estimated change in the candy share is -0.8 p.p. (SE = 0.4) among households whose respondents report stigma at or above the median, compared with 0.5 p.p. (SE = 0.6) below the median. Third, the difference itself is statistically significant at the 10% level. Differences in the other food shares, and across the personal stigma split, are not statistically significant at conventional levels. This pattern is consistent with anticipated social judgment discouraging purchases of potentially stigmatized goods.

5.3 Effects on subjective wellbeing

Improving the wellbeing of low-income populations is a central objective of social protection. The increases in stigma documented above therefore raise the question of whether the psychological costs of participation may offset at least some of the benefits that come from additional monetary resources. We have descriptive evidence documenting shame and humiliation as predictors of life satisfaction (Hojman and Miranda, 2018), and declines in subjective wellbeing have often been interpreted as a result of welfare stigma in previous work (Gao and Zhai, 2017; Qi and Wu, 2018). Figure A10 shows strong negative correlations between personal and social stigma, and life satisfaction in the ESAFAM data.[54]

To examine the overall implications for recipients' wellbeing, I estimate the effects of AFAM-PE and TUS on self-reported life satisfaction and mental health. Life satisfaction is measured on a scale from 1 (very dissatisfied) to 10 (very satisfied). The mental health indicator equals one if respondents report no mental, psychological, or intellectual difficulties, such that higher values indicate better outcomes for both measures. Table 5 reports estimates pooling both programs and survey waves. In the preferred specification, participation reduces life satisfaction by 0.638 points (SE = 0.309) and the probability of reporting no mental health difficulties by 5.5 p.p. (SE

[54]The figure also shows these relationships using ELBU data for both adult and youth respondents, as described in Section 3. Appendix Figure A11 reports the corresponding correlations with depressive symptoms reported by respondents in ELBU.

= 2.4). Both effects are statistically significant at the 5% level. These results indicate that the benefits of additional resources do not necessarily translate into improvements in recipients' self-reported wellbeing. Together with the increases in stigma documented above, they are consistent with psychological costs possibly offsetting the benefits of participation.

5.3.1 Mediation analysis

To explore the role of welfare stigma in the subjective wellbeing effects, I conduct a mediation decomposition following the logic of Heckman et al. (2013). The analysis uses the second survey wave and combines the estimated effects of eligibility on stigma outcomes with its conditional associations with life satisfaction. Let Y_i denote standardized life satisfaction and M_i the standardized stigma measures, including either personal and social stigma jointly or the overall index. I estimate:

$$\begin{aligned} D_i &= \alpha_D + \pi Z_i + q_i'\eta_D + u_{Di} \\ M_{ki} &= \alpha_{M_k} + \xi_k Z_i + q_i'\eta_k + u_{ki}, \qquad k = 1, \ldots, K \\ Y_i &= \alpha_Y + cZ_i + \phi' M_i + q_i'\eta_Y + u_{Yi} \end{aligned}$$

where q_i contains the linear running variable terms, their interactions with eligibility, the set of controls, and program specific terms in the case of the pooled (across programs) specification. The coefficient ξ_k captures the eligibility effect on stigma measure k, while ϕ_k captures its conditional association with life satisfaction.

The contribution associated with each stigma measure is the product of these coefficients. To decompose this Wald estimator, I divide both the stigma and remaining reduced form components by the first stage discontinuity to express both components in participation-effect units:

$$\widehat{\beta}_{\mathrm{IV},Y} = \frac{\sum_{k=1}^{K} \widehat{\phi}_k \widehat{\xi}_k}{\widehat{\pi}} + \frac{\widehat{c}}{\widehat{\pi}} = \widehat{C}_M + \widehat{C}_R \tag{5}$$

Here, $\widehat{\beta}_{\mathrm{IV},Y}$ is the second-wave Wald ratio used for this decomposition, computed from the reduced-form eligibility effect on life satisfaction and the first stage using the same sample and regressors. The share accounted for by stigma is $\widehat{C}_M / \widehat{\beta}_{\mathrm{IV},Y}$. Moreover, I estimate standard errors by resampling

households and re-estimating the full decomposition over 1,000 bootstrap replications.[55]

Figure 4 presents this decomposition using personal and social stigma jointly in Panel (a), and overall stigma in Panel (b). In the pooled sample, the personal and social stigma components are -0.110 and -0.051 SD, respectively, accounting for approximately 94% of the estimated decline in life satisfaction. Using the overall stigma index yields a similar component of -0.164 SD, accounting for approximately 95% of the total effect (-0.172 SD). The program specific patterns follow closely the main stigma results. This exercise provides suggestive evidence that the increases in stigma are large enough to account for much of the estimated decline in life satisfaction.

5.4 Computing income equivalent wellbeing costs

To provide an illustrative benchmark for the magnitude of the stigma related wellbeing loss, I express it in income equivalent terms, drawing on the subjective wellbeing valuation literature (e.g., Clark and Oswald, 2002; Oswald and Powdthavee, 2008; Frey et al., 2010). To do this, I combine the estimated stigma related life satisfaction loss from welfare participation, $\widehat{L}$, with the conditional association between life satisfaction and log household income in ELBU, $\widehat{\kappa}$. I use ELBU as an external benchmark for the income vs. life satisfaction relationship. ELBU provides repeated observations of both variables for Uruguayan households, allowing me to use (permanent) income averaged across waves to reduce the influence of transitory fluctuations. To make the benchmark more relevant to the population studied here, I restrict the ELBU sample to adults reporting AFAM-PE or TUS receipt in either wave. The corresponding income equivalent percentage, $\widehat{\text{IE}}$, is then:

$$\widehat{\text{IE}} = 100 \left[\exp\left(\frac{\widehat{L}}{\widehat{\kappa}} \right) - 1 \right] \tag{6}$$

Using the overall stigma index, the pooled estimate yields an income equivalent loss of around 58.2% of household income. Figure 5 presents the corresponding bootstrap distribution, with a 95% bootstrap interval of [19.1%, 131.5%].[56] This exercise suggests substantial wellbeing costs,

[55] A causal interpretation of this analysis would require that stigma has the same effect on life satisfaction across eligible and non-eligible individuals and is unrelated to omitted determinants of life satisfaction after conditioning on eligibility and other controls. These assumptions may not hold though. For example, distress could increase reported stigma and reduce life satisfaction, confounding their relationship. The RD estimates identify participation effects on both outcomes, but cannot independently identify the effect of stigma on life satisfaction, so these should be interpreted as an accounting decomposition.

[56] Following the accounting decomposition, $\widehat{L} = -\widehat{\phi}\widehat{\xi}/\widehat{\pi}$, with life satisfaction rescaled to 0–1 in both datasets. The ELBU income coefficient comes from a pooled OLS regression using log real household income averaged across

although the interval is relatively wide.

6 Conclusion

This paper empirically examines welfare stigma after take-up by analyzing the effects of Uruguay's two largest social assistance programs, AFAM-PE and TUS. Using a regression discontinuity design that combines administrative and survey data, it identifies the causal effects of participation on self-reported feelings of shame and perceived mistreatment by others. Both programs increase personal stigma, while TUS also increases social stigma, a dimension for which AFAM-PE shows no statistically significant effect. These findings provide evidence that receiving government support comes with psychological costs that are likely shaped by institutional design. In particular, the public visibility of TUS, delivered through a marked card used at authorized retailers, may expose recipients to public scrutiny and reinforce social stigma. This interpretation is consistent with the locality level results and qualitative evidence from Uruguay, highlighting local visibility as a potential channel through which welfare participation affects recipients' experiences.

While this study documents the existence and nature of welfare stigma, understanding its precise mechanisms remains an open question. Future research could directly test the role of benefit visibility by randomly assigning generic, untagged cards or standard payment procedures that make welfare participation less conspicuous. Theoretical models that incorporate these psychological costs distinct from material benefits and behavioral responses would also help to structurally assess the net welfare effects of cash transfers. Moreover, repeated measures of stigma linked to administrative records could shed light on how these costs evolve over time, when exactly they arise, and if these indeed persist after recipients exit.

These findings highlight the importance of accounting for psychological costs in social protection design. Reducing program visibility, including through a shared, less conspicuous payment system for AFAM-PE and TUS, could be an option worth implementing. Complementary interventions targeting internal shame may also help since psychological or identity based programs can strengthen self-worth and improve decision making among marginalized populations (Ghosal

waves, including transfers, with wave indicators and controls for household size, age and age squared, sex, ethnicity, and geographic location. It is worth noting the caveat that this valuation remains sensitive to income endogeneity concerns and to the comparability of the populations underlying the two estimates (Powdthavee, 2010; Fujiwara, 2013).

et al., 2022). Framing benefits as entitlements, particularly those tied to children's wellbeing, may similarly reinforce beneficiaries' agency and legitimacy, consistent with the importance of framing emphasized by Bertrand et al. (2006). Preserving dignity should be part of policy design.

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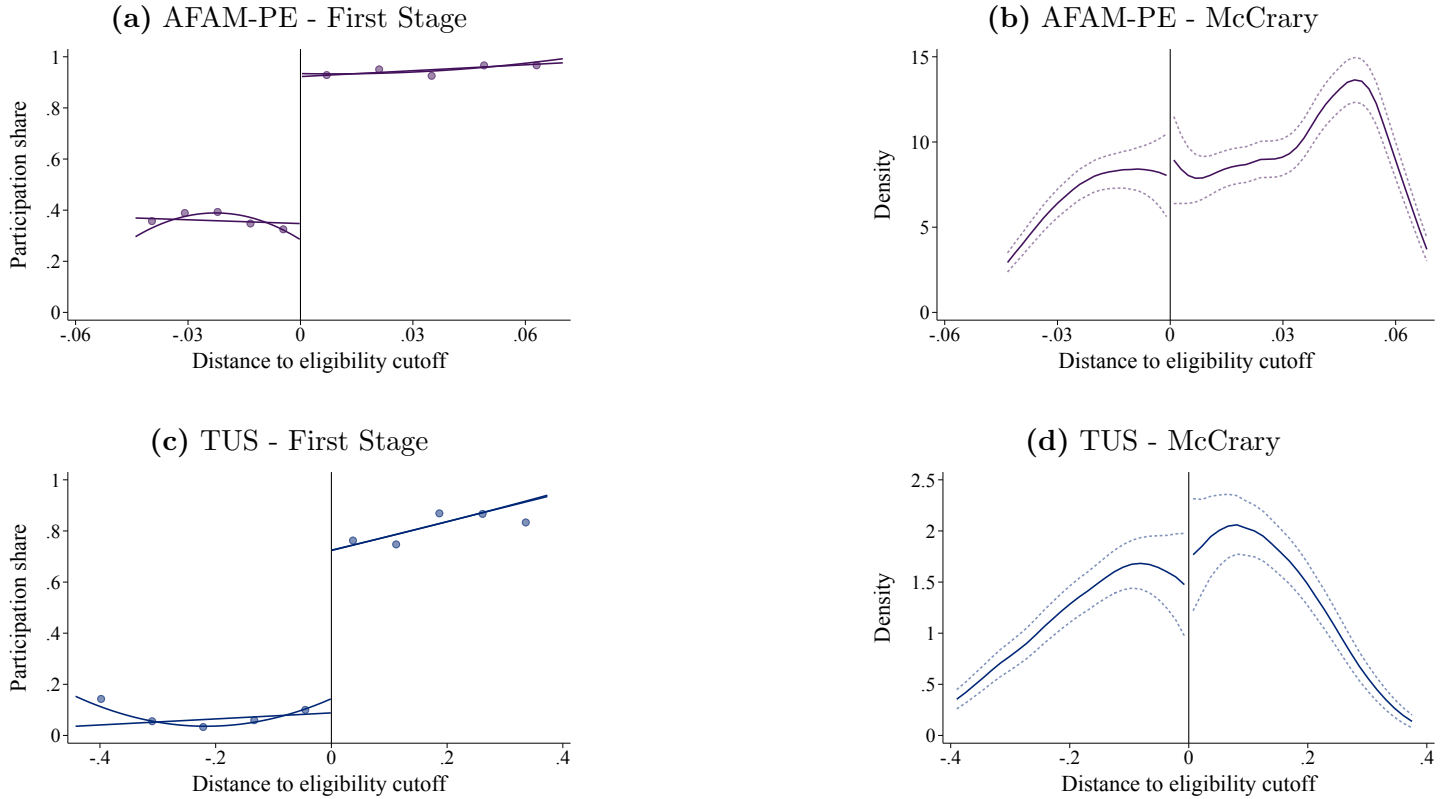
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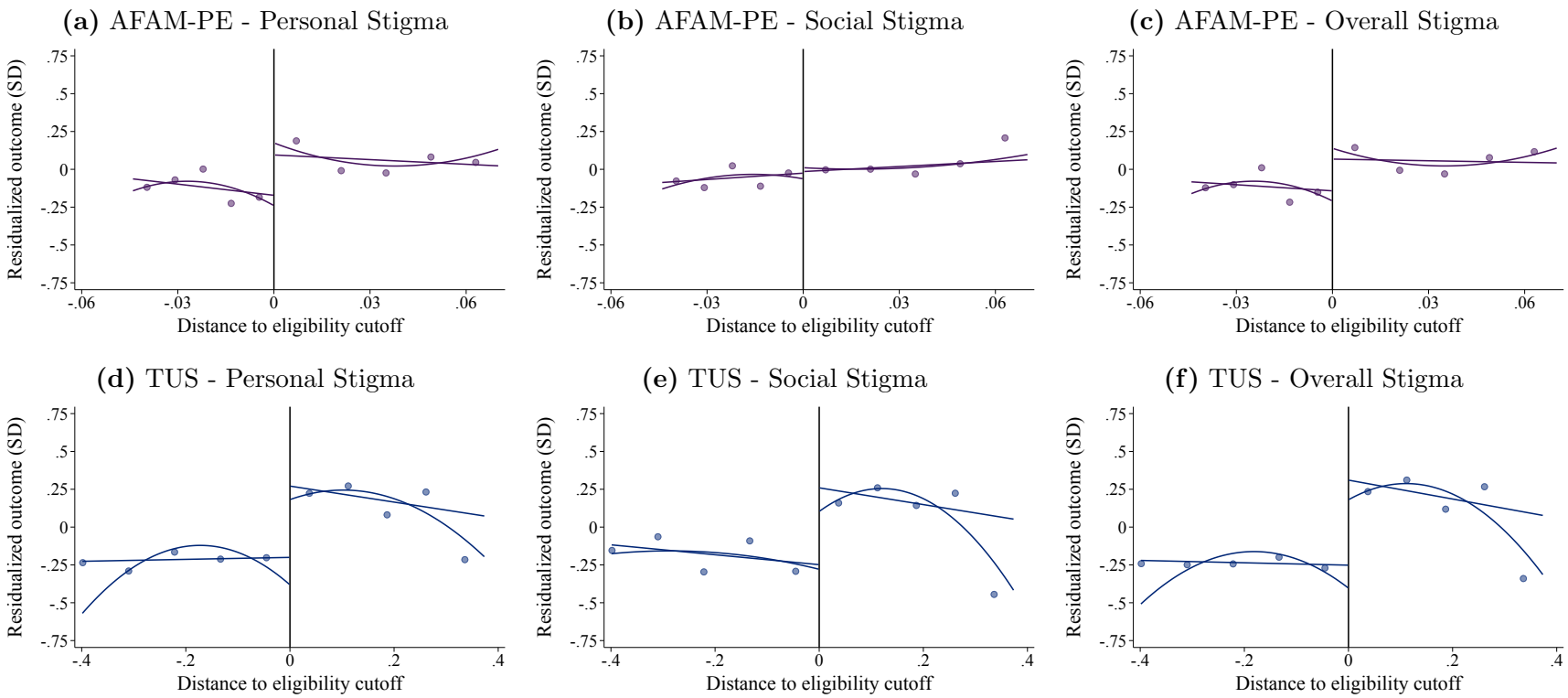
Figures and Tables

Figure 1:
RDD Identification Validity Requirements



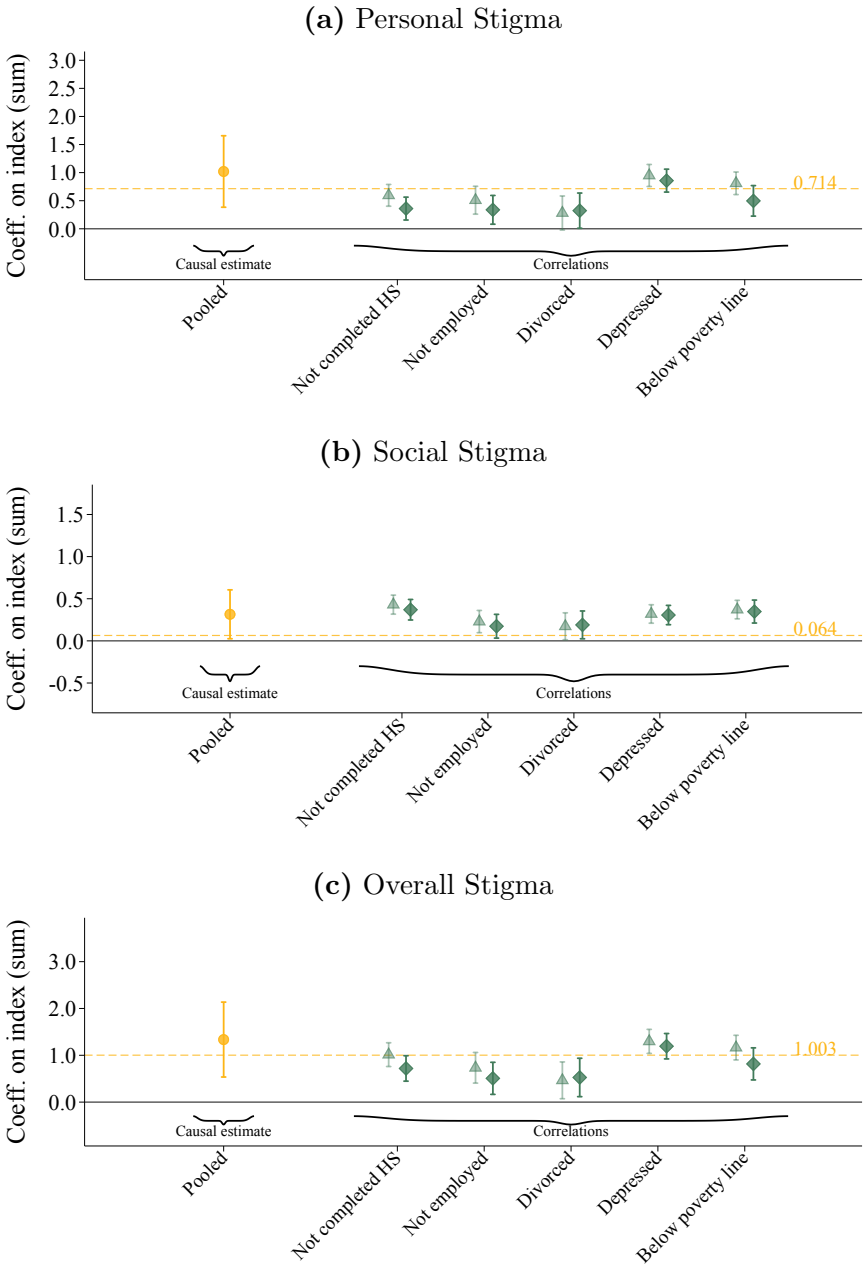
Notes: This figure reports first stage relationships and tests for manipulation of the eligibility score. Panels (a) and (c) plot participation against the normalized eligibility score for AFAM-PE and TUS, respectively. Dots report mean participation within ten equal width bins (five on each side of the cutoff). Solid lines show fitted linear and quadratic trends. Estimated first stage discontinuities are 0.56 ($p = 0.000$) for AFAM-PE and 0.65 ($p = 0.000$) for TUS. Panels (b) and (d) report density estimates for AFAM-PE and TUS, respectively. Solid lines show local linear density estimates. Dashed lines indicate 95% confidence intervals. Estimated log density discontinuities are $\hat{\tau} = 0.140$ (S.E. 0.235) for AFAM-PE and $\hat{\tau} = 0.168$ (S.E. 0.266) for TUS. For AFAM-PE and TUS, respectively, p-values from the Cattaneo et al. (2018) test are 0.083 and 0.807, and those from the Bugni and Canay (2021) test are 0.401 and 1.000. Panels (a) and (b) use bandwidth $[-0.040; 0.070]$; Panels (c) and (d) use bandwidth $[-0.400; 0.400]$. Vertical lines indicate normalized eligibility cutoffs.

Figure 2:
RDD Impact Estimates of AFAM-PE and TUS on Perceived Stigma



Notes: This figure plots personal, social, and overall stigma (z-scores over each program sample) against the normalized eligibility score. Outcomes are residualized with respect to sex (women = 1), age, region (Montevideo = 1), and ethnicity (white = 1). Dots report mean residualized outcomes within ten bins (five on each side of the cutoff). Solid lines show fitted linear and quadratic trends. Estimation bandwidths are $[-0.040; 0.070]$ for AFAM-PE and $[-0.400; 0.400]$ for TUS. Vertical lines indicate eligibility cutoffs at zero. Table 2 reports the corresponding regression estimates.

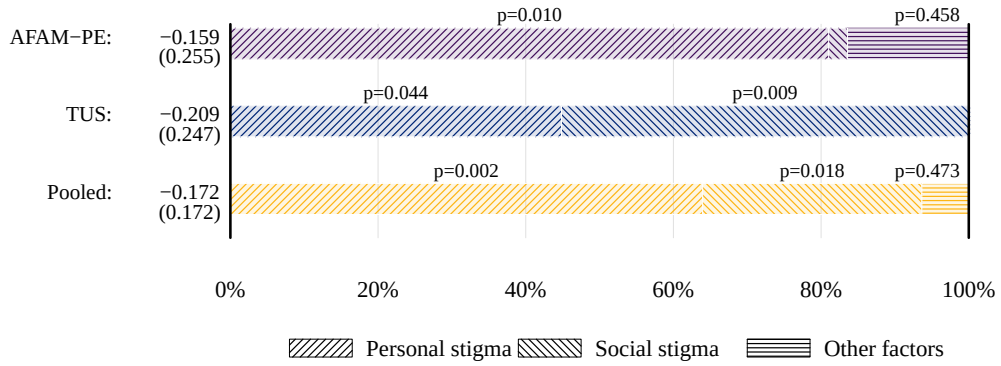
Figure 3:
Benchmarking the Magnitude of Welfare Stigma Effects Using External Data



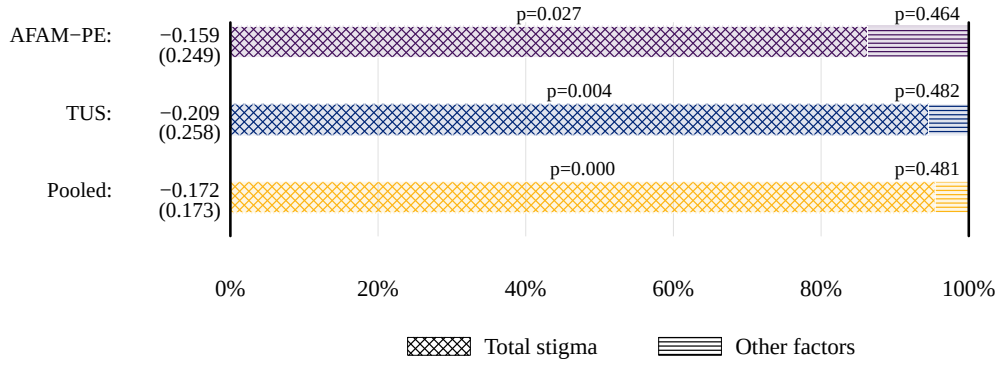
Notes: This figure benchmarks the magnitude of the causal effects of welfare eligibility against descriptive associations with stigma variables using ELBU data. The first group of coefficients reports reduced form RDD effects of eligibility in the pooled AFAM-PE and TUS sample. The middle group reports OLS associations between stigma and socioeconomic or mental health characteristics in ELBU. Bars indicate 95% confidence intervals. The RDD specification controls for sex (women = 1), age, region (Montevideo = 1), ethnicity (white = 1), and program and survey-wave indicators. ELBU specifications include the same demographic controls, log per-capita household income, and survey-wave indicators. Standard errors are clustered by ICC score in the RDD specification and by respondent in the ELBU specifications.

Figure 4:
Decomposing the Effect of Welfare Participation on Life Satisfaction

(a) Personal and Social Stigma

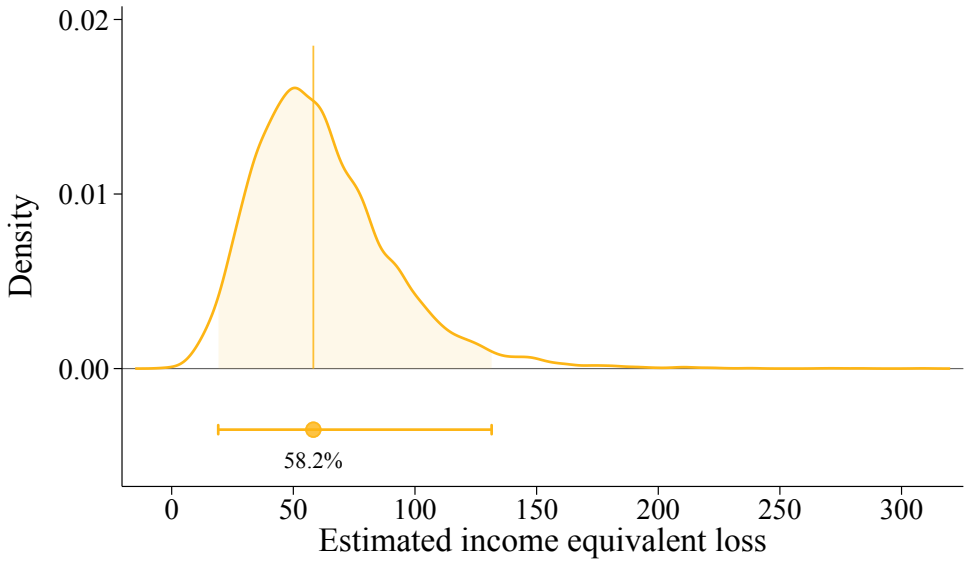


(b) Overall Stigma



Notes: This figure reports a mediation analysis of the RDD effect of welfare participation on life satisfaction. Indirect components combine the estimated effects on stigma with the conditional associations between stigma and life satisfaction. The direct component is the remaining participation effect after subtracting the indirect components. Eligibility effects are estimated in reduced form and divided by the first stage effect on participation. All components are expressed in standard deviation units. Controls include sex (women = 1), age, region (Montevideo = 1), and ethnicity (white = 1). Standard errors are computed from the bootstrap distribution of each component across 1,000 replications. Displayed p-values are calculated as $\Phi(-|\hat{\theta}/\widehat{SE}_{\text{boot}}|)$, where Φ is the standard normal cumulative distribution function, $\hat{\theta}$ is the estimated component, and $\widehat{SE}_{\text{boot}}$ is its bootstrap standard error. For readability, p-values are shown only for sufficiently large bar segments.

Figure 5:
Income Equivalence of Stigma Related Wellbeing Losses



Notes: This figure expresses the life satisfaction loss associated with overall stigma in income equivalent terms, as a percentage of household income. The estimate is calculated as $100[\exp(\widehat{L}/\widehat{\kappa}) - 1]$, where $\widehat{L}$ is the magnitude of the stigma component from the pooled mediation decomposition and $\widehat{\kappa}$ is the conditional association between life satisfaction and log household income in ELBU. Life satisfaction is rescaled to 0–1 making the ESAFAM and ELBU measures follow a common range since they have different original scales (1–10 vs. 1–5, respectively). Controls include household size, age, age squared, sex, ethnicity, geographic location, and survey-wave indicators. The vertical line and dot indicate the point estimate. The curve shows the smoothed density of 10,000 bootstrap replications, independently resampling ESAFAM score clusters within program-by-eligibility strata and ELBU households. The shaded region and horizontal bar indicate the central 95% bootstrap interval.

Table 1:
Summary Statistics of Stigma Related Variables

Variable	Pooled sample			AFAM-PE			TUS		
	Obs. (1)	Mean (2)	S.D. (3)	Obs. (4)	Mean (5)	S.D. (6)	Obs. (7)	Mean (8)	S.D. (9)
Panel A: Personal Stigma									
Question: For each of the following feelings, please place a number from 0 to 3 reflecting how frequently the feeling is for you.									
Embarrassed	1,688	0.28	0.61	1,091	0.24	0.55	597	0.34	0.70
Ridiculous	1,666	0.21	0.58	1,075	0.17	0.49	591	0.28	0.70
Self-conscious	1,685	0.40	0.78	1,089	0.35	0.72	596	0.48	0.87
Humiliated	1,688	0.21	0.57	1,091	0.19	0.53	597	0.25	0.63
Stupid	1,666	0.19	0.54	1,075	0.18	0.51	591	0.21	0.61
Childish	1,674	0.36	0.73	1,085	0.36	0.72	589	0.37	0.74
Helpless	1,676	0.21	0.56	1,085	0.21	0.55	591	0.21	0.57
Blushed	1,686	0.75	0.93	1,093	0.72	0.89	593	0.80	1.00
Laughable	1,650	0.24	0.63	1,067	0.20	0.55	583	0.33	0.74
Disgusting	1,635	0.09	0.40	1,059	0.08	0.36	576	0.12	0.46
Personal stigma (sum)	1,603	1.83	3.01	1,038	1.61	2.64	565	2.22	3.56
Personal stigma (z-score)	1,603	0.00	1.00	1,038	-0.07	0.88	565	0.13	1.18
Panel B: Social Stigma									
Question: Have you felt that you have been [...] during the last three months?									
Disrespected	1,708	0.44	0.79	1,108	0.42	0.77	600	0.48	0.83
Treated unfairly	1,689	0.44	0.78	1,091	0.40	0.72	598	0.51	0.88
Discriminated against	1,715	0.22	0.65	1,111	0.19	0.60	604	0.28	0.73
Social stigma (sum)	1,687	1.11	1.53	1,091	1.02	1.40	596	1.27	1.73
Social stigma (z-score)	1,687	-0.00	1.00	1,091	-0.06	0.91	596	0.11	1.13
Panel C: Overall Stigma									
Overall stigma (sum)	1,600	2.93	3.90	1,036	2.61	3.41	564	3.51	4.62
Overall stigma (z-score)	1,600	0.00	1.00	1,036	-0.08	0.87	564	0.15	1.18

Notes: This table reports survey items measuring stigma, shame, and humiliation, along with descriptive statistics for the personal, social, and overall stigma indices. Responses range from 0 (rarely or never) to 3 (always or almost always), with intermediate categories of 1 (occasionally) and 2 (often). Higher values indicate more frequent feelings or experiences. In Panel A, bold identifies the items retained in the preferred personal stigma index with Appendix D describing item selection. Each aggregate outcome is reported as a simple sum and as a standardized index with mean zero and standard deviation one in the pooled AFAM-PE and TUS estimation sample. The overall stigma index adds up the retained personal and social stigma items.

Table 2:
RDD Impact Estimates of Welfare Participation on Perceived Stigma

	Personal stigma		Social stigma		Overall stigma	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: AFAM-PE						
$\hat{\beta}_{\text{OLS}}$	0.228*	0.290**	0.018	0.015	0.182	0.229*
	(0.116)	(0.124)	(0.116)	(0.122)	(0.115)	(0.123)
$\hat{\beta}_{\text{IV}}$	0.396*	0.524**	0.031	0.026	0.317	0.414*
	(0.204)	(0.227)	(0.202)	(0.220)	(0.202)	(0.224)
Obs.	1,036	1,036	1,036	1,036	1,036	1,036
Panel B: TUS						
$\hat{\beta}_{\text{OLS}}$	0.459**	0.473**	0.491***	0.511***	0.546***	0.565***
	(0.193)	(0.194)	(0.165)	(0.164)	(0.183)	(0.184)
$\hat{\beta}_{\text{IV}}$	0.705**	0.724**	0.764***	0.790***	0.843***	0.868***
	(0.295)	(0.296)	(0.258)	(0.254)	(0.284)	(0.283)
Obs.	562	562	562	562	562	562
Panel C: Pooled						
$\hat{\beta}_{\text{OLS}}$	0.309***	0.339***	0.205**	0.206**	0.318***	0.342***
	(0.100)	(0.102)	(0.092)	(0.094)	(0.096)	(0.098)
$\hat{\beta}_{\text{IV}}$	0.517***	0.585***	0.346**	0.359**	0.534***	0.591***
	(0.169)	(0.177)	(0.155)	(0.162)	(0.163)	(0.171)
Obs.	1,598	1,598	1,598	1,598	1,598	1,598
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1	1	1	1	1	1

Notes: This table reports regression discontinuity estimates of the effects of eligibility for and participation in welfare programs on (z-scores over the pooled sample) personal, social, and overall stigma. Panels A and B report estimates for AFAM-PE and TUS, respectively; Panel C reports pooled estimates. Rows labeled $\hat{\beta}_{\text{OLS}}$ report reduced form effects of eligibility, while rows labeled $\hat{\beta}_{\text{IV}}$ report fuzzy RDD effects of participation, following Equations (1) and (3), respectively. Controls include sex (women = 1), age, region (Montevideo = 1), and ethnicity (white = 1). Estimation bandwidths are $[-0.040; 0.070]$ for AFAM-PE and $[-0.400; 0.400]$ for TUS. Standard errors clustered by ICC score in parentheses. Figure 2 presents the corresponding RD plots. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 3:
Heterogeneous RDD Impact Estimates of TUS on Perceived Stigma

	Personal stigma		Social stigma	
	(1)	(2)	(3)	(4)
Panel A: TUS				
$\hat{\beta}_{\text{IV}}$	0.629**	0.627**	0.838***	0.831***
	(0.310)	(0.309)	(0.265)	(0.257)
Panel B: TUS Affiliated Shop % Split				
$\hat{\beta}_{\text{IV}}$	0.211	0.275	1.653***	1.628***
	(0.563)	(0.570)	(0.503)	(0.506)
$\hat{\beta}_{\text{IV}} + \hat{\chi}$	0.838**	0.778**	0.210	0.215
	(0.345)	(0.338)	(0.279)	(0.274)
Diff. F-Test	0.907	0.559	6.526**	6.035**
Panel C: TUS Recipient % Split				
$\hat{\beta}_{\text{IV}}$	0.019	-0.004	1.592***	1.538***
	(0.515)	(0.511)	(0.542)	(0.540)
$\hat{\beta}_{\text{IV}} + \hat{\chi}$	0.960**	0.929**	0.328	0.327
	(0.397)	(0.387)	(0.286)	(0.277)
Diff. F-Test	2.116	2.129	4.281**	3.973**
Panel D: Poverty Split				
$\hat{\beta}_{\text{IV}}$	-0.102	-0.116	1.653***	1.613**
	(0.607)	(0.610)	(0.636)	(0.634)
$\hat{\beta}_{\text{IV}} + \hat{\chi}$	0.918**	0.934**	0.348	0.393
	(0.382)	(0.375)	(0.274)	(0.271)
Diff. F-Test	2.049	2.192	3.573*	3.160*
Obs.	498	498	498	498
Controls	No	Yes	No	Yes
Polynomial	1	1	1	1

Notes: This table reports fuzzy regression discontinuity estimates of the effects of TUS participation on personal and social stigma (z-scores over the pooled sample), allowing effects to vary by locality context. Panel A reports the baseline TUS estimate. Panels B–D report estimates from separate regressions interacting participation with indicators for at/above-median locality level shop density, TUS beneficiary density, and poverty level, respectively. Rows labeled $\hat{\beta}_{\text{IV}}$ report effects for below-median localities, while rows labeled $\hat{\beta}_{\text{IV}} + \hat{\chi}$ report effects for at/above-median localities, where $\hat{\chi}$ is the estimated interaction coefficient. Diff. F-Test reports the F-test of equality between the two effects. Controls include sex (women = 1), age, region (Montevideo = 1), and ethnicity (white = 1). Panel A includes only these controls. In Panels B–D, controlled specifications also include the other two locality-context variables not used to define the split. Standard errors clustered by ICC score in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 4:
RDD Impact Estimates of Welfare Participation on Labor Market Responses

	Employed		Informal		Hours		Search	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\hat{\beta}_{\text{OLS}}$	0.011	0.033	-0.000	0.000	-2.10*	-1.54	0.048***	0.051***
	(0.024)	(0.023)	(0.025)	(0.025)	(1.16)	(1.13)	(0.016)	(0.016)
$\hat{\beta}_{\text{IV}}$	0.010	0.046	-0.001	-0.001	-3.42*	-2.56	0.076***	0.083***
	(0.038)	(0.038)	(0.041)	(0.041)	(1.88)	(1.84)	(0.026)	(0.026)
N-E mean	0.536	0.536	0.480	0.480	38.40	38.40	0.135	0.135
Obs.	8,299	8,299	4,242	4,242	4,242	4,242	8,299	8,299
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Polynomial	1	1	1	1	1	1	1	1

Notes: This table reports pooled fuzzy regression discontinuity estimates of the effects of welfare participation on labor market outcomes among all household members. The sample pools AFAM-PE and TUS observations from both survey waves. All specifications include survey wave (Wave 2 = 1) and program (TUS = 1) indicators, program specific linear running variable terms, and their interactions with eligibility or participation. Employed indicates whether the individual worked at least one hour in the previous week, excluding household chores. Informal indicates whether the individual does not contribute to social security through their job. Hours measures usual weekly hours worked across all jobs. Search indicates whether the individual searched for a job in the previous week to the survey date. Informality and hours worked are conditional on employment. N-E mean reports the outcome mean among non-eligible individuals. Controls include sex (women = 1), age, region (Montevideo = 1), and ethnicity (white = 1). Standard errors clustered at the individual level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 5:
RDD Impact Estimates of Welfare Participation on Mental Health and Life Satisfaction

	Mental health		Life satisfaction	
	(1)	(2)	(3)	(4)
$\hat{\beta}_{\text{OLS}}$	-0.026*	-0.033**	-0.337*	-0.400**
	(0.015)	(0.014)	(0.184)	(0.187)
$\hat{\beta}_{\text{IV}}$	-0.043*	-0.055**	-0.522*	-0.638**
	(0.023)	(0.024)	(0.297)	(0.309)
N-E mean	0.981	0.981	6.839	6.839
Obs.	3,022	3,022	3,022	3,022
Controls	No	Yes	No	Yes
Polynomial	1	1	1	1

Notes: This table reports pooled fuzzy regression discontinuity estimates of the effects of welfare participation on mental health and life satisfaction among survey respondents. The sample pools AFAM-PE and TUS observations from both survey waves. All specifications include survey wave (Wave 2 = 1) and program (TUS = 1) indicators, program specific linear running variable terms, and their interactions with eligibility or participation. Mental health equals one if the respondent reports no mental, psychological, or intellectual difficulty (effects are expressed in probability terms). Life satisfaction is based on 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?"* Responses are analyzed on the original 1–10 scale. Controls include sex (women = 1), age, region (Montevideo = 1), and ethnicity (white = 1). Standard errors clustered at the individual level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

A Appendix Figures and Tables

Figure A1:
Card Comparison Across Programs

(a) AFAM-PE debit card



Source: BROU

(b) TUS food card

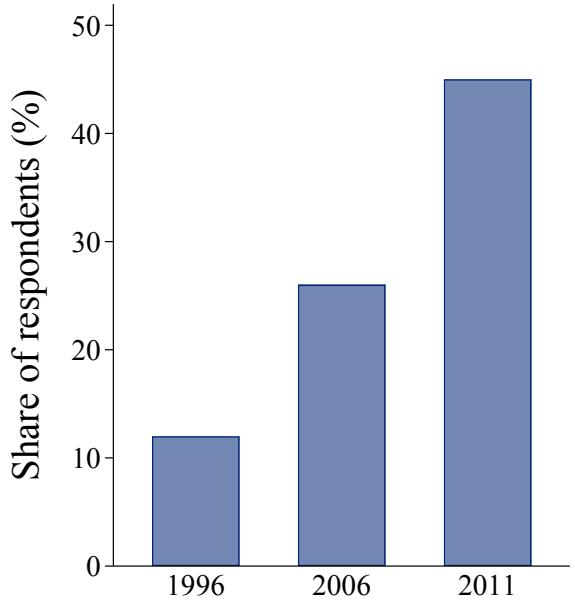


Source: BROU

Notes: This figure shows the layouts of the magnetic cards through which each cash transfer is typically delivered to households. Panel (a) exhibits an example of a debit card for AFAM-PE (even though recipients can choose any bank card of their choosing). Panel (b) exhibits the only available food card for TUS at the time of the study period, showing its governmental and social plan tag.

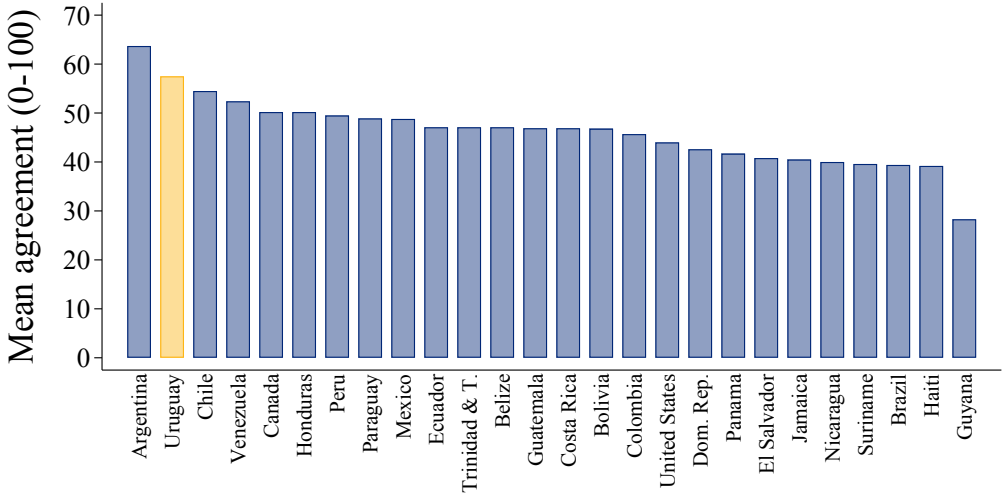
Figure A2:
Beliefs About Poverty Causes and Welfare Stigma in Uruguay

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



Source: Zuasnabar (2018)

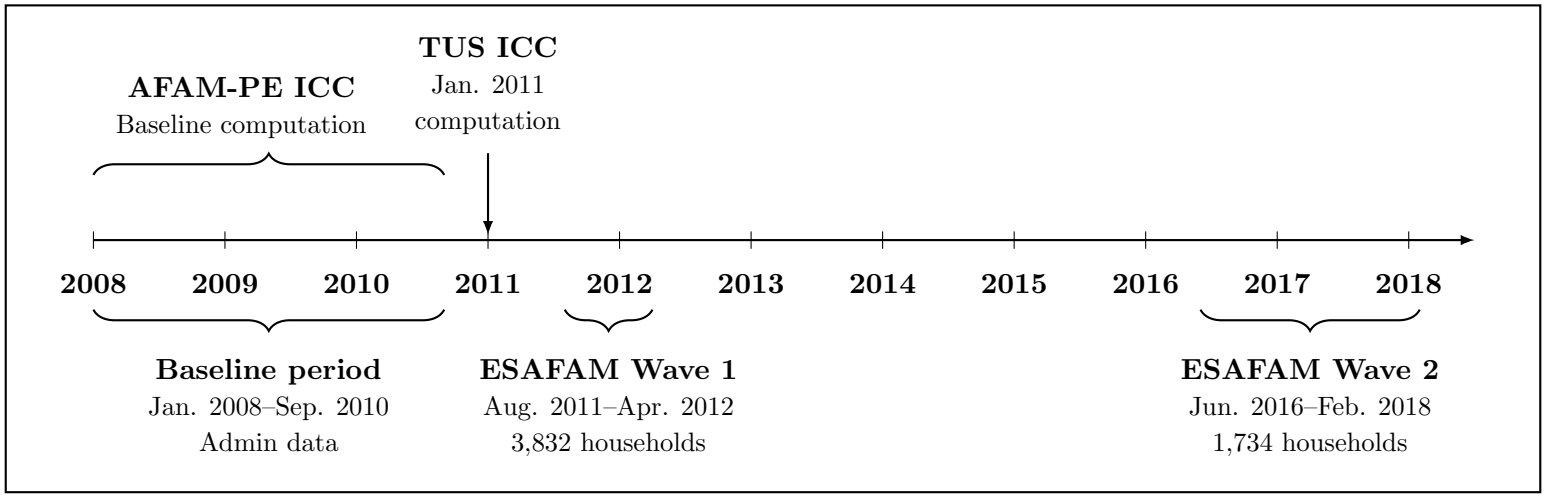
(b) Do you agree that those who are on welfare are lazy?



Source: AmericasBarometer (2012)

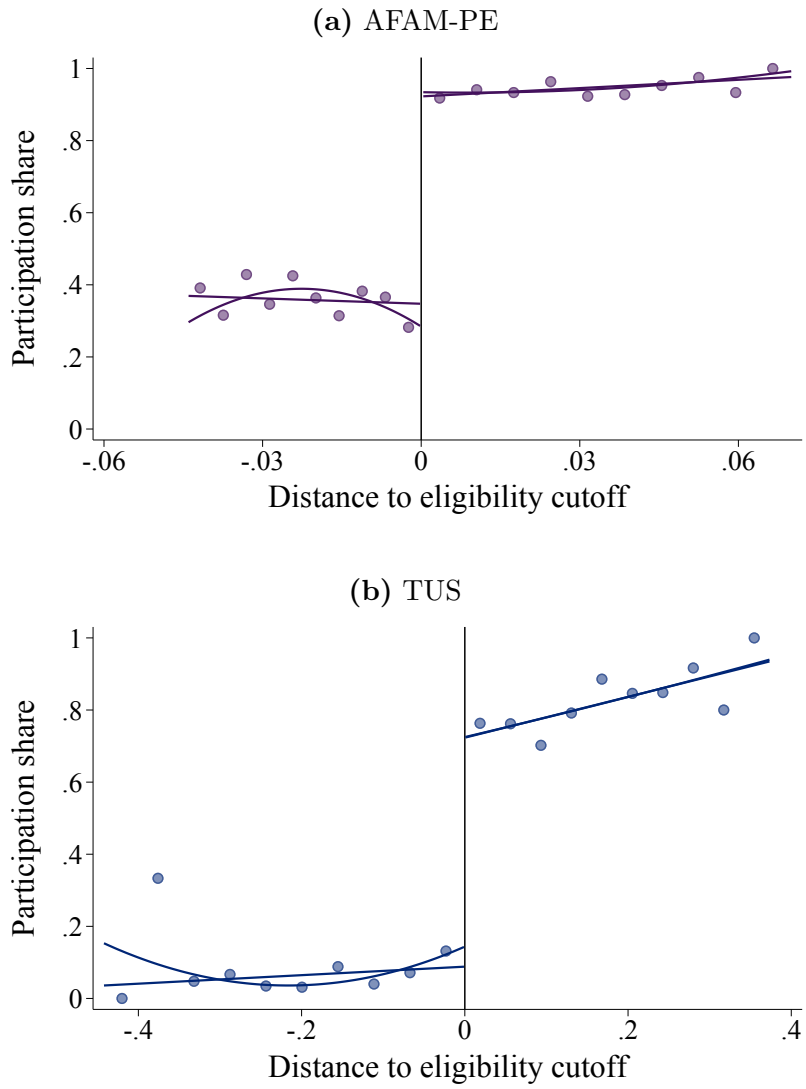
Notes: This figure presents survey evidence on beliefs about poverty and welfare recipients. Panel (a) uses World Values Survey (WVS) results reported by Zuasnabar (2018) for Uruguay in 1996, 2006, and 2011. It reports the share of respondents who answer the question "*Why are there people in need in Uruguay?*" by selecting "*Because they are lazy and lack willpower.*" Panel (b) reports the country mean of agreement with the statement "*Some people say that those who receive social assistance from government programs are lazy*," using the 2012 AmericasBarometer (LAPOP) comparison for all available countries. Country means are rescaled from the original 1–7 response scale to 0–100.

Figure A3:
Study Timeline



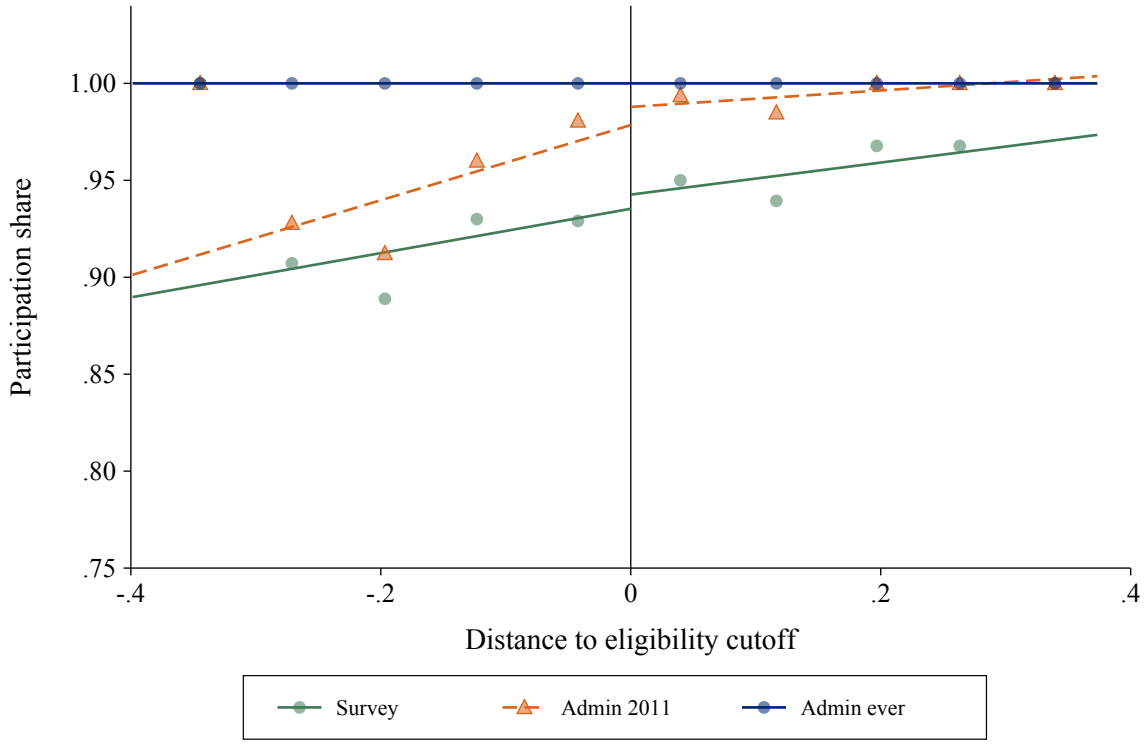
Notes: This figure illustrates the timeline of the study. The baseline period runs from January 2008 to September 2010 and provides administrative baseline data for all households, particularly used to compute the AFAM-PE eligibility index. The TUS eligibility index corresponds to a January 2011 re-certification, when ICC weights were recalculated and most TUS households were reassessed under the updated score. The ESAFAM survey was conducted in two waves: Wave 1 from August 2011 to April 2012 (n = 3,832 households) and Wave 2 from June 2016 to February 2018 (n = 1,734 households). Stigma-related outcomes 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 Discontinuities (Ten Bins on Each Side)



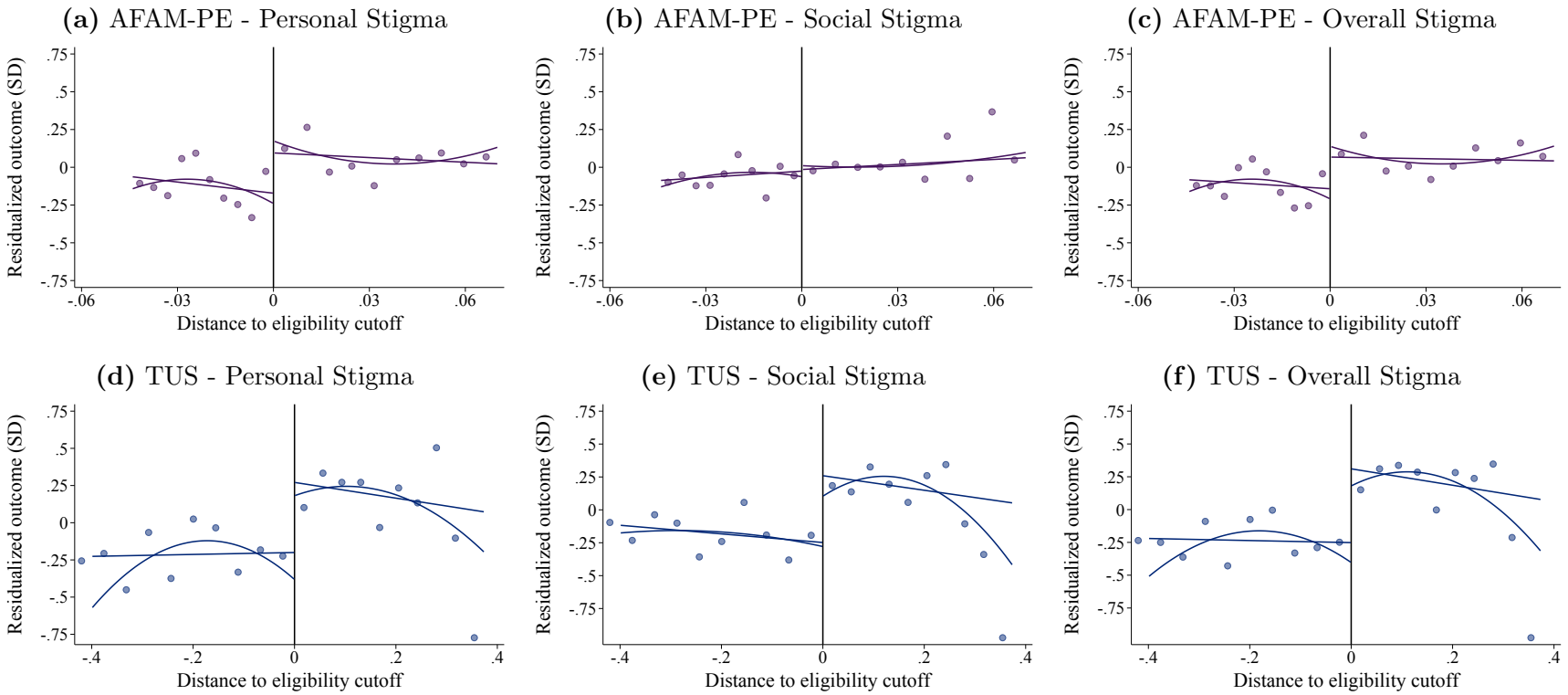
Notes: This figure reproduces the first stage plots in Figure 1 using ten bins on each side of the cutoff. Dots report mean participation within score bins in the common stigma estimation sample. The running variable is the normalized baseline ICC score, with eligibility beginning at zero. Solid lines show fitted linear and quadratic trends on each side of the cutoff. Vertical lines indicate eligibility cutoffs.

Figure A5:
AFAM-PE Receipt at the TUS Eligibility Cutoff in 2011



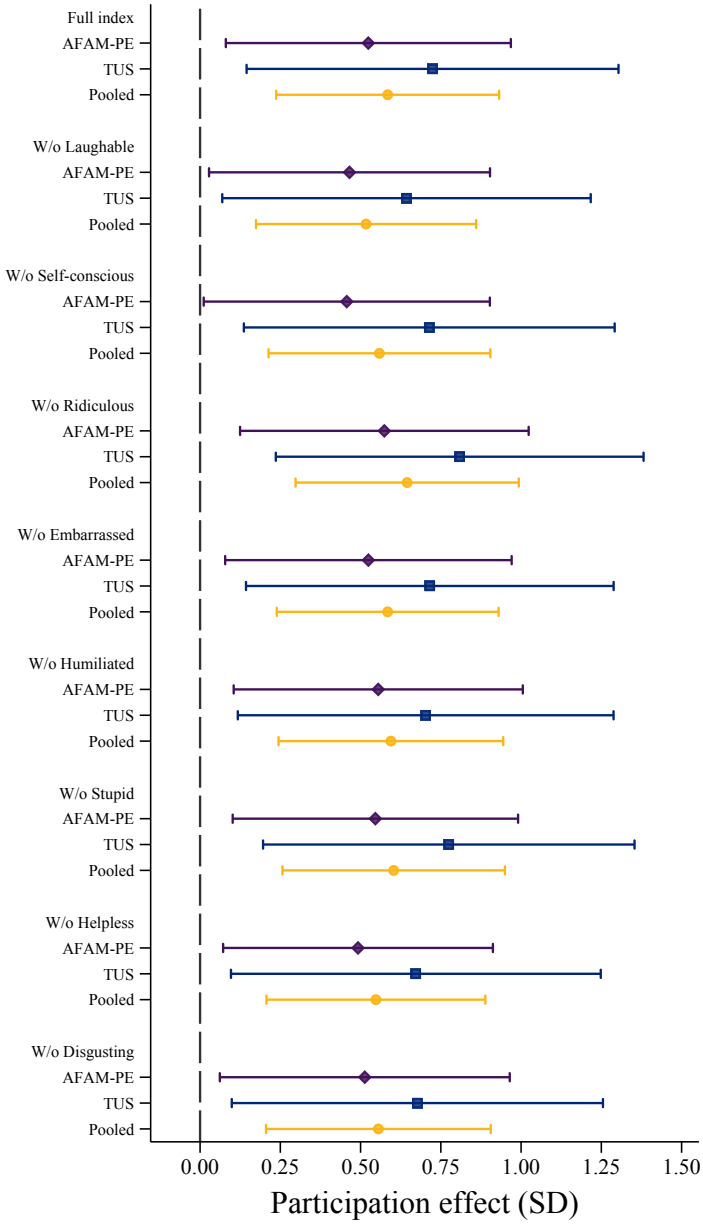
Notes: This figure plots AFAM-PE receipt against the normalized TUS eligibility score for respondents interviewed in 2011. Green circles report survey-based receipt, orange triangles report administrative receipt in the interview month, and blue circles report receipt at any point in the available administrative records. Solid green and blue lines and dashed orange lines show fitted linear trends on each side of the cutoff. Estimated discontinuities are 0.007 ($p = 0.789$) for survey-based receipt and 0.009 ($p = 0.545$) for interview-month administrative receipt. Standard errors clustered by ICC score. Ever receipt equals one throughout the sample. The vertical line indicates the eligibility cutoff at zero.

Figure A6:
RDD Impact Estimates of AFAM-PE and TUS on Perceived Stigma
(Ten Bins per Side)



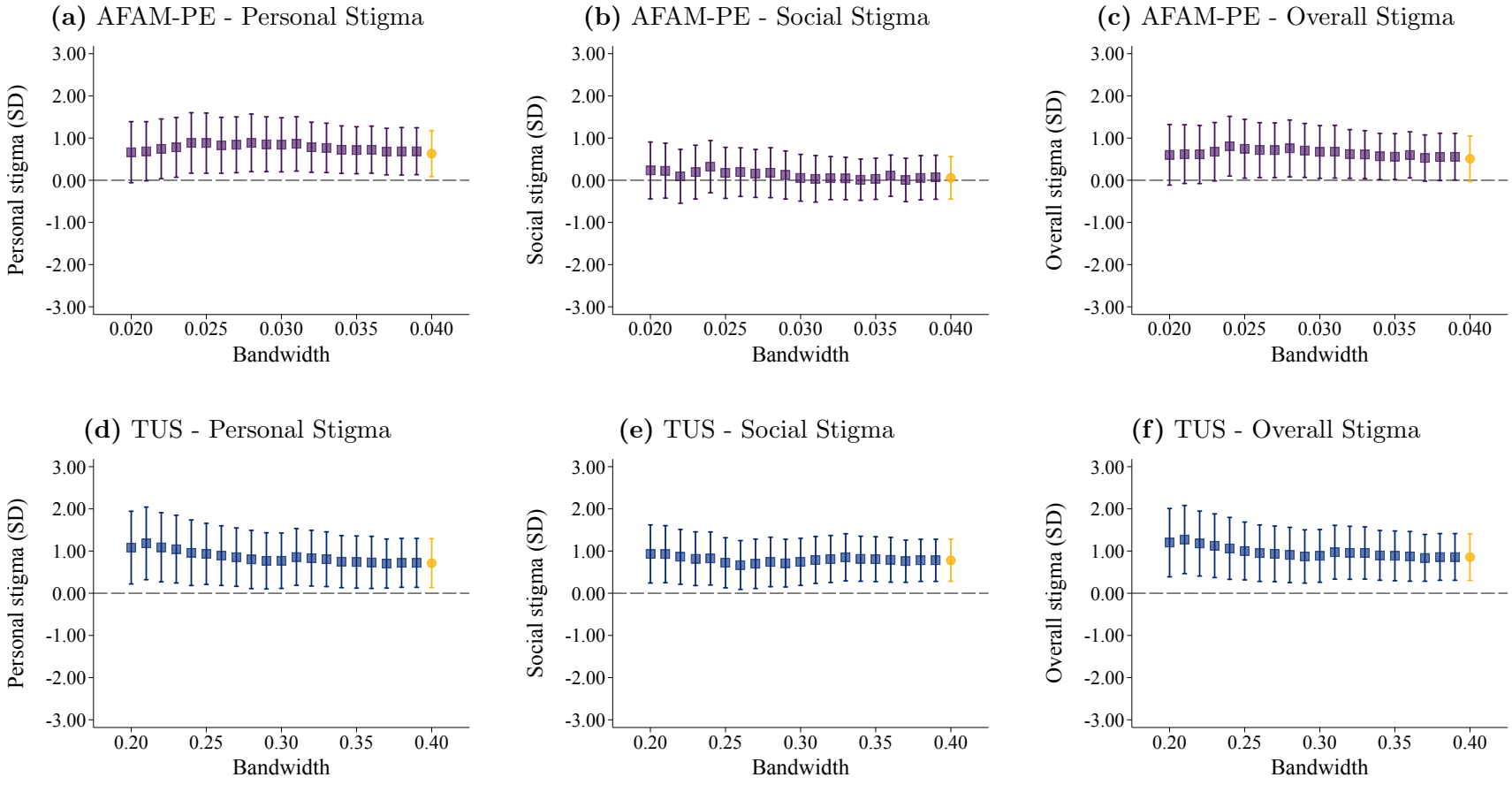
Notes: This figure reproduces the RD plots of personal, social, and overall stigma (z-scores over each program sample) using twenty bins (ten on each side of the cutoff). Outcomes are residualized with respect to sex (women = 1), age, region (Montevideo = 1), and ethnicity (white = 1). Dots report mean residualized outcomes within score bins. Solid lines show fitted linear and quadratic trends on each side of the cutoff. Estimation windows are [−0.047, 0.073] for AFAM-PE and [−0.400, 0.400] for TUS. Vertical lines indicate eligibility cutoffs at zero. Table 2 reports the corresponding regression estimates.

Figure A7:
Robustness Check: Leaving Out Individual Personal Stigma Items



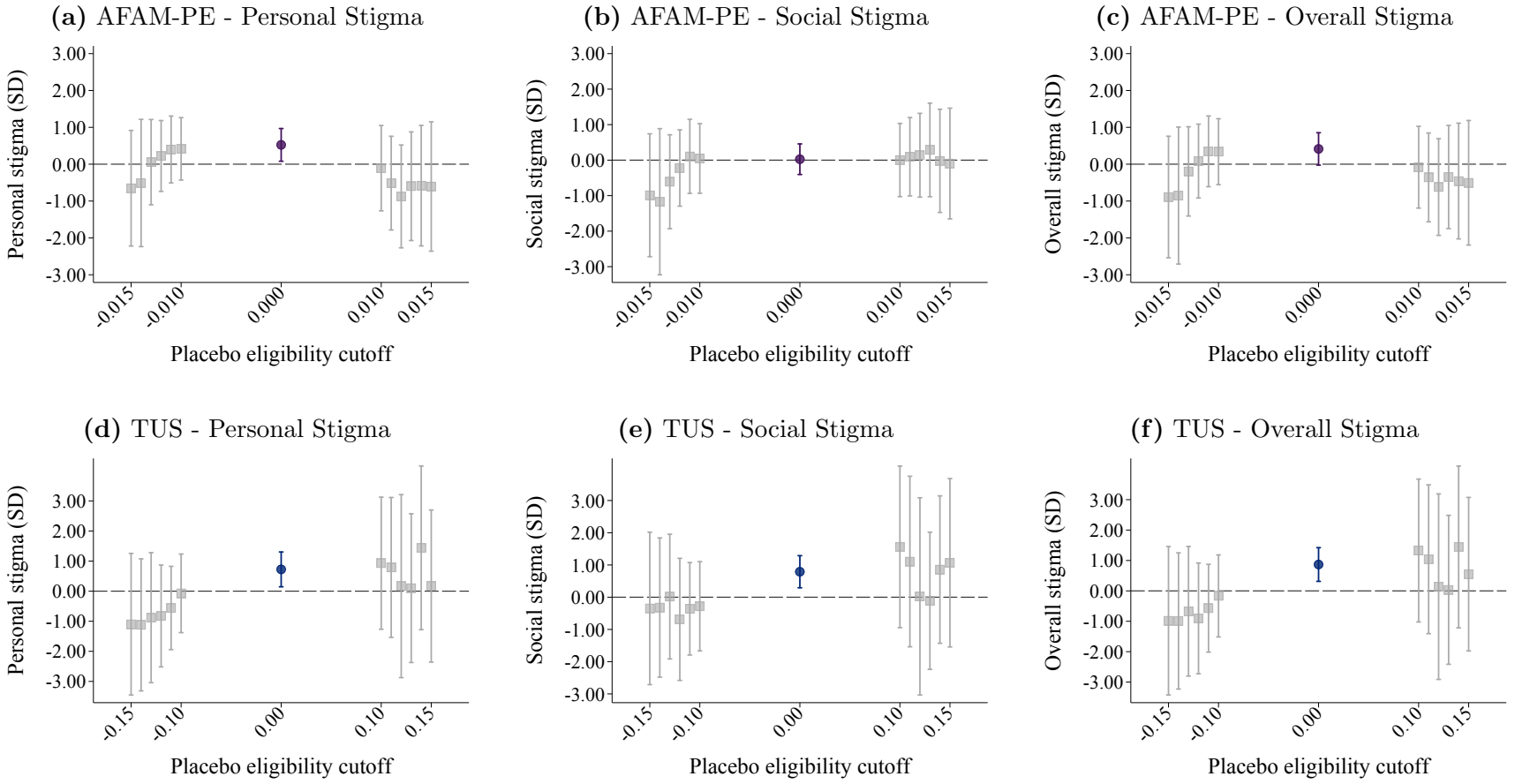
Notes: This figure reports linear fuzzy regression discontinuity estimates of the effects of welfare participation on personal stigma for AFAM-PE, TUS, and the pooled sample. The first block uses the full eight item personal stigma index. Each subsequent block excludes the named item and standardizes the remaining seven item sum over the same pooled estimation sample. Controls include sex (women = 1), age, region (Montevideo = 1), and ethnicity (white = 1). Standard errors clustered by ICC score. Bars show 95% confidence intervals.

Figure A8:
Robustness Check: Alternative Narrower Bandwidths



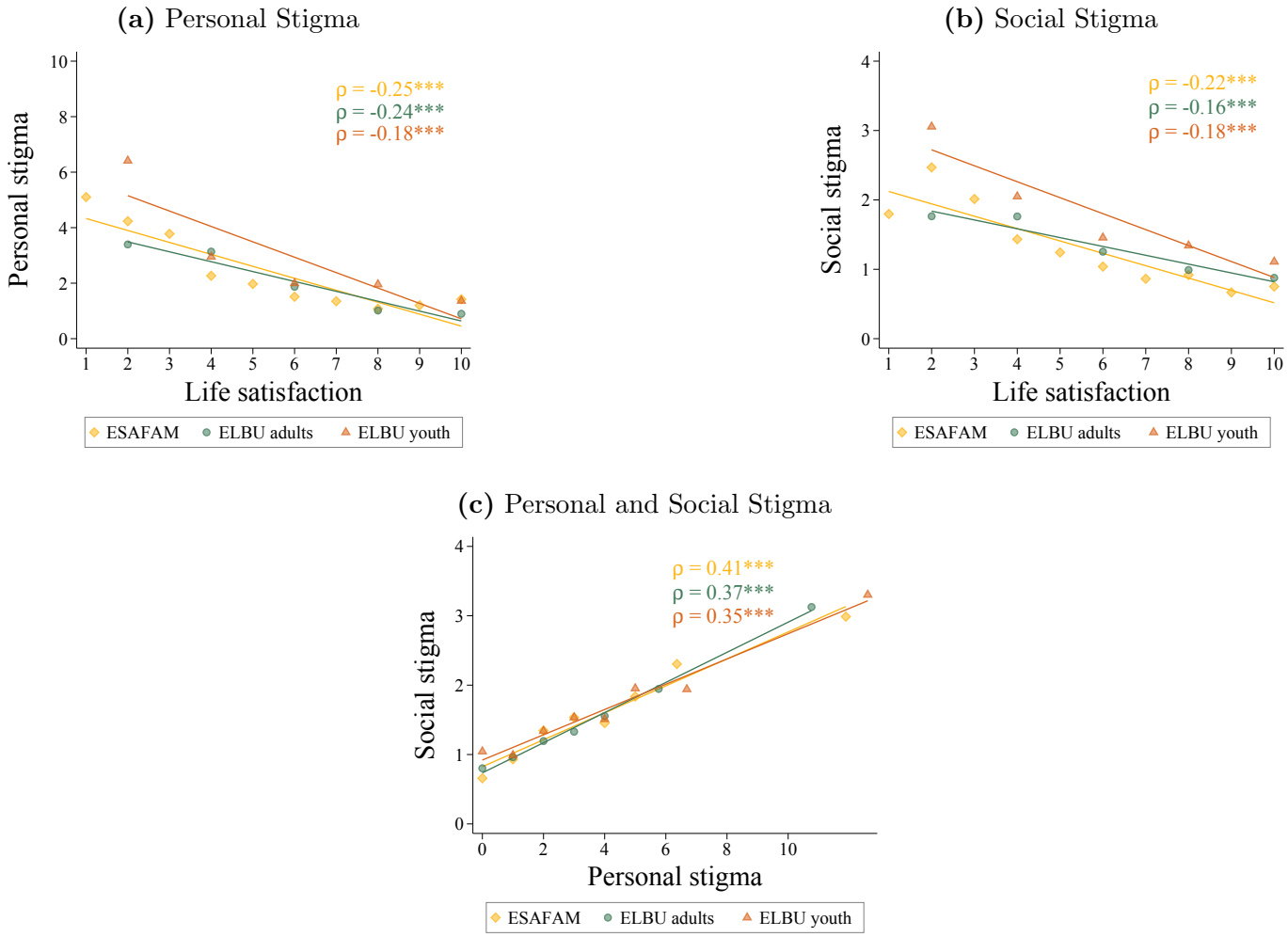
Notes: This figure reports linear fuzzy regression discontinuity estimates of the effects of welfare participation on personal, social, and overall stigma across alternative bandwidths. The horizontal axis shows symmetric bandwidth h, using observations satisfying $0 < |ICC^*| < h$. Bandwidths range from 0.020 to 0.040 in increments of 0.001 for AFAM-PE and from 0.20 to 0.40 in increments of 0.01 for TUS. Gold round markers identify the bandwidth plotted from the main estimates. Estimates follow Equation (3). Outcomes are expressed in standard deviation units. Controls include sex (women = 1), age, region (Montevideo = 1), and ethnicity (white = 1). Standard errors clustered by ICC score. Bars show 95% confidence intervals.

Figure A9:
Robustness Check: Placebo Estimates



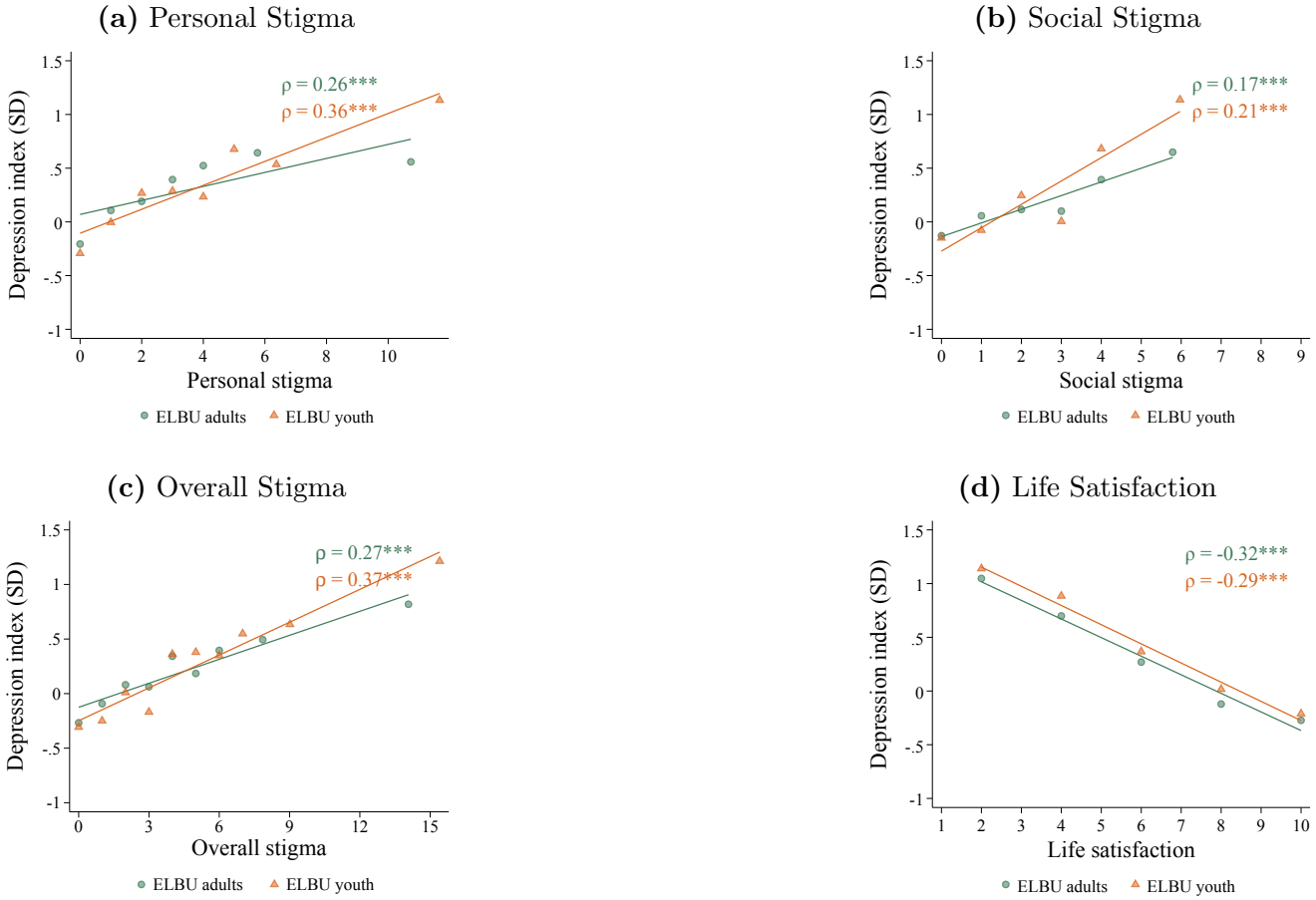
Notes: This figure reports linear fuzzy regression discontinuity estimates for personal, social, and overall stigma at placebo eligibility cutoffs. Placebo cutoffs are $\pm 0.010, \pm 0.011, \ldots, \pm 0.015$ for AFAM-PE and $\pm 0.10, \pm 0.11, \ldots, \pm 0.15$ for TUS. Gray squared markers indicate placebo estimates, while the estimate at the actual cutoff of zero is shown in the program's color with a round marker. Estimates follow Equation (3). Outcomes are expressed in standard deviation units. Controls include sex (women = 1), age, region (Montevideo = 1), and ethnicity (white = 1). Standard errors clustered by ICC score. Bars show 95% confidence intervals.

Figure A10:
Correlation Between Stigma and Subjective Wellbeing Variables



Notes: This figure reports binned relationships between personal stigma, social stigma, and life satisfaction in the pooled AFAM-PE and TUS sample. Dots report bin means and solid lines show fitted linear trends. Stigma indices are simple sums of their corresponding items. Correlation coefficients are reported in each panel. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Figure A11:
Correlation Between Stigma Variables, Life Satisfaction, and Depressive Symptoms in ELBU



Notes: This figure reports binned relationships between stigma, life satisfaction, and depressive symptoms in ELBU. Green circles represent adults and orange triangles represent youth; solid lines show linear fits to the bin means. Stigma indices are simple sums of their items, with overall stigma combining personal and social stigma. The adult depression index sums three binary items while the youth index uses the 13-item Short Mood and Feelings Questionnaire. Both are standardized within their respective samples, with higher values indicating more depressive symptoms. Correlation coefficients use individual observations. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A1:
Attrition Between Survey Waves

	AFAM-PE				TUS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Attrition from W1 to W2								
Attrition (=1)	0.052	0.096**	0.009	0.025	-0.118**	-0.119**	0.167**	0.156*
	(0.041)	(0.042)	(0.061)	(0.060)	(0.060)	(0.059)	(0.085)	(0.085)
Panel B: Baseline household characteristics of attriters								
Adult women (share)	-0.010	-0.011	-0.009	-0.010	0.022	0.022	0.021	0.021
	(0.011)	(0.011)	(0.011)	(0.011)	(0.015)	(0.015)	(0.015)	(0.015)
Mean adult age (years)	-0.403	-0.536	-0.339	-0.473	0.787	0.805	0.628	0.635
	(0.364)	(0.365)	(0.365)	(0.367)	(0.512)	(0.514)	(0.515)	(0.516)
Mean adult education (years)	0.031	0.039	0.048	0.053	0.056	0.040	0.012	-0.003
	(0.132)	(0.132)	(0.133)	(0.133)	(0.187)	(0.185)	(0.192)	(0.191)
Montevideo (=1)	0.067***	0.067***	0.071***	0.070***	0.045	0.042	0.061**	0.058**
	(0.020)	(0.020)	(0.020)	(0.020)	(0.028)	(0.028)	(0.028)	(0.028)
Household members (#)	-0.089	-0.110**	-0.102*	-0.122**	-0.267**	-0.262**	-0.155	-0.146
	(0.055)	(0.055)	(0.055)	(0.055)	(0.110)	(0.111)	(0.109)	(0.110)
Children age $\leq$ 6 (#)	-0.009	-0.015	-0.016	-0.020	0.022	0.028	0.069	0.074
	(0.028)	(0.026)	(0.028)	(0.026)	(0.053)	(0.051)	(0.053)	(0.051)
Children age $\leq$ 12 (#)	-0.051	-0.052	-0.058*	-0.057*	-0.047	-0.043	0.032	0.039
	(0.033)	(0.033)	(0.034)	(0.033)	(0.070)	(0.070)	(0.070)	(0.070)
Children age $\leq$ 18 (#)	-0.059	-0.059	-0.064*	-0.064*	-0.115	-0.119	-0.028	-0.027
	(0.036)	(0.037)	(0.037)	(0.037)	(0.087)	(0.087)	(0.087)	(0.087)
Children < 14 attending school (share)	-0.033*	-0.028	-0.034*	-0.029	-0.017	-0.021	-0.012	-0.014
	(0.019)	(0.019)	(0.020)	(0.020)	(0.027)	(0.025)	(0.028)	(0.025)
Owns home (=1)	-0.025	-0.014	-0.025	-0.014	0.042	0.041	0.042	0.041
	(0.020)	(0.019)	(0.020)	(0.019)	(0.026)	(0.026)	(0.027)	(0.026)
Obs.	2,408	2,408	2,408	2,408	1,106	1,106	1,106	1,106
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Polynomial	1	1	2	2	1	1	2	2

Notes: This table reports regression discontinuity tests of attrition between the two ESAFAM survey waves. Panel A reports discontinuities in household attrition at the eligibility cutoffs. Panel B compares baseline characteristics of attriting and retained households. Columns (1)–(4) report AFAM-PE estimates, while Columns (5)–(8) report TUS estimates. Controls include baseline sex (women = 1), age, and region (Montevideo = 1), and second wave ethnicity (white = 1), which is unavailable at baseline. The corresponding control is omitted when it is the outcome. The F-test in Panel B tests the joint significance of the attrition coefficients across the listed characteristics. Standard errors clustered by ICC score in parentheses. P-values are reported in square brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A2:
Baseline Balance Tests

	AFAM-PE				TUS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Household characteristics								
Adult women (%)	-0.029	-0.031	0.005	0.002	0.033	0.032	0.055	0.048
	(0.035)	(0.037)	(0.052)	(0.052)	(0.038)	(0.038)	(0.055)	(0.056)
Mean adult age (years)	-1.352	-1.226	-1.698	-1.050	-1.495	-1.576	0.150	0.090
	(1.165)	(1.232)	(1.610)	(1.650)	(1.408)	(1.416)	(2.144)	(2.143)
Mean adult education (years)	-0.588	-0.584	-0.923	-0.924	0.422	0.548	0.751	0.821
	(0.440)	(0.459)	(0.688)	(0.693)	(0.478)	(0.471)	(0.678)	(0.669)
Montevideo (=1)	-0.378***	-0.380***	0.074	0.077	0.052	0.055	0.312***	0.309***
	(0.080)	(0.080)	(0.122)	(0.123)	(0.073)	(0.073)	(0.106)	(0.107)
Household members (#)	0.049	0.093	0.061	0.095	-0.079	-0.014	-0.022	0.121
	(0.193)	(0.199)	(0.296)	(0.298)	(0.279)	(0.280)	(0.387)	(0.387)
Children age $\leq$ 6 (#)	0.086	-0.007	0.189	0.104	-0.037	-0.083	0.153	0.167
	(0.089)	(0.084)	(0.131)	(0.121)	(0.146)	(0.145)	(0.198)	(0.195)
Children age $\leq$ 12 (#)	0.058	-0.028	0.201	0.140	0.243	0.261	0.239	0.338
	(0.110)	(0.108)	(0.160)	(0.155)	(0.185)	(0.186)	(0.266)	(0.268)
Children age $\leq$ 18 (#)	-0.050	-0.046	0.025	0.018	0.171	0.258	0.229	0.357
	(0.119)	(0.127)	(0.178)	(0.183)	(0.236)	(0.231)	(0.336)	(0.331)
Children $<$ 14 attending school (%)	0.012*	0.007	-0.004	-0.003	0.016	0.017	0.013	0.015
	(0.007)	(0.005)	(0.003)	(0.005)	(0.016)	(0.017)	(0.014)	(0.016)
Owns home (=1)	0.092	0.052	-0.003	0.042	0.016	0.029	0.124	0.138
	(0.069)	(0.066)	(0.099)	(0.093)	(0.071)	(0.071)	(0.111)	(0.109)
Joint F-test	3.28	3.29	0.79	0.53	0.89	1.00	1.36	1.33
	[0.000]	[0.000]	[0.639]	[0.872]	[0.539]	[0.445]	[0.195]	[0.211]
Panel B: Respondent characteristics								
Woman (=1)	0.112**	0.077	0.191**	0.166**	-0.047	-0.054	-0.020	-0.019
	(0.052)	(0.052)	(0.078)	(0.078)	(0.037)	(0.037)	(0.054)	(0.054)
Age (years)	-5.519***	-4.902***	-4.478**	-2.846	-2.345	-2.704	-1.009	-1.584
	(1.542)	(1.576)	(2.220)	(2.286)	(1.709)	(1.670)	(2.550)	(2.481)
Education (years)	0.377	0.200	0.423	0.309	0.087	0.189	0.842	0.980
	(0.532)	(0.541)	(0.836)	(0.826)	(0.525)	(0.522)	(0.764)	(0.762)
White (=1)	-0.033	-0.021	-0.057	-0.070	0.009	0.012	0.071	0.058
	(0.042)	(0.043)	(0.063)	(0.064)	(0.065)	(0.065)	(0.099)	(0.100)
Joint F-test	4.12	3.84	2.49	2.38	1.16	1.16	0.56	0.64
	[0.003]	[0.004]	[0.042]	[0.051]	[0.326]	[0.327]	[0.692]	[0.632]
Obs.	946	946	946	946	519	519	519	519
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Polynomial	1	1	2	2	1	1	2	2

Notes: This table reports regression discontinuity tests of balance in baseline household and respondent characteristics. Columns (1)–(4) report AFAM-PE estimates, while Columns (5)–(8) report TUS estimates. Controls include baseline sex (women = 1), age, and region (Montevideo = 1), and second wave ethnicity (white = 1), which is unavailable at baseline. The corresponding control is omitted when it is the outcome. Adult characteristics are computed among household members aged 14 or older. Child variables are counts, except school attendance among children under 14, which is a household share. Adult women is also measured as a share. F-tests assess the joint significance of the discontinuities within each panel. Standard errors clustered by ICC score in parentheses. P-values are reported in square brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A3:
Baseline Balance Tests
(Interior Subsample)

	AFAM-PE				TUS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Household characteristics								
Adult women (%)	-0.060	-0.063	-0.041	-0.038	0.043	0.042	0.047	0.046
	(0.045)	(0.045)	(0.070)	(0.070)	(0.043)	(0.043)	(0.064)	(0.064)
Mean adult age (years)	-1.696	-1.232	-1.784	-0.855	-1.328	-1.454	0.381	0.252
	(1.449)	(1.462)	(2.084)	(2.103)	(1.645)	(1.667)	(2.469)	(2.504)
Mean adult education (years)	-0.490	-0.457	-1.233	-1.308	0.557	0.637	0.610	0.694
	(0.551)	(0.559)	(0.918)	(0.930)	(0.563)	(0.559)	(0.787)	(0.776)
Household members (#)	0.026	0.048	0.070	0.127	-0.287	-0.239	0.027	0.078
	(0.224)	(0.228)	(0.359)	(0.369)	(0.328)	(0.332)	(0.456)	(0.452)
Children age ≤ 6 (#)	0.049	-0.010	0.121	0.100	-0.133	-0.154	0.099	0.064
	(0.107)	(0.100)	(0.159)	(0.150)	(0.171)	(0.170)	(0.240)	(0.240)
Children age ≤ 12 (#)	0.038	-0.007	0.191	0.174	0.212	0.236	0.220	0.218
	(0.129)	(0.126)	(0.187)	(0.186)	(0.218)	(0.217)	(0.307)	(0.303)
Children age ≤ 18 (#)	-0.006	-0.006	0.130	0.119	0.196	0.253	0.119	0.175
	(0.145)	(0.148)	(0.225)	(0.230)	(0.272)	(0.270)	(0.374)	(0.369)
Children < 14 attending school (%)	0.014*	0.014*	-0.008	-0.001	0.024	0.024	0.016	0.017
	(0.008)	(0.008)	(0.005)	(0.005)	(0.024)	(0.024)	(0.017)	(0.017)
Owns home (=1)	-0.049	-0.024	-0.089	-0.075	0.020	0.026	0.077	0.088
	(0.083)	(0.080)	(0.122)	(0.118)	(0.086)	(0.085)	(0.131)	(0.128)
Joint F-test	0.96	0.97	0.87	0.52	1.07	1.15	0.33	0.35
	[0.474]	[0.463]	[0.549]	[0.857]	[0.381]	[0.324]	[0.965]	[0.956]
Panel B: Respondent characteristics								
Woman (=1)	0.111*	0.087	0.221***	0.214***	-0.068	-0.073	-0.030	-0.038
	(0.058)	(0.056)	(0.084)	(0.081)	(0.046)	(0.045)	(0.072)	(0.069)
Age (years)	-3.359*	-2.237	-1.291	0.956	-1.471	-1.968	-1.736	-2.128
	(1.779)	(1.738)	(2.584)	(2.499)	(2.050)	(1.982)	(3.127)	(3.002)
Education (years)	0.403	0.332	0.070	-0.143	0.147	0.278	0.869	0.968
	(0.630)	(0.638)	(1.030)	(1.041)	(0.607)	(0.602)	(0.840)	(0.846)
White (=1)	-0.030	-0.035	-0.100	-0.107	0.023	0.032	0.087	0.094
	(0.051)	(0.052)	(0.075)	(0.076)	(0.077)	(0.077)	(0.113)	(0.113)
Joint F-test	1.53	1.45	2.07	2.03	1.02	1.00	0.75	0.75
	[0.191]	[0.216]	[0.084]	[0.090]	[0.398]	[0.410]	[0.555]	[0.560]
Obs.	677	677	677	677	376	376	376	376
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Polynomial	1	1	2	2	1	1	2	2

Notes: This table reports regression discontinuity tests of balance in baseline household and respondent characteristics among respondents residing outside Montevideo. Columns (1)–(4) report AFAM-PE estimates, while Columns (5)–(8) report TUS estimates. Controls include baseline sex (women = 1), age, and region (Montevideo = 1), and second wave ethnicity (white = 1), which is unavailable at baseline. The corresponding control is omitted when it is the outcome. Adult characteristics are computed among household members aged 14 or older. Child variables are counts, except school attendance among children under 14, which is a household share. Adult women is also measured as a share. F-tests assess the joint significance of the discontinuities within each panel. Standard errors clustered by ICC score in parentheses. P-values are reported in square brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A4:
RDD Impact Estimates of Welfare Participation on Perceived Stigma (Sum Scores)

	Personal stigma		Social stigma		Overall stigma	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: AFAM-PE						
$\hat{\beta}_{\text{OLS}}$	0.685*	0.872**	0.028	0.023	0.712	0.894*
	(0.350)	(0.372)	(0.178)	(0.186)	(0.450)	(0.480)
$\hat{\beta}_{\text{IV}}$	1.191*	1.577**	0.047	0.040	1.238	1.618*
	(0.614)	(0.682)	(0.309)	(0.336)	(0.789)	(0.875)
Obs.	1,036	1,036	1,036	1,036	1,036	1,036
Panel B: TUS						
$\hat{\beta}_{\text{OLS}}$	1.381**	1.424**	0.751***	0.781***	2.132***	2.206***
	(0.580)	(0.584)	(0.253)	(0.251)	(0.714)	(0.717)
$\hat{\beta}_{\text{IV}}$	2.122**	2.179**	1.168***	1.208***	3.290***	3.387***
	(0.888)	(0.889)	(0.395)	(0.389)	(1.107)	(1.103)
Obs.	562	562	562	562	562	562
Panel C: Pooled						
$\hat{\beta}_{\text{OLS}}$	0.930***	1.020***	0.313**	0.315**	1.243***	1.336***
	(0.301)	(0.307)	(0.141)	(0.143)	(0.375)	(0.383)
$\hat{\beta}_{\text{IV}}$	1.556***	1.759***	0.528**	0.549**	2.085***	2.308***
	(0.507)	(0.533)	(0.237)	(0.247)	(0.635)	(0.666)
Obs.	1,598	1,598	1,598	1,598	1,598	1,598
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1	1	1	1	1	1

Notes: This table reports linear regression discontinuity estimates of the effects of eligibility for and participation in welfare programs on personal, social, and overall stigma sums. Outcomes sum the eight retained personal stigma items, the three social stigma items, and all eleven items, respectively, without standardization. Panels A and B report AFAM-PE and TUS estimates, respectively; Panel C reports pooled estimates. Rows labeled $\hat{\beta}_{\text{OLS}}$ report reduced form effects of eligibility, while rows labeled $\hat{\beta}_{\text{IV}}$ report fuzzy RDD effects of participation. Columns (2), (4), and (6) include controls for sex (women = 1), age, region (Montevideo = 1), and ethnicity (white = 1). Standard errors clustered by ICC score for program specific estimates in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A5:
Robustness Check: Alternative Aggregation Methods

	Personal stigma		
	Sum score	PCA weights	Factor weights
	(1)	(2)	(3)
Panel A: AFAM-PE			
$\hat{\beta}_{\text{IV}}$	0.524**	0.498**	0.495**
	(0.227)	(0.225)	(0.225)
Obs.	1,036	1,036	1,036
Panel B: TUS			
$\hat{\beta}_{\text{IV}}$	0.724**	0.719**	0.714**
	(0.296)	(0.295)	(0.295)
Obs.	562	562	562
Correlation with sum score	1.000	0.997	0.997
Controls	Yes	Yes	Yes
Polynomial	1	1	1

Notes: This table reports linear fuzzy regression discontinuity estimates of the effects of welfare participation on personal stigma under alternative scoring methods. Columns compare the preferred standardized eight item sum with PCA weighted and factor loading weighted scores. The correlation row compares each score with the preferred sum over the pooled scoring sample. Controls include sex (women = 1), age, region (Montevideo = 1), and ethnicity (white = 1). Standard errors clustered by ICC score in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A6:
Robustness Check: Including Missing Stigma Items

	Personal stigma		Social stigma (m)		Overall stigma (m)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: AFAM-PE						
$\hat{\beta}_{\text{OLS}}$	0.207*	0.278**	0.014	0.020	0.165	0.223*
	(0.112)	(0.119)	(0.111)	(0.117)	(0.112)	(0.120)
$\hat{\beta}_{\text{IV}}$	0.360*	0.506**	0.024	0.036	0.287	0.405*
	(0.195)	(0.218)	(0.194)	(0.212)	(0.196)	(0.219)
Obs.	1,105	1,105	1,105	1,105	1,105	1,105
Panel B: TUS						
$\hat{\beta}_{\text{OLS}}$	0.512***	0.523***	0.543***	0.557***	0.611***	0.625***
	(0.185)	(0.187)	(0.159)	(0.158)	(0.176)	(0.177)
$\hat{\beta}_{\text{IV}}$	0.794***	0.807***	0.857***	0.874***	0.953***	0.970***
	(0.287)	(0.288)	(0.253)	(0.249)	(0.277)	(0.276)
Obs.	600	600	600	600	600	600
Panel C: Pooled						
$\hat{\beta}_{\text{OLS}}$	0.319***	0.354***	0.221**	0.226**	0.334***	0.363***
	(0.096)	(0.098)	(0.089)	(0.090)	(0.093)	(0.095)
$\hat{\beta}_{\text{IV}}$	0.538***	0.616***	0.377**	0.400**	0.565***	0.634***
	(0.163)	(0.172)	(0.150)	(0.157)	(0.158)	(0.166)
Obs.	1,705	1,705	1,705	1,705	1,705	1,705
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1	1	1	1	1	1

Notes: This table reports linear regression discontinuity estimates of the effects of eligibility for and participation in welfare programs on personal, social, and overall stigma when partially missing items are assigned zero. Scales with no answered items remain missing. Rows labeled $\hat{\beta}_{\text{OLS}}$ report reduced form effects of eligibility, while rows labeled $\hat{\beta}_{\text{IV}}$ report fuzzy RDD effects of participation. Columns (2), (4), and (6) include controls for sex (women = 1), age, region (Montevideo = 1), and ethnicity (white = 1). Standard errors clustered by ICC score for program-specific estimates. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A7:
Robustness Check: Estimates for Alternative Narrower Bandwidths

	Personal stigma		Social stigma	
	(1)	(2)	(3)	(4)
Panel A: AFAM-PE				
$\hat{\beta}_{\text{IV}}$ (Cut 0.005 from tails)	0.408**	0.522**	0.012	0.002
	(0.206)	(0.222)	(0.207)	(0.220)
Bandwidth	$[-0.035, 0.065]$	$[-0.035, 0.065]$	$[-0.035, 0.065]$	$[-0.035, 0.065]$
Obs.	965	965	965	965
$\hat{\beta}_{\text{IV}}$ (Cut 0.01 from tails)	0.563**	0.661***	0.093	0.075
	(0.236)	(0.247)	(0.231)	(0.242)
Bandwidth	$[-0.030, 0.060]$	$[-0.030, 0.060]$	$[-0.030, 0.060]$	$[-0.030, 0.060]$
Obs.	916	916	916	916
$\hat{\beta}_{\text{IV}}$ (Cut 0.015 from tails)	0.606**	0.692**	0.162	0.154
	(0.259)	(0.271)	(0.244)	(0.260)
Bandwidth	$[-0.025, 0.055]$	$[-0.025, 0.055]$	$[-0.025, 0.055]$	$[-0.025, 0.055]$
Obs.	840	840	840	840
Panel B: TUS				
$\hat{\beta}_{\text{IV}}$ (Cut 0.05 from tails)	0.730**	0.740**	0.797***	0.807***
	(0.315)	(0.315)	(0.278)	(0.272)
Bandwidth	$[-0.35, 0.35]$	$[-0.35, 0.35]$	$[-0.35, 0.35]$	$[-0.35, 0.35]$
Obs.	546	546	546	546
$\hat{\beta}_{\text{IV}}$ (Cut 0.1 from tails)	0.766**	0.771**	0.753**	0.744***
	(0.334)	(0.335)	(0.294)	(0.285)
Bandwidth	$[-0.30, 0.30]$	$[-0.30, 0.30]$	$[-0.30, 0.30]$	$[-0.30, 0.30]$
Obs.	518	518	518	518
$\hat{\beta}_{\text{IV}}$ (Cut 0.15 from tails)	0.902**	0.933**	0.765**	0.722**
	(0.364)	(0.369)	(0.315)	(0.303)
Bandwidth	$[-0.25, 0.25]$	$[-0.25, 0.25]$	$[-0.25, 0.25]$	$[-0.25, 0.25]$
Obs.	482	482	482	482
Controls	No	Yes	No	Yes
Polynomial	1	1	1	1

Notes: This table reports linear fuzzy regression discontinuity estimates of the effects of welfare participation on personal and social stigma across alternative bandwidths. Panel A reports AFAM-PE estimates after trimming 0.005, 0.010, and 0.015 from each tail of the running variable, while Panel B reports TUS estimates after trimming 0.05, 0.10, and 0.15. Columns (1)–(2) report personal stigma, while Columns (3)–(4) report social stigma. Columns (2) and (4) include controls for sex (women = 1), age, region (Montevideo = 1), and ethnicity (white = 1). Standard errors clustered by ICC score in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A8:
Robustness Check: Donut RDD Estimates

	Personal stigma		Social stigma	
	(1)	(2)	(3)	(4)
Panel A: AFAM-PE				
$\hat{\beta}_{\text{IV}}$	0.540**	0.683***	-0.003	-0.009
	(0.216)	(0.241)	(0.215)	(0.233)
Donut cut	$n \notin [-0.001, 0.001]$	$n \notin [-0.001, 0.001]$	$n \notin [-0.001, 0.001]$	$n \notin [-0.001, 0.001]$
Obs.	1,021	1,021	1,021	1,021
$\hat{\beta}_{\text{IV}}$	0.547**	0.683**	-0.002	-0.017
	(0.237)	(0.265)	(0.236)	(0.256)
Donut cut	$n \notin [-0.0025, 0.0025]$	$n \notin [-0.0025, 0.0025]$	$n \notin [-0.0025, 0.0025]$	$n \notin [-0.0025, 0.0025]$
Obs.	984	984	984	984
$\hat{\beta}_{\text{IV}}$	0.567**	0.710***	-0.063	-0.080
	(0.237)	(0.256)	(0.254)	(0.270)
Donut cut	$n \notin [-0.005, 0.005]$	$n \notin [-0.005, 0.005]$	$n \notin [-0.005, 0.005]$	$n \notin [-0.005, 0.005]$
Obs.	947	947	947	947
Panel B: TUS				
$\hat{\beta}_{\text{IV}}$	0.619**	0.636**	0.807***	0.822***
	(0.294)	(0.295)	(0.261)	(0.258)
Donut cut	$n \notin [-0.01, 0.01]$	$n \notin [-0.01, 0.01]$	$n \notin [-0.01, 0.01]$	$n \notin [-0.01, 0.01]$
Obs.	543	543	543	543
$\hat{\beta}_{\text{IV}}$	0.607*	0.615*	0.899***	0.931***
	(0.341)	(0.339)	(0.294)	(0.290)
Donut cut	$n \notin [-0.025, 0.025]$	$n \notin [-0.025, 0.025]$	$n \notin [-0.025, 0.025]$	$n \notin [-0.025, 0.025]$
Obs.	511	511	511	511
$\hat{\beta}_{\text{IV}}$	0.805**	0.819**	0.909***	0.960***
	(0.375)	(0.378)	(0.333)	(0.335)
Donut cut	$n \notin [-0.05, 0.05]$	$n \notin [-0.05, 0.05]$	$n \notin [-0.05, 0.05]$	$n \notin [-0.05, 0.05]$
Obs.	474	474	474	474
Controls	No	Yes	No	Yes
Polynomial	1	1	1	1

Notes: This table reports linear fuzzy regression discontinuity estimates of the effects of welfare participation on personal and social stigma after excluding observations nearest the eligibility cutoff. Panel A reports AFAM-PE estimates using exclusion windows of 0.001, 0.0025, and 0.005; Panel B reports TUS estimates using windows of 0.01, 0.025, and 0.05. Columns (1)–(2) report personal stigma; Columns (3)–(4) report social stigma. Columns (2) and (4) include controls for sex (women = 1), age, region (Montevideo = 1), and ethnicity (white = 1). Standard errors clustered by ICC score in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A9:
Robustness Check: Local Linear RDD Estimates

	Personal stigma		Social stigma		Overall stigma	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: AFAM-PE						
$\hat{\beta}_{\text{IV}}$ (uniform kernel)	0.684	0.712	0.181	0.144	0.598	0.605
	(0.205)***	(0.213)***	(0.203)	(0.208)	(0.203)***	(0.210)***
	[0.339]**	[0.351]**	[0.312]	[0.320]	[0.331]*	[0.340]*
$\hat{\beta}_{\text{IV}}$ (triangular kernel)	0.677	0.707	0.180	0.144	0.592	0.602
	(0.206)***	(0.214)***	(0.203)	(0.208)	(0.204)***	(0.211)***
	[0.339]**	[0.351]**	[0.311]	[0.320]	[0.331]*	[0.340]*
$\hat{\beta}_{\text{IV}}$ (Epanechnikov kernel)	0.684	0.712	0.181	0.144	0.598	0.605
	(0.205)***	(0.213)***	(0.203)	(0.208)	(0.203)***	(0.210)***
	[0.339]**	[0.351]**	[0.312]	[0.320]	[0.331]*	[0.340]*
Obs.	1,036	1,036	1,036	1,036	1,036	1,036
Panel B: TUS						
$\hat{\beta}_{\text{IV}}$ (uniform kernel)	0.855	0.868	0.692	0.640	0.930	0.919
	(0.309)***	(0.304)***	(0.269)**	(0.264)**	(0.297)***	(0.292)***
	[0.447]*	[0.437]**	[0.383]*	[0.373]*	[0.416]**	[0.406]**
$\hat{\beta}_{\text{IV}}$ (triangular kernel)	0.939	0.978	0.700	0.660	0.998	1.012
	(0.338)***	(0.327)***	(0.285)**	(0.274)**	(0.319)***	(0.307)***
	[0.490]*	[0.470]**	[0.421]*	[0.402]	[0.450]**	[0.430]**
$\hat{\beta}_{\text{IV}}$ (Epanechnikov kernel)	0.942	0.973	0.731	0.684	1.012	1.017
	(0.326)***	(0.318)***	(0.278)***	(0.270)**	(0.311)***	(0.301)***
	[0.474]**	[0.458]**	[0.408]*	[0.392]*	[0.439]**	[0.422]**
Obs.	562	562	562	562	562	562
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1	1	1	1	1	1

Notes: This table reports semi-parametric local linear fuzzy regression discontinuity estimates of the effects of welfare participation on standardized personal, social, and overall stigma using uniform, triangular, and Epanechnikov kernels, following Cattaneo et al. (2018) and Calonico et al. (2014). Columns (2), (4), and (6) include controls for sex (women = 1), age, region (Montevideo = 1), and ethnicity (white = 1). Point estimates are bias corrected. Conventional standard errors for the bias corrected estimates are reported in parentheses; robust bias corrected standard errors are reported in square brackets. Standard errors clustered by ICC score. Stars beside each standard error reflect its corresponding two-sided p-value. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A10:
Robustness Check: Interior Subsample

	Personal stigma		Social stigma		Overall stigma	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: AFAM-PE						
$\hat{\beta}_{\text{OLS}}$	0.243*	0.255*	0.055	0.045	0.209	0.214
	(0.131)	(0.131)	(0.142)	(0.142)	(0.131)	(0.131)
$\hat{\beta}_{\text{IV}}$	0.386*	0.412*	0.088	0.073	0.332	0.346
	(0.209)	(0.212)	(0.225)	(0.230)	(0.209)	(0.212)
Obs.	729	729	729	729	729	729
Panel B: TUS						
$\hat{\beta}_{\text{OLS}}$	0.699***	0.725***	0.368*	0.422**	0.683***	0.724***
	(0.245)	(0.246)	(0.191)	(0.191)	(0.230)	(0.232)
$\hat{\beta}_{\text{IV}}$	0.979***	1.010***	0.540**	0.609**	0.966***	1.017***
	(0.336)	(0.335)	(0.263)	(0.261)	(0.315)	(0.316)
Obs.	407	407	407	407	407	407
Panel C: Pooled						
$\hat{\beta}_{\text{OLS}}$	0.417***	0.428***	0.184*	0.176	0.394***	0.399***
	(0.125)	(0.127)	(0.111)	(0.111)	(0.119)	(0.120)
$\hat{\beta}_{\text{IV}}$	0.634***	0.660***	0.289*	0.280*	0.602***	0.618***
	(0.190)	(0.194)	(0.168)	(0.170)	(0.181)	(0.184)
Obs.	1,136	1,136	1,136	1,136	1,136	1,136
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1	1	1	1	1	1

Notes: This table reports linear regression discontinuity estimates of the effects of eligibility for and participation in welfare programs on personal, social, and overall stigma among respondents residing outside Montevideo at baseline. Outcomes retain the standardization used for the main indices. Rows labeled $\hat{\beta}_{\text{OLS}}$ report reduced form effects of eligibility, while rows labeled $\hat{\beta}_{\text{IV}}$ report fuzzy RDD effects of participation. Columns (2), (4), and (6) include controls for sex (women = 1), age, and ethnicity (white = 1). Standard errors clustered by ICC score for program specific estimates in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A11:
Robustness Check: Inverse Probability Weighting for Attrition

	Personal stigma		Social stigma		Overall stigma	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: AFAM-PE						
$\hat{\beta}_{\text{OLS}}$	0.262**	0.290**	0.022	0.025	0.210*	0.233*
	(0.123)	(0.132)	(0.121)	(0.125)	(0.122)	(0.131)
$\hat{\beta}_{\text{IV}}$	0.472**	0.532**	0.038	0.044	0.379*	0.427*
	(0.223)	(0.246)	(0.218)	(0.228)	(0.220)	(0.242)
Obs.	1,036	1,034	1,036	1,034	1,036	1,034
Panel B: TUS						
$\hat{\beta}_{\text{OLS}}$	0.504**	0.457**	0.527***	0.534***	0.595***	0.562***
	(0.199)	(0.205)	(0.165)	(0.163)	(0.188)	(0.190)
$\hat{\beta}_{\text{IV}}$	0.752**	0.711**	0.801***	0.855***	0.893***	0.883***
	(0.302)	(0.328)	(0.253)	(0.265)	(0.288)	(0.309)
Obs.	562	562	562	562	562	562
Inverse Prob. Weighting	No	Yes	No	Yes	No	Yes
Attrition controls	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial	1	1	1	1	1	1

Notes: This table reports regression discontinuity estimates of the effects of eligibility for and participation in welfare programs on personal, social, and overall stigma (z-scores over the pooled sample), with baseline balance and attrition corrections using inverse probability weighting. Rows labeled $\hat{\beta}_{\text{OLS}}$ report reduced form effects of eligibility, while rows labeled $\hat{\beta}_{\text{IV}}$ report fuzzy RDD effects of participation. Estimates follow Equations (1) and (3), respectively. Controls include sex (women = 1), age, baseline region (Montevideo = 1), ethnicity (white = 1), and baseline household size. Columns (2), (4), and (6) use inverse probability weights for household retention between survey waves. Retention probabilities are estimated separately by program using a household logit with eligibility, the running variable, their interaction, and zero filled baseline region and household size. Missing baseline values are set to zero and missingness indicators are included. Estimation bandwidths are $[-0.040; 0.070]$ for AFAM-PE and $[-0.400; 0.400]$ for TUS. Standard errors clustered by ICC score in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A12:
Robustness Check: Lee Bounds for Attrition

	Personal stigma (1)	Social stigma (2)	Overall stigma (3)
Panel A: AFAM-PE			
$\hat{\beta}_{\text{OLS}}$	0.248**	0.058	0.214*
	(0.115)	(0.113)	(0.116)
Lee bounds	[0.159, 0.516]	[-0.126, 0.353]	[0.076, 0.478]
$\hat{\beta}_{\text{IV}}$	0.451**	0.106	0.389*
	(0.210)	(0.206)	(0.211)
Lee bounds	[0.409, 1.016]	[-0.318, 0.619]	[0.271, 0.978]
Re-interviewed households	1,117	1,117	1,117
Baseline households	2,614	2,614	2,614
Panel B: TUS			
$\hat{\beta}_{\text{OLS}}$	0.496***	0.506***	0.580***
	(0.185)	(0.159)	(0.176)
Lee bounds	[-0.117, 0.778]	[0.046, 0.825]	[0.041, 0.909]
$\hat{\beta}_{\text{IV}}$	0.786***	0.821***	0.927***
	(0.289)	(0.255)	(0.279)
Lee bounds	[-0.043, 1.472]	[0.109, 1.478]	[0.196, 1.633]
Re-interviewed households	600	600	600
Baseline households	1,189	1,189	1,189
Baseline controls	Yes	Yes	Yes
Polynomial	1	1	1

Notes: This table reports linear regression discontinuity estimates of the effects of eligibility for and participation in welfare programs on personal, social, and overall stigma (z-scores over the pooled sample), together with Lee bounds. Rows labeled $\hat{\beta}_{\text{OLS}}$ report reduced form effects of eligibility, while rows labeled $\hat{\beta}_{\text{IV}}$ report fuzzy RDD effects of participation. Missing stigma items are assigned zero for households re-interviewed in Wave 2; outcomes for households not re-interviewed remain missing. Controls include baseline sex (women = 1), age, region (Montevideo = 1), ethnicity (white = 1), and missingness indicators. Eligibility bounds apply to always re-interviewed households; participation bounds apply to always re-interviewed compliers and use discontinuities in retention-by-participation probabilities. Bounds use covariate adjusted moments at baseline sample means and tail trimming of cumulative distributions constrained to be monotone and between zero and one. Retention is assumed to decrease with eligibility for AFAM-PE and increase for TUS. Standard errors clustered by ICC score in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A13:
RDD Impact Estimates on Retained Personal Stigma Items

	Self-conscious (1)	Ridiculous (2)	Embarrassed (3)	Humiliated (4)	Stupid (5)	Helpless (6)	Laughable (7)	Disgusting (8)
Panel A: AFAM-PE								
$\hat{\beta}_{\text{IV}}$	0.537	0.091	0.335	0.189	0.183	0.447	0.539	0.273
	(0.205)***	(0.213)	(0.215)	(0.200)	(0.252)	(0.274)	(0.239)**	(0.202)
	[0.020]**	[0.644]	[0.139]	[0.386]	[0.584]	[0.178]	[0.050]**	[0.277]
	{0.099}*	{0.812}	{0.495}	{0.802}	{0.812}	{0.495}	{0.188}	{0.634}
Obs.	1,036	1,036	1,036	1,036	1,036	1,036	1,036	1,036
Panel B: TUS								
$\hat{\beta}_{\text{IV}}$	0.472	0.054	0.501	0.559	0.152	0.655	0.745	0.608
	(0.292)	(0.283)	(0.297)*	(0.265)**	(0.253)	(0.276)**	(0.270)***	(0.231)***
	[0.139]	[0.842]	[0.109]	[0.040]**	[0.535]	[0.030]**	[0.020]**	[0.030]**
	{0.297}	{0.842}	{0.297}	{0.129}	{0.792}	{0.109}	{0.079}*	{0.089}*
Obs.	562	562	562	562	562	562	562	562
Panel C: Pooled								
$\hat{\beta}_{\text{IV}}$	0.438	0.078	0.375	0.325	0.233	0.504	0.609	0.425
	(0.169)***	(0.163)	(0.172)**	(0.167)*	(0.170)	(0.180)***	(0.174)***	(0.160)***
	[0.030]**	[0.683]	[0.030]**	[0.079]*	[0.198]	[0.010]***	[0.030]**	[0.020]**
	{0.099}*	{0.683}	{0.099}*	{0.149}	{0.317}	{0.069}*	{0.030}**	{0.099}*
Obs.	1,598	1,598	1,598	1,598	1,598	1,598	1,598	1,598
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial	1	1	1	1	1	1	1	1

Notes: This table reports fuzzy regression discontinuity estimates of the effects of welfare participation on the eight retained personal stigma items. Each column reports a separate estimate for a standardized item. Controls include sex (women = 1), age, region (Montevideo = 1), and ethnicity (white = 1). Clustered standard errors in parentheses, resampling based p-values in square brackets, and Romano–Wolf p-values in braces. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A14:
RDD Impact Estimates on Social Stigma Items

	Disrespected (1)	Treated unfairly (2)	Discriminated against (3)
Panel A: AFAM-PE			
$\hat{\beta}_{\text{IV}}$	0.079	-0.118	0.109
	(0.221)	(0.239)	(0.237)
	[0.703]	[0.683]	[0.683]
	{0.950}	{0.950}	{0.950}
Obs.	1,036	1,036	1,036
Panel B: TUS			
$\hat{\beta}_{\text{IV}}$	0.446	0.605	0.589
	(0.272)	(0.274)**	(0.243)**
	[0.129]	[0.030]**	[0.050]**
	{0.129}	{0.059}*	{0.059}*
Obs.	562	562	562
Panel C: Pooled			
$\hat{\beta}_{\text{IV}}$	0.218	0.235	0.298
	(0.167)	(0.167)	(0.162)*
	[0.238]	[0.178]	[0.030]**
	{0.366}	{0.366}	{0.158}
Obs.	1,598	1,598	1,598
Controls	Yes	Yes	Yes
Polynomial	1	1	1

Notes: This table reports fuzzy regression discontinuity estimates of the effects of welfare participation on the three social stigma items. Each column reports a separate estimate for a standardized item. Controls include sex (women = 1), age, region (Montevideo = 1), and ethnicity (white = 1). Clustered standard errors in parentheses, resampling-based p-values in square brackets, and Romano–Wolf p-values in braces. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A15:
RDD Impact Estimates on Excluded Personal Stigma Items

	Blushed	Childish
	(1)	(2)
Panel A: AFAM-PE		
$\hat{\beta}_{\text{IV}}$	-0.089	-0.156
	(0.234)	(0.265)
	[0.733]	[0.515]
	{0.743}	{0.743}
Obs.	1,035	1,035
Panel B: TUS		
$\hat{\beta}_{\text{IV}}$	0.002	0.311
	(0.266)	(0.239)
	[0.990]	[0.228]
	{0.990}	{0.337}
Obs.	560	560
Panel C: Pooled		
$\hat{\beta}_{\text{IV}}$	-0.022	0.112
	(0.169)	(0.167)
	[0.950]	[0.465]
	{0.950}	{0.752}
Obs.	1,595	1,595
Controls	Yes	Yes
Polynomial	1	1

Notes: This table reports fuzzy regression discontinuity estimates of the effects of welfare participation on the items excluded from the preferred personal stigma index. Each column reports a separate estimate for a standardized item. Controls include sex (women = 1), age, region (Montevideo = 1), and ethnicity (white = 1). Clustered standard errors in parentheses, resampling-based p-values in square brackets, and Romano–Wolf p-values in braces. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A16:
RDD Impact Estimates of Welfare Participation on Labor Market Outcomes among Survey Respondents

	Employed		Informal		Hours		Search	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\hat{\beta}_{\text{OLS}}$	-0.001	0.014	-0.041	-0.028	-1.12	-0.65	0.062**	0.048
	(0.037)	(0.037)	(0.037)	(0.037)	(1.87)	(1.86)	(0.029)	(0.029)
$\hat{\beta}_{\text{IV}}$	-0.007	0.017	-0.065	-0.045	-1.82	-1.12	0.099**	0.077
	(0.061)	(0.063)	(0.059)	(0.060)	(3.00)	(3.01)	(0.048)	(0.049)
N-E mean	0.648	0.648	0.494	0.494	35.30	35.30	0.166	0.166
Obs.	3,016	3,016	1,874	1,874	1,874	1,874	3,016	3,016
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Polynomial	1	1	1	1	1	1	1	1

Notes: This table reports pooled regression discontinuity estimates of the effects of welfare eligibility and participation on labor market outcomes among survey respondents. The sample pools AFAM-PE and TUS observations from both survey waves, restricting observations to respondents with valid second-wave stigma measures. Rows labeled $\hat{\beta}_{\text{OLS}}$ report reduced form effects of eligibility, while rows labeled $\hat{\beta}_{\text{IV}}$ report fuzzy RDD effects of participation. All specifications include survey-wave (Wave 2 = 1) and program (TUS = 1) indicators, program specific linear running variable terms, and their interactions with eligibility or participation. Employed indicates whether the individual worked at least one hour in the previous week, excluding household chores. Informal indicates whether the individual does not contribute to social security through their job. Hours measures usual weekly hours worked across all jobs. Search indicates whether the individual searched for a job in the previous week. Informality and hours worked are conditional on employment. N-E mean reports the outcome mean among non-eligible individuals. Columns (2), (4), (6), and (8) include controls for sex (women = 1), age, region (Montevideo = 1), and ethnicity (white = 1). Standard errors clustered at the individual level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A17:
RDD Impact Estimates of TUS Participation on Food Consumption by Stigma Splits

	Produce		Eggs		Candy	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: TUS						
$\hat{\beta}_{\text{IV}}$	0.004	0.006	-0.002	-0.001	-0.002	-0.002
	(0.019)	(0.019)	(0.005)	(0.005)	(0.003)	(0.003)
Panel B: Personal Stigma Split						
$\hat{\beta}_{\text{IV}}$	0.004	0.011	0.000	0.001	-0.000	-0.001
	(0.026)	(0.026)	(0.007)	(0.007)	(0.006)	(0.006)
$\hat{\beta}_{\text{IV}} + \hat{\chi}$	0.011	0.009	-0.007	-0.007	-0.002	-0.001
	(0.027)	(0.028)	(0.008)	(0.008)	(0.003)	(0.003)
Diff. F-Test	0.043	0.003	0.458	0.537	0.028	0.007
Panel C: Social Stigma Split						
$\hat{\beta}_{\text{IV}}$	-0.003	-0.000	-0.000	0.001	0.005	0.005
	(0.027)	(0.027)	(0.008)	(0.008)	(0.006)	(0.006)
$\hat{\beta}_{\text{IV}} + \hat{\chi}$	0.007	0.008	-0.005	-0.004	-0.008**	-0.008**
	(0.028)	(0.027)	(0.006)	(0.006)	(0.004)	(0.004)
Diff. F-Test	0.069	0.047	0.258	0.255	3.245*	3.250*
N-E mean	0.164	0.164	0.024	0.024	0.009	0.009
Obs.	537	537	537	537	537	537
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1	1	1	1	1	1

Notes: This table reports fuzzy regression discontinuity estimates of the effects of TUS participation on the shares of total monthly food expenditures allocated to fruits and vegetables, eggs, and candy. Panel A reports the overall TUS estimates. Panels B and C report estimates by personal and social stigma, respectively. Rows labeled $\hat{\beta}_{\text{IV}}$ report effects below the relevant stigma median, while rows labeled $\hat{\beta}_{\text{IV}} + \hat{\chi}$ report effects at or above the median, where $\hat{\chi}$ is the estimated interaction coefficient. Diff. F-Test reports the F-test of equality between the two effects. Columns (2), (4), and (6) include controls for sex (women = 1), age, region (Montevideo = 1), and ethnicity (white = 1). N-E mean reports the outcome mean among non-eligible households. The estimation bandwidth is $[-0.400; 0.400]$. Standard errors clustered by ICC score in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

B Quotes from Previous 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 main points. One is that these statements underscore the prevalent stigmatizing narratives in Uruguay, highlighting the strong notion of the "undeserving poor". The other one is that such 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."*

Secondly, 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 Model details

Take-up and continuation. Under the budget and time constraints in Section 2, define utility before image costs as

$$V_t(d) = \max_{c,x,h,\ell} \{u(c,\ell) + \alpha v(x) - da_t\} \tag{C.1}$$

A beneficiary continues his enrollment when $V_2(1) - p - \nu s \geq V_2(0)$ An eligible, informed parent initially enrolls if

$$V_1(1) - V_1(0) - \mathbb{E}[p + \nu s] \quad + \beta \mathbb{E}\big[\max\{V_2(1) - p - \nu s, V_2(0)\} - V_2(0)\big] \geq 0 \tag{C.2}$$

The expectation reflects initial beliefs, which may mispredict actual experienced costs (Loewenstein et al., 2003). The first image cost term accounts for receipt in period 1 and the maximum captures the option to leave in period 2. A shift in beliefs toward higher costs raises expected first period costs and weakly lowers the continuation value, reducing take-up. For a continuing recipient, greater visibility lowers second period utility at rate s.

Commitments. Let initial expenditures set a commitment $q = \rho x_1$, where $0 \leq \rho \leq 1$. Reducing later expenditures below q imposes a cost $K(x_2, q)$ on the parent (Chetty and Szeidl, 2007). This cost is zero when $x_2 \geq q$ and increases as expenditures fall below q. These commitments can arise with or without enrollment. Second period valuation before image costs becomes then

$$V_2(d, q) = \max_{c,x,h,\ell} \{u(c,\ell) + \alpha v(x) - da_2 - K(x, q)\} \tag{C.3}$$

subject to the same resource constraints. For each d_1, initial allocations thus solve

$$\begin{aligned} \max_{c_1,x_1,h_1,\ell_1} \Big\{ & u(c_1,\ell_1) + \alpha v(x_1) - d_1\big(a_1 + \mathbb{E}[p + \nu s]\big) \\ & + \beta \mathbb{E}\Big[\max_{d_2 \leq d_1} \{V_2(d_2, \rho x_1) - d_2(p + \nu s)\}\Big] \Big\} \end{aligned} \tag{C.4}$$

under the first period constraints, with $d_2 \in \{0, 1\}$. Continuation needs $p + \nu s \leq V_2(1, q) - V_2(0, q)$. Commitments encourage continuation when they worsen the outside option more than the receiving benefits option.

Wellbeing. Write $U_t(d)$ and $x_t(d)$ for realized utility and expenditure along the path starting with decision d. Relative to never enrolling, we have

$$\begin{aligned} G &= U_1(1) - U_1(0) + \beta[U_2(1) - U_2(0)] \\ C &= v(x_1(1)) - v(x_1(0)) + \beta[v(x_2(1)) - v(x_2(0))] \\ A &= G - \alpha C. \end{aligned} \tag{C.5}$$

Thus benefits to children then can offset a loss in parents' own wellbeing, allowing $A < 0$ with $G \geq 0$.

Now, let $q(d) = \rho x_1(d)$. For a parent who continues, a negative gain and voluntary continuation require, respectively,

$$\begin{aligned} U_1(1) - U_1(0) &< \beta[V_2(0, q(0)) - V_2(1, q(1)) + p + \nu s] \\ p + \nu s &\leq V_2(1, q(1)) - V_2(0, q(1)) \end{aligned} \tag{C.6}$$

The first comparison includes image costs in both periods through U_1 and the continuation payoff, while first period costs are already incurred when the parent decides whether to continue, and commitments can further worsen the outside option. Thus, continued receipt can be preferable to leaving even when the full participation history yields a loss relative to never enrolling for parents.

D Principal Component Analysis (PCA)

This appendix outlines the construction of the personal stigma (shame proneness) index using the survey module proposed by Zavaleta (2007). Given that items are ordered categorical responses, I first estimate their polychoric correlation matrix. I use PCA to assess dimensionality and a one common factor model to evaluate how strongly each item measures the underlying construct. The latter can be written as

$$s_{ij} = \lambda_j F_i + \nu_{ij}$$

where s_{ij} is respondent i's score on item j, F_i is the latent personal stigma factor, λ_j is the item specific factor loading, and ν_{ij} is item specific variation.

I begin with all ten personal stigma items listed in Table 1. The first component of the polychoric PCA explains 48.7% of total variance (Table D1). Although the second eigenvalue is 1.092 and therefore narrowly exceeds the Kaiser rule of one, parallel analysis is more conservative: it retains 1 component because the second observed eigenvalue is below the random data 95th-pctile threshold of 1.111 (Figure D1). The initial scale has a KMO of 0.90, alpha of 0.88, and omega of 0.88.

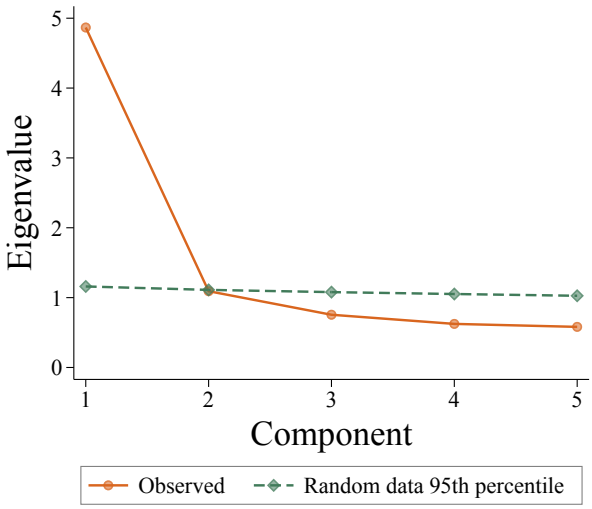
I then estimate a one factor model by iterated principal factors. Table D2 reports factor loadings and communalities. Childish and blushed items are clearly weak indicators, with communalities of 0.168 and 0.191, and are thus excluded. Disgusting is empirically borderline but not a clearly weak item as its initial loading is 0.558 and its communality is 0.311. Given this, it is retained since the loading is comfortably above 0.40, the communality exceeds 0.30, and the underlying survey wording captures feeling repulsive to others, which is directly relevant to personal stigma.

The preferred eight item PCA is then re-estimated to check its validity. Component 1 explains 56.1% of total variance and Component 2 has an eigenvalue below one (Table D3), which is a good sign. All one factor loadings are positive and at least 0.580, while the scale has a KMO of 0.90, alpha of 0.89, and omega of 0.89 (Tables D4 and D5). Table D6 shows descriptive loading stability across programs and eligibility groups.

The preferred index as a result is the standardized sum of the eight retained items. Table A5 compares it with PCA- and factor loading weighted scores. These alternatives correlate 0.997 and 0.997 with the preferred index, respectively, reassuringly. The corresponding AFAM-PE and

TUS estimates remain substantively unchanged as expected. Figure D2 summarizes the raw sum distributions of the preferred personal stigma index and the separately defined social stigma index.

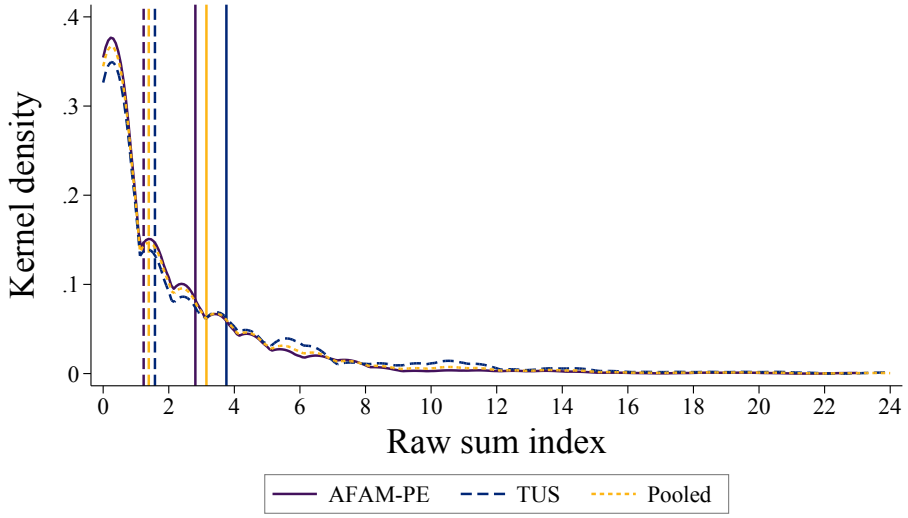
Figure D1:
Parallel Analysis of the Individual Initial Items



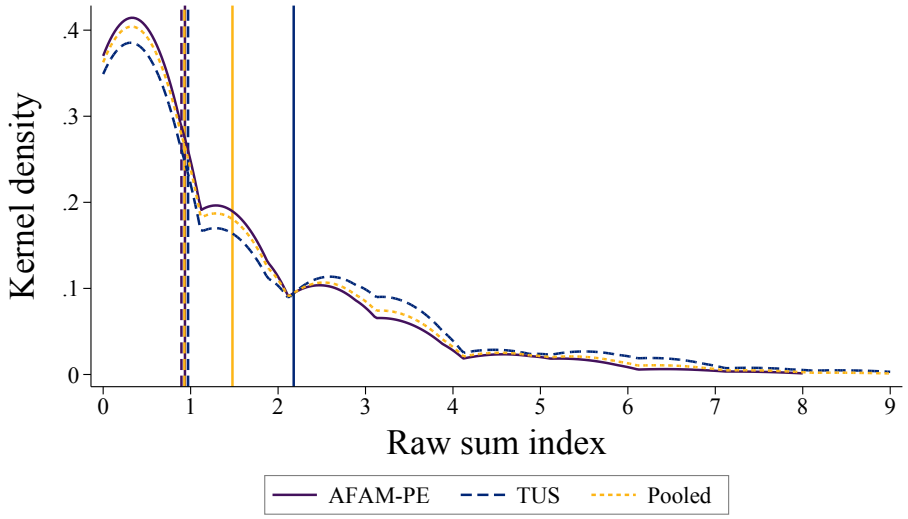
Notes: This figure reports a parallel analysis for the ten item personal stigma scale. The orange line shows observed eigenvalues from the polychoric PCA. The green line shows the corresponding 95th-percentile eigenvalues from 1,000 independent random data draws with the same sample size and number of items. Components are retained when their observed eigenvalues exceed the corresponding random data thresholds.

Figure D2:
Kernel Densities by Stigma Index and Program

(a) Personal stigma



(b) Social stigma



Notes: This figure reports kernel densities of the personal stigma sum in Panel (a) and the social stigma sum in Panel (b). Personal stigma includes the eight retained items; social stigma includes all three mistreatment items. Purple, blue, and gold lines represent the AFAM-PE, TUS, and pooled estimation samples, respectively. Densities use an Epanechnikov kernel with a bandwidth of 0.5. Table 1 reports the corresponding descriptive statistics.

Table D1:
Principal Components (Initial Items)

Component	Eigenvalue (1)	Difference (2)	Proportion (3)	Cumulative (4)
Component 1	4.867	3.775	0.487	0.487
Component 2	1.092	0.337	0.109	0.596
Component 3	0.755	0.131	0.076	0.671

Notes: This table reports the first three components from a PCA of the ten- tem polychoric correlation matrix.

Table D2:
Factor Loadings (Initial Items)

Variable	Factor loading (1)	Communality (2)
Self-conscious	0.691	0.478
Ridiculous	0.780	0.609
Embarrassed	0.766	0.587
Humiliated	0.754	0.568
Laughable	0.669	0.448
Stupid	0.732	0.535
Childish	0.409	0.168
Blushed	0.437	0.191
Helpless	0.676	0.457
Disgusting	0.558	0.311

Notes: This table reports loadings and communalities from a one factor iterated principal factor model estimated on the ten item polychoric correlation matrix. Communality is the share of each item's variance explained by the common factor.

Table D3:
Principal Components (Preferred Items)

Component	Eigenvalue (1)	Difference (2)	Proportion (3)	Cumulative (4)
Component 1	4.487	3.776	0.561	0.561
Component 2	0.711	0.100	0.089	0.650

Notes: This table reports the first two components from a PCA of the preferred eight item polychoric correlation matrix. The first component explains 56.1% of total variance.

Table D4:
One-Factor Loadings (Preferred Items)

Variable	Factor loading (1)	Communality (2)
Self-conscious	0.694	0.481
Ridiculous	0.771	0.595
Embarrassed	0.756	0.572
Humiliated	0.783	0.612
Laughable	0.660	0.435
Stupid	0.705	0.497
Helpless	0.686	0.471
Disgusting	0.580	0.336

Notes: This table reports loadings and communalities from a one factor iterated principal factor model estimated on the preferred eight item polychoric correlation matrix. Communality is the share of each item's variance explained by the common factor.

Table D5:
Additional Index Diagnostics

Scale	N (1)	PC1 (%) (2)	KMO (3)	Alpha (4)	Ordinal alpha (5)	Omega (6)
Initial personal stigma items	1,600	48.7	0.896	0.772	0.877	0.881
Personal stigma	1,603	56.1	0.904	0.791	0.887	0.888

Notes: PCA and KMO statistics use the polychoric correlation matrix. Alpha is conventional Cronbach's alpha, and ordinal alpha and omega use the polychoric correlations.

Table D6:
Factor Loading Stability

Variable	Pooled (1)	AFAM-PE (2)	TUS (3)	Eligible (4)	Ineligible (5)
Self-conscious	0.694	0.703	0.679	0.691	0.688
Ridiculous	0.771	0.732	0.808	0.768	0.782
Embarrassed	0.756	0.748	0.765	0.769	0.717
Humiliated	0.783	0.763	0.811	0.790	0.749
Laughable	0.660	0.642	0.666	0.675	0.599
Stupid	0.705	0.637	0.797	0.694	0.723
Helpless	0.686	0.683	0.705	0.717	0.598
Disgusting	0.580	0.530	0.626	0.522	0.721

Notes: This table reports one factor loadings from separate polychoric factor models for the pooled sample, each program, and eligibility groups.