

Government Transfers and Political Support

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We estimate the impact of a large anti-poverty program – the Uruguayan *PANES* – on political support for the government that implemented it. Using the discontinuity in program assignment based on a pre-treatment score, we find that program beneficiary households are 25 to 31 percentage points more likely to favor the current government (relative to the previous government). Program impacts on political support are larger among poorer households and for those near the center of the political spectrum, as predicted by the probabilistic voting model in political economy. We estimate the annual cost to the government of increasing their political support by 1% on the order of US\$89 million or 1.7% of annual government expenditures.

Keywords: Conditional cash transfers, redistributive politics, voting, regression discontinuity.

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Are voters willing to trade-off some of their ideological attachment in exchange for higher consumption? This is a frequent assumption in leading models of individual voting behavior (Persson and Tabellini, 2002): the extent to which voters are willing to trade-off consumption for political ideology determines politicians' ability to use transfer programs to capture votes. In the classic probabilistic voting model (Lindbeck and Weibull, 1987, Dixit and Londregan, 1996, 1998), competing parties target transfers to marginal - or "swing" – voters, i.e., those closest to the centre of the political spectrum, since a one dollar transfer to this group leads to a greater increase in political support than a transfer to groups with stronger ideological attachments. Given the declining marginal utility of consumption, the model also predicts that a transfer of a given size is also more effective at swaying the political allegiance of poorer voters. These findings may break down for theoretical reasons including intertemporal commitment problems (Verdier and Snyder, 2002), "political machine" dynamics whereby transfers are more effectively targeted to parties' core supporters, or risk averse political parties (Cox and McCubbins, 1984).

Despite the central role that voters' response to government transfers plays in political economy theories, empirical evidence on the impact transfers on individual voting behavior is remarkably scant and rarely based on credible research designs. Identifying the effect of redistributive politics on individual political preference is challenging for several reasons. Most fundamentally, political parties' tactical considerations, like those described above, imply that funds are not randomly allocated across voters. For instance, political patronage strategies could lead parties' core supporters to be favored by redistribution, leading to reverse causality, leading simple OLS regressions of individual political preferences on transfers received to yield upwardly biased estimates of transfer impacts. Yet the opposite bias could arise if incumbents, sensing a re-election threat, increased transfers to voters further away from the party's core. Even in the absence of tactical spending by parties and politicians, omitted variables (e.g. household socioeconomic status) might affect both the receipt of transfers and political preferences, leading to a spurious correlation between the two.

This paper estimates the causal effect of government transfers on political support for the incumbent party using data from Uruguay. To our knowledge, this is the first paper to tackle this question using individual level data and a credible source of econometric identification. In October 2004, against the backdrop of a large economic crisis, a left-wing government took power in Uruguay for the first time and swiftly introduced a large anti-poverty program, called *PANES*. *PANES* was a temporary conditional cash transfer program similar to those recently implemented elsewhere in Latin

America, most famously the *Progresal/Oportunidades* program in Mexico. Household eligibility for the program was determined by a poverty score based on a large number of pre-treatment covariates: only households with a score below a predetermined threshold were eligible for the program. Eighteen months following the start of the program, approximately 3,000 households with poverty scores in the neighborhood of the threshold were surveyed, and asked a series of questions including the respondents' support for the current government. Because assignment to the program was nearly "as good as random assignment", as we argue below, we are able to circumvent the problems of reverse causality, endogenous political selection, and omitted variables highlighted above to reliably estimate the impact of government transfers on political preferences, and thus better understand the trade-off between household consumption and political ideology.

In our main empirical finding, the regression discontinuity analysis demonstrates that *PANES* beneficiaries were between 25 to 31 percentage points more likely than non-beneficiaries to favor the current government (relative to the previous one). The result is largely unchanged across a variety of specifications and with the inclusion of a wide set of household controls. Back-of-the-envelope calculations suggest that securing one extra supporter costs the government US\$1,980 per year. This implies that a government that sought to increase their vote share by 1% would need to spend US\$89 million, or 1.7% of national government budget. Uruguay has highly developed democratic political institutions for a middle-income country, suggesting that some of the findings could also be relevant for wealthy countries.

The findings also provide some of the most definitive empirical evidence to date in support of the leading political economy theories described above, especially in illuminating the trade-off between consumption and political ideology. In particular, as predicted by the probabilistic voting model, we find that the effect of government transfers on political support is significantly larger among poorer households, and among those near the center of the political spectrum, than among other households.

In the most closely related work, Levitt and Snyder (1997) study the effect of spending at the district level on voting behavior in the elections for the US House of Representatives. To circumvent the potentially spurious correlation between spending and voting, they instrument spending in each district with spending in neighboring districts within the same State. They find a positive effect of non-transfer federal spending on the incumbent's vote share, but surprisingly no effect of transfer spending. A possible concern with the instrumental variable strategy in that paper is a violation of the exclusion restriction, for instance, if spending on roads or military bases in nearby districts directly affect voters'

choices. Sole-Olle and Sorribas-Navarro (2008) use the same approach as Levitt and Snyder (1997) – again using aggregate voting data and spending at higher levels of government as an IV for local spending – and estimate impacts of government spending on support for the incumbent in Spain. Green (2006a) uses the discontinuity in assignment to *Progresa* across Mexican communities to estimate the effect of the program on voting behavior. She finds a slightly larger incumbent vote share in treated communities but this pattern is also present before the program, suggesting endogenous political selection of program beneficiaries rather than a causal impact. A related analysis using an observational design and U.S. data is Markus (1988).¹

A related literature explores the implications of voters' political ideology on political parties' transfer choices. Dahlberg and Johansson (2002) find support for the swing voter model using the introduction of discretionary funds in Sweden, while others find evidence of core (infra-marginal) voters being disproportionately targeted for redistribution (Case 2000 on Albania, Schady 2002 on Peru, and Green 2006b on Mexico).

The paper proceeds as follows. Section I presents a stylized probabilistic model of voting behavior. Section II presents details of the *PANES* program and the data. Section III investigates the effect of the transfer program on political support for the government and presents some insights into the channels behind the increase in support, and the final section concludes.

I. THE PROBABILISTIC VOTING MODEL

The standard probabilistic voting model (Lindbeck and Wiebull, 1987; Dixit and Londregan, 1996) is a useful framework for the empirical analysis. Consider a governing party (*A*) that chooses a schedule of transfers to distribute among citizens. Both *A* and the opposition party *B* have a fixed ideological orientation in the medium-run (a common assumption in these models), but the transfers they provide different social groups is a choice variable. For simplicity, we assume that the transfer schedule of the opposition party *B* is fixed at what it was when they were last in power (to parallel our empirical application, as discussed below), and focus on the policy decisions of the incumbent party.

¹ After we wrote this paper, we became aware of Chen (2008a, 2008b). Chen estimates the impact of government transfers on voting in the United States, and estimates the cost of an additional vote is \$7000. Like us he finds that this cost is increasing in household income, but he finds that the incumbent's core supporters are cheaper to buy off, in contrast to our finding. Like Levitt and Snyder (1997), Chen uses aggregated local voting data, rather than the individual level data we prefer, and finds that there is systematic targeting of US government assistance as a function of baseline voting patterns (with Republican areas favored), complicating the interpretation of his econometric results.

Voters differ both in their pre-transfer income (Y) and their underlying ideological affinities, X . Political affinities are normalized so that a voter with affinity X has a preference X for the opposition party over the government; thus voters at $X=0$ are ideologically indifferent between the two parties. Voters also care about final consumption C , namely, the sum of their pre-transfer income Y and transfer income T , where the latter can be positive (subsidies) or negative (taxes).

There are G groups of individuals who can be targeted by government transfers, indexed by $g \in \{1, 2, \dots, G\}$, where group g has N_g members. Groups can be thought of as those with certain observable and targetable socio-demographic characteristics (e.g., the elderly poor living in the capital city). Individuals within each group are allowed to have heterogeneous political affinities X . The cumulative distribution function of political affinities for group g is denoted F_g , and the density function is f_g . Individuals are indexed by i .

The consumption utility for individuals in group g when the governing party A is in power is denoted $U_g(C_{Ag})$, with a standard concave utility function, $U_g' > 0$ and $U_g'' < 0$ for all g . C_{Ag} is the sum of pre-transfer income and the transfer chosen for group g . We allow transfer income to potentially enter in with a different effect than pre-transfer income through the coefficient $\theta \geq 0$: $C_{Ag} = Y_g + \theta T_{Ag}$; we test whether $\theta=0$ in the empirical analysis below. Analogously, individual utility with the opposition in power is $U_g(C_{Bg})$. Taking into account both final consumption and political affinities, voter i in group g has a political preference² for the governing party iff:

$$(1) \quad X_{ig} \leq U_g(Y_g + \theta T_{Ag}) - U_g(Y_g + \theta T_{Bg}) \equiv X_g^*$$

X_g^* is the threshold political affinity below which individuals in group g prefer the ruling party. The total number of voters in group g who support the government, V_{Ag} , is thus a function of the distribution of these underlying political affinities:

$$(2) \quad V_{Ag} = N_g F_g(X_g^*)$$

The total number of government supporters across all social groups is denoted $V_A = \sum_g V_{Ag}$.

² We follow most of the political economy literature in assuming that voters sincerely express their political preferences in surveys and at the ballot box. With infinitesimal voters, non-truth telling would also be an equilibrium best response but it greatly complicates the analysis.

Now consider the marginal effect of a larger transfer to group g on their political support for the party in power (A), which has a direct empirical analogue in our empirical analysis:

$$(3) \quad \partial V_{Ag} / \partial T_{Ag} = f_g(X_g^*) U_g'(C_{Ag}) \theta N_g$$

The f_g term implies that larger transfers translate into more votes when there is a greater density of voters near the threshold between voting for the government or the opposition. To illustrate, if the transfer level is already set at so high a level that nearly all group members already support the government, then a further increase will not yield many additional votes. Similarly, if the transfer is very low (or negative, i.e., a large tax) and few group members support the government, then a small transfer increase moves few individuals close to political indifference. Transfers will thus be most effectively targeted at groups with many “swing voters”, those groups close to the political center for whom small consumption gains can make a big difference in counteracting political affinities. We empirically test this implication below by comparing the impact of a government transfer across social groups with different predicted political affiliations.

The marginal utility U_g' term, combined with the assumption of strictly concave utility, implies that a given transfer has a larger impact for poorer individuals, those at lower levels of pre-transfer income. This insight might partially explain why political parties in most countries campaign for some redistribution to the poor independent of their ideological orientation. This theoretical implication is also tested below by examining the interaction between pre-program per capita income and transfer receipt.

When individuals’ political preferences are not swayed by government transfers, namely when $\theta=0$, the expression equals zero. A finding that the receipt of a government transfer affects political preferences to any degree would lead us to reject the hypothesis that $\theta=0$, something we also directly test below.

Note that finally that the N_g term implies that more votes can be had by boosting transfers to larger groups. However, this scale effect drops out once the budget balance condition is considered, since it is also more expensive to increase transfers to all members of a larger group.³

³ These models typically use equations (1) to (3) to determine the choice of the optimal transfer schedule in the context of a game between the government and the political opposition. Specifically, the ruling party chooses to set the transfer schedule to maximize its votes V_A subject to budget balance condition, $\sum_g \{N_g T_{Ag}\} = 0$. This generates an intuitive first order condition, in which the government equates the marginal vote gain from increased transfers across all social groups (taking

Model (1) to (3) provides testable implications on voter behavior in response to government transfers.

II. THE PANES PROGRAM IN URUGUAY

Uruguay is a small Latin America country, home to 3.5 million individuals, half of whom live in the capital, Montevideo. The country experienced rapid economic growth in the first decades of the twentieth century, and was among the first countries in the region to complete the demographic transition, implement universal primary education, and establish a generous European-style old age pension system. Uruguay is currently among the most developed Latin American countries according to the UNDP Human Development Index, with strong life expectancy and schooling indicators (Table 1). According to The Economist Intelligence Unit, the country's political system has very low levels of corruption and free and fair electoral process (Table 1).⁴

Economic growth stagnated in the second half of the twentieth century, and the country went through a severe economic crisis at the start of this decade. Between 2001 and 2002 per capita income fell 11.4%, the poverty rate increased from 18.8% to 23.6% while unemployment reached its highest level in twenty years (at 20%), the exchange rate collapsed, and a financial crisis led to bank runs. Currently, PPP-adjusted annual per capita income is just below US\$10,000. The crisis laid bare the weakness of the existing social safety net, which was largely focused on transfers to the elderly population.⁵ Yet constrained in part by a severe fiscal adjustment, the ruling centrist *Colorado* party government (which had been in power since 1999 in coalition with the right-wing *Blanco* party) focused on preserving existing programs rather than adopting new measures, with the exception of a small emergency food plan.

The left-wing *Frente Amplio* (FA) coalition took power after October 2004 elections,⁶ capitalizing on widespread dissatisfaction with the economy and the previous government's

the policy position of the opposition to be fixed for simplicity, although the finding generalizes to the strategic game (see Dixit and Londregan, 1996): $f_g(X_g^*) U_g'(C_{Ag}) \theta = \lambda_A$ for all g . We are unable to explore how closely government transfer policies approximate this equilibrium condition in our application since we only have detailed data on a non-representative subset of the population, namely, the poor households who were surveyed near the *PANES* program eligibility threshold. This data limitation leads us to restrict our empirical focus to these voters' responsiveness to a large government transfer.

⁴ *The Economist* ranks Uruguay as one of only two "Full democracy" countries in Latin America (the other is Costa Rica). *Transparency International* ranks Uruguay second only to Chile in the region in terms of perceived control of corruption.

⁵ In 2002, total expenditure on elderly pensions represented 96% of all government social expenditures and almost 13% of GDP. This is reflected in marked differences in the incidence of poverty across age groups: while nearly half of children under age five lived in poverty that year, the rate for those 65 and older was only 2%.

⁶ The Uruguayan electoral system is presidential with proportional representation in Congress.

management of the crisis. The FA campaigned on a platform that promised extensive redistribution to the poor and structural economic reforms. The new FA government swiftly created a new Ministry for Social Development (*Ministerio de Desarrollo Social, MIDES*) and moved to design and implement the National Social Emergency Plan (*Plan de Atención Nacional a la Emergencia Social: PANES*).

II.a PANES objectives and components

The *PANES* program was designed to be temporary, running from April 2005 to December 2007, and it had two main aims: first, to provide direct assistance to households who had experienced from a rapid deterioration in living standards since the onset of the 2001-2002 crisis; and second, and in light of rising poverty during the 1980s and 1990s, to strengthen the human and social capital of the poor, to enable them to eventually climb out of poverty on their own.

The *PANES* target population consisted of the very poorest households in the country, namely the bottom quintile of the income distribution among those falling below the national poverty line, or approximately 8% of all households (and 12% of the population). In all, 102,353 households eventually became program beneficiaries.

PANES included several distinct components. The major element was a monthly cash transfer (*ingreso ciudadano*, “citizen income”), whose value was set at US\$56 (UY\$1,360 at the 2005 exchange rate of US\$1=UY\$24.43), independent of household size. This is a very large transfer for the target population, amounting to approximately 53% of the pre-program average household self-reported income of US\$105) per month. Households with children or pregnant women were also entitled to a food card (*tarjeta de alimentos*), an in-kind transfer that operated through an electronic debit card, whose monthly value varied between US\$13 and US\$33. Seventy percent of *PANES* beneficiaries also received a food card. Additional but smaller components included public works employment opportunities, job training, and health care subsidies; more details on *PANES* are in the appendix.

II.b PANES eligibility, enrollment and baseline data

Enrollment of participants occurred in stages. All low income households were invited to apply and the government made a large outreach effort, sending enumerators to poor communities with the intent of publicizing the program and prompting potential beneficiaries to apply. Eventually, 188,671 applicant households were visited by Ministry of Social Development personnel and administered a baseline

survey, providing detailed information on household characteristics, housing, income, work, and schooling.

To determine assignment to *PANES*, the government used a poverty score that depended only on household socioeconomic characteristics collected in the baseline survey, rather than directly on household income. This choice was driven by a number of factors. First, target households had highly unstable income, so current income was often a bad proxy for permanent income. Second, because this population often worked in the informal sector, it was difficult to verify their reported income levels against official social security records, opening up the possibility of misreporting. By using a wide array of socioeconomic characteristics in the poverty score, as opposed to self-reported income, the government hoped to minimize strategic misreporting.

The poverty score was devised by researchers at the University of the Republic (*Universidad de La República*), including by two authors of this paper, and was based on a probit model of the likelihood of being above a critical per capita income level, using a highly saturated function of household variables.⁷ The model was first estimated using the 2004 National Household Survey (*Encuesta Continua de Hogares*). The resulting coefficient estimates were then used to predict a poverty score for each applicant household using data in the *PANES* baseline survey. Only households with poverty scores below a predetermined threshold - i.e., those with low predicted income - were assigned to program treatment. The eligibility thresholds were allowed to vary across the country's five main administrative regions,⁸

This discontinuous rule for program assignment was suggested to Ministry officials by the authors of this paper with the explicit goal of carrying out the prospective evaluation of *PANES*. Government officials proved receptive to the proposal and remarkably uninvolved in the design and

⁷ These included: the type of household (head only; head and spouse; head and children; head, spouse and children only; with non-relatives, with relatives other than head, spouse or children), an indicator for public employees in the household, an indicator for pensioners in the household, average education of individuals over age 18 and its square, interactions of age indicators (0-5- 6-17, 18-24, 25-39, 40-54,45-64, 65 or more) with gender, indicators for age of the household head, residential overcrowding, whether household is renting its residence, toilet facilities (no toilet, flush toilet, pit latrine, other) and a wealth index based on durables ownership.

⁸ The regional thresholds were set in such a way to guarantee that a similar share of poor households residing in each area were administered the program. This suggests that regional differences in the threshold were not driven by political considerations. The five regions are: Montevideo, North (Artigas, Salto, Rivera), Center-North (Paysandú, Río Negro, Tacuarembó, Durazno, Treinta y Tres, Cerro Largo), Center-South (Soriano, Florida, Flores, Lavalleja, Rocha) and South (Colonia, San José, Canelones, Maldonado).

calculation of the eligibility score.⁹ Similarly, neither the enumerators nor households were ever informed about the exact variables that entered into the determination of the score, the weights attached to them or the program eligibility threshold, easing concerns about manipulation of the score.

There was one additional participation condition related to household income, although in practice this rule disqualified only a handful of applicants. Among households with a poverty score making them eligible for *PANES*, only those with monthly per capita formal sector income (as reported in the social security records, and excluding old age pension earnings and child benefits) below UY\$1,300 could receive the program. All participating households were informed of this rule.¹⁰

The program was rolled out quickly within a year of its launch in 2005. The total cost of the program by the end of 2007 was US\$247,657,026, i.e., US\$2,420 per beneficiary household. On annual basis, this is 0.41% of GDP and 1.95% of Government's Public Social Expenditure. The program was partially financed through an Inter-American Development Bank loan.

II.c. Follow-up survey data

The *PANES* follow-up survey was carried out between December 2006 and March 2007, roughly eighteen months after the start of the program.¹¹ The questionnaire was designed by the authors of this paper in collaboration with Ministry of Social Development staff and the Department of Sociology at the University of the Republic, who were also in charge of data collection. To exploit the discontinuity design, the original survey sample contained data on 3,000 households, including both eligible and ineligible applicants, in the neighborhood of the program eligibility threshold score. There was a desire to over-represent eligible households, leading the sample to be split between eligible and ineligible households in a 2:1 ratio.¹² The initial non-response rate was relatively high at 35%, so 893 replacement households with approximately the same score as the non-response households were

⁹ There was one exception: when officials realized that few elderly would receive program assistance, they asked for a slight adjustment to the poverty score formula. This can largely explain the small numbers (10%) of ineligible households who eventually did receive *PANES* transfers, as we show below in figure 1.

¹⁰ Program participation was also technically contingent on school attendance of all children under age 14 years and regular health checkups for all children and pregnant women, as in many other Latin American contingent cash transfer programs (e.g., Mexico's *PROGRESA/Oportunidades*). However, we have no record of any households losing *PANES* benefits for failing to meet these criteria. In effect, the cash transfers were unconditional.

¹¹ A second follow-up survey was conducted in 2008. We plan to incorporate these data in future research.

¹² This main sample was complemented with data on 500 eligible households farther away from the eligibility threshold, although we do not use these data in the discontinuity analysis in this paper.

subsequently interviewed;¹³ we discuss the implications of non-response later in the paper. Overall, the *PANES* follow-up survey contains data on 9,423 individuals from 2,811 households.

To limit strategic responses, surveyed households were not informed about the scope of the survey: both the title of the survey and information provided to respondents only referred to the university department and made no specific mention to *PANES* or the Ministry. Questions about the *PANES* program were asked at the very end of the questionnaire. In addition to information on housing, household composition, durables possession, work, income and schooling (as in the baseline survey), the follow-up survey collects information on health, economic expectations, knowledge of political rights, participation in social groups, opinions about the *PANES* program, and political attitudes, including support for the government.

The survey data has also been linked to *PANES* administrative data on program receipt, as well as to national social security records that include information on income from formal employment or self-employment, as well as receipt of other government transfers.

II.d Program implementation

Figure 1 reports program receipt as a function of the baseline poverty score. This score is normalized so that all figures are centered on zero, the eligibility threshold, and such that predicted income increases moving to the right on the horizontal axis. In this and all subsequent figures (though not in the regressions that follow) the running variable, the normalized poverty score, is discretized into intervals. Since there are approximately twice as many households to the left of the eligibility threshold (i.e., the eligible households) as to the right, we present twice as many cells for eligible households (50) as for ineligible ones (25), such that each cell contains approximately the same number of observations (37 households). The cells correspond to equally spaced percentiles of the score distribution.

Figure 1 reports the proportion of households in the follow-up survey who declare having benefited from the program at any point since its inception. A linear polynomial on each side of the discontinuity point is also fit to the data. As expected, almost all (97%) potential beneficiaries - i.e., those with a standardized poverty score below poverty zero - have benefited from the program. However, there is some slippage as roughly 10% of ineligible households have also participated at

¹³ Because by design only eligible households with baseline social security income below UY\$1,300 were included in the sample, we also exclude 78 ineligible households with baseline income above this threshold.

some point.¹⁴ This implies that enforcement of the rule was not as strict as implied by the letter of the law, with some ineligible households also receiving treatment. Still, there remains a very large discontinuity in the probability of receipt at the threshold, on the order of 85 percentage points. Although some households lost eligibility over time due to rising non-*PANES* income (as the Uruguayan economy recovered after 2004), roughly 78% of the eligible households still report receiving a *PANES* transfer in the past month.

Although the program included a variety of components, we will not attempt in the rest to disentangle what roles these different elements played in shaping outcomes since there was potentially non-random selection into some of them. We concentrate on the overall effect of program participation at the threshold, which for the vast majority of beneficiary households consisted solely of the monthly income transfer and the food card.

III. RESULTS

We use the follow-up survey, in conjunction with data from the baseline survey and the *Latinobarómetro* public opinion surveys, to explore program effects on political support, the main outcome of interest. We first present average treatment effects and we then explore heterogeneous treatment effects among groups with different baseline characteristics. We also test the validity of our identification assumption, namely that assignment around the eligibility threshold was “as good as random”, as envisioned in the prospective program evaluation design. A leading concern is that manipulation of program assignment by either program officials or enumerators or due to strategic responses, or a correlation between survey non-response and political views. We also highlight the channels through which the program affects political opinions by investigating respondents’ post-program income, their specific views about the policy as well as subjective assessment of their own well-being and the country’s current situation.

III.a. Political support for the government

¹⁴ Self-reported receipt of *PANES* is nearly identical to data in program administrative records, reassuring us about the quality of household survey responses and the matching across datasets. We report program participation as reported in the follow-up household survey in what follows.

We use the following question from the follow-up survey to measure support for the incumbent government: “*In relation to the previous government, do you believe that the current government is -1: worse, 0: the same, 1: better?*”¹⁵

Figure 2 presents answers to this question as a function of the poverty score. The discontinuity at zero provides an estimate of the proportion of individuals who support the current government relative to the previous one, in the *PANES* eligible group versus the ineligible.¹⁶ *PANES* households are significantly more likely to be pro-government: among eligible households the proportion preferring the current government is around 80%, compared to 56% for ineligible households (still a high level of support, as might be expected, since the left-wing coalition is widely supported among the poor). The estimated discontinuity implies that program eligibility is associated with a 24 percentage point increase in support for the government over the opposition coalition. This figures provides evidence that households’ political views are extremely responsive to the receipt of government transfers.

As a way to refine the analysis in Figure 2, we present regression results to examine the robustness of the results to different parametric specifications and to the inclusion of different baseline control variables. Let S_{iR} be the poverty score assigned to household i in region R (where a higher score denotes higher predicted income) and let A_R denote the eligibility threshold in region R , such that in principle only households with scores lower than A_R are eligible for treatment. Let $N_{iR} = S_{iR} - A_R$ be the normalized poverty score. Following Card and Lee (2008), we regress the variable of interest (here being a *PANES* beneficiary) for household i , y_{iR} , on a constant, an indicator for households below the threshold $1(N_{iR} < 0)$, and two parametric polynomials in the normalized score ($f(N_{iR})$ and $g(N_{iR})$), on each side of the threshold, such that $f(0) = g(0) = 0$:

$$(4) \quad y_{iR} = \beta_0 + \beta_1 1(N_{iR} < 0) + f(N_{iR}) + 1(N_{iR} < 0) g(N_{iR}) + u_{iR}$$

The impact of program assignment is captured in the coefficient β_1 , i.e., the change in y at the eligibility threshold. The two fitted plots in Figure 1 and in subsequent figures are obtained by letting $f(\cdot)$ and $g(\cdot)$ to be linear functions of N , though in the regressions we allow for higher order polynomials.

¹⁵ The questionnaire presents the answers in the following order “1: the same, 2: worse, 3: better, 9: does not know?”. In order to use all the observations, we have recodified *does not know* (9) as *the same* (1). Results are essentially identical if we ignore these observations.

¹⁶ This is $1 \times \text{Pr}(\text{Prefer current government}) + 0 \times \text{Pr}(\text{Indifferent between previous and current government}) + (-1) \times \text{Pr}(\text{Prefer previous government}) = \text{Pr}(\text{Prefer current government}) - \text{Pr}(\text{Prefer previous government})$.

The top panel of Table 2 reports regression discontinuity (RD) estimates of equation (5) with an indicator for being a *PANES* beneficiary household as the dependent variable in a first stage specification; these and the subsequent regressions include households with valid responses to both the self-reported program participation and political orientation survey questions. Columns 1 to 4 present specifications with different parameterizations of the functions $f(\cdot)$ and $g(\cdot)$: no polynomial, a first order polynomial (as in Figure 1), up to a third order polynomial. The first stage is strong and estimates vary minimally, between 0.79 and 0.87, when different polynomials are included.

The second panel of Table 2 reports reduced form intention-to-treat (ITT) estimates, where the dependent variable is political support for the government. All estimates are of similar magnitude and statistically significant, suggesting an increase of 20 to 26 percentage points in support for the government among those eligible for *PANES*. Rescaling the ITT estimates by the probability of receiving treatment yields instrumental variable estimates of the local average treatment effect, and these are reported in the bottom panel of Table 2. Being a *PANES* recipient increases support for the government by between 25 and 31 percentage points. We strongly reject the hypothesis that government transfer income does not affect support for the government (in the model in section I, rejecting $H_0: \theta=0$).

With these estimates in hand, we can estimate the cost to an incumbent government of boosting political support using a transfer program. The *PANES* program cost on average US\$2,420 per beneficiary household over the 33 months of the program, equivalently US\$880 per year. This figure is an upper bound on transfers since it includes both program administrative costs as well as certain small project components that benefited both treated and untreated households (e.g., additional funding for teachers in poor communities), but it serves as a useful starting point. Since the average number of voting age adults per household in the sample is 1.78, the annual cost per voter is $US\$880/1.78=495$ assuming that the impact on other adults in the household is similar to that reported by survey respondents. Since *PANES* treatment increases political support by at least 0.25 (Table 2), the annual cost per additional government political supporter is $US\$495/0.25 = 1,980$.

A useful exercise for interpreting the magnitude of this effect is to consider the percentage point vote gain accruing to the government as a result of the *PANES*, under the assumption that the survey responses translate directly into votes. Because 102,353 households were eventually admitted to the program, using the benchmark estimate for the effect of the program of 0.25, this gives an

additional 25,588 votes for the *Frente Amplio*. In the 2004 Uruguayan general the FA received 1,124,761 votes,¹⁷ so this shift is equivalent to an increase in the votes for the FA coalition of 2.3%.¹⁸

We estimate the cost of using a government transfer program to secure one additional political supporter to be approximately US\$1,980 per year, or 36% of 2006 GDP per capita. Even though this study and Levitt and Snyder (1997) employ quite different econometric methodologies and so are not directly comparable, they estimate the cost of securing an additional vote in U.S. House of Representatives elections at US\$14,000, roughly two thirds of 1990 U.S. GDP per capita (in 1990 dollars), so almost twice our estimate.

We explore the sensitivity of responsiveness to the transfer across income levels and political orientation below. The sample households have very low monthly income (only US\$93, or US\$24 per capita, at baseline), and are also traditionally aligned with the political left, as confirmed by the high degree of support for the government even among *PANES* ineligible households, and thus may not be representative of the Uruguayan population as a whole.

III.b Validity of the regression discontinuity design

An alternative explanation for the patterns in Figures 1 and 2 is that assignment to *PANES* favored households with higher greater underlying support for the governing *Frente Amplio* (FA) party. Evidence on manipulation of a program eligibility score in a recent Colombian health insurance program (Conover and Camacho, 2007) suggests that this is far from a remote possibility in Latin America. Unfortunately, we lack data on baseline household political orientation, which prevents us from testing this hypothesis directly. However, a range of evidence makes the alternative explanation highly implausible.

Although virtually all eligible households received the program (Figure 1), manipulation in this context might have taken two other forms. The first possibility is that some ineligible supporters of the FA were eventually admitted to the program. Indeed, Figure 1 shows that around 10% of ineligible households received *PANES* transfers at some point. If this was the result of manipulation, one would expect the ineligible households who were eventually admitted to the program to be stronger FA supporters on average than eligible households. However, there is no evidence that leakage to ineligible

¹⁷ This is 50.04% of all votes cast. Turnout in the 2004 election was typically high for Uruguay, at 89.6% of all adults. (Source: Data Bank, Social Science School, University of the Republic, <http://www.fcs.edu.uy/pri/en/electoral.html>).

¹⁸ The source of this figure is http://encarta.msn.com/fact_631504889/uruguay_facts_and_figures.html. Uruguay's GDP (in exchange rate terms) in 2006 was US\$19.3 billion, or US\$5,514 per capita.

households was driven by political patronage. Regressions like those in panel B of Table 2 that exclude ineligible households do not find significant effects of eligibility on political support. Moreover, note that if this sort of manipulation were taking place, the main RD estimates presented in Table 2 would presumably underestimate the effect of the *PANES* program on political support rather than overstate it.

A second concern is that the variables recorded in the baseline survey, and that determined the poverty score for eligibility in *PANES*, were manipulated by either government officials or enumerators, or that households with closer FA ties somehow learned the formula and were thus able to respond strategically to the questionnaire in order to gain eligibility. Again, this is highly unlikely since the variables entering into the poverty score formula were developed by the authors of this paper and never publicly disclosed, or even directly shared with Ministry for Social Development officials.

An additional concern could arise if survey non-response were systematically related to program eligibility. As a check for non-random assignment around the eligibility threshold, we estimate equation 5 for multiple pre-treatment covariates as well as survey non-response in Table 4 (and present the results graphically in appendix Figure A2). If score manipulation systematically occurred, we might find these characteristics varying discontinuously at the eligibility threshold, to the extent that they are correlated with households' political orientation. We fail to find robust evidence of a discontinuity at the threshold for many household covariates, including: average household members' age and education (for individuals over 18), income, household size, and for the gender, age and years of education of the survey respondent, as well as in the survey non-response in the original survey sample. Consistent with this validity check, the RD results in Table 2 were almost unchanged when household controls were included as controls.

As an additional check for manipulation around the eligibility score threshold, we present the distribution of the standardized score. If manipulation occurred so that ineligible households were assigned a low score, one would expect excess bunching of households below the threshold (DiNardo and Lee, 2004; McCrary, 2008a). Figure 3 reports the proportion of households with different score levels. This is computed for the entire population of households (22,239) in the neighborhood of the threshold $[-0.02, 0.02]$. Following McCrary (2008a) we augment this graph with a local linear estimator of the density function on either side of the threshold. There is some visual indication that households just below the eligibility threshold are overrepresented relative to those just ineligible for the program and that this difference is statistically significant. However, the estimated jump is only on the order of 3 percentage points. Even under the extreme scenario that all these extra households were pro-

government, this could only explain a small fraction of our estimated impact on political support for the government of over 25 percent. Manipulation is not responsible for the large effects in Table 2.

III.c Heterogeneous effects of government transfers

Having established that the association between *PANES*, program assignment and political support for the government is likely to be causal, we next investigate heterogeneous treatment effects. We focus on the two key theoretical implications of the standard probabilistic voting model described above, namely that (i) the political allegiance of poorer individuals is likely to be more responsive to government transfers (due to the declining marginal utility of consumption), and that (ii) those with centrist underlying political affinities are more responsive to transfers than individuals with more extreme political views.

We split the household sample into 30 equally sized groups corresponding to baseline income, where each group contains roughly 85 household observations. Since income did not enter directly into the determination of the *PANES* eligibility score, there is considerable variation in program assignment among households at the same income level.¹⁹ We then run separate IV regressions that control for a linear normalized eligibility score control (as in column 2 of Table 2, Panel C) for each of these 30 groups. Figure 4 reports the results graphically: each dot corresponds to the estimated fuzzy RD effect for each of the 30 income groups as a function of baseline income, and the relationship is clearly negative and approximately linear. The 30 regression coefficients are then regressed on a polynomial in the average baseline log income (by group) to yield the solid fitted plot in the figure, where the dotted lines represent 95% confidence intervals (with standard errors clustered by income percentile).

The effect of *PANES* on political support falls with the level of pre-treatment income: the estimated coefficient is -0.322 (s.e. 0.087, Table 3) implying that a 10% increase in baseline income reduces the gain in government support by 3 percentage points. While at the smallest of the observed household per capita incomes in our sample, the estimated coefficient on receiving *PANES* is nearly 0.70, towards the upper end – which corresponds roughly to the national poverty line – it falls practically to zero. These estimates are likely to be a lower bound on the true income effect, since household income is likely to be somewhat mismeasured for a poor population with degrees of

¹⁹ A further source of variation in program assignment stems from the fact that the eligibility threshold point was set somewhat differently across the country's five regions, so households at a given per capita income level could be treated in one region but not others.

informal sector and self-employment, leading to attenuation bias (although it is difficult for us to quantify the extent of this bias towards zero).

We next estimate the effect of treatment across voters with different predicted political affinities. Unfortunately, the follow-up survey does not provide direct information on respondents' voting behavior in earlier elections. However, the Uruguay *Latinobarómetro* survey asks the following question: “*If elections were held this Sunday, which party would you vote for?*”. We use *Latinobarómetro* data from 2001 to 2004 to estimate a probit model for the probability of voting for the *Frente Amplio* (FA) on the following covariates: gender, age and age squared and the interaction with gender, years of education and its square, indicators for *departamentos*, and an indicator for homeownership. The probability of voting for the FA increases with age, peaking at around age 40 and then declining (appendix Table A1), while education is positively associated with being left-leaning, gender differences appear minor, and there are significant differences across *departamentos*. We use this model to predict pre-program political orientations for sample households, where the same covariates are available in the *PANES* baseline survey. Then using a procedure analogous to that used to understand treatment effects across income groups, we estimate heterogeneous effects of *PANES* treatment across individuals with different predicted political support for the government.

Panel B of Figure 4 shows that the effect of *PANES* varies considerably with respect to predicted political affinity. It is small and close to zero for voters with very low propensity to vote for the FA, it rises for groups with sizeable probabilities of voting for either the FA or the center-right coalition, and then declines again for voters who seem strongly aligned with the FA. In the figure we report a best fit quadratic regression plot, together with 95% confidence intervals. The estimated coefficients in Table 3, bottom panel, imply that the influence of *PANES* transfers peak at 46% likelihood of voting for the governing FA party. An inverted-U shaped relationship also holds if instead of using voting intentions we use underlying political ideology, also collected in the *Latinobarómetro* (“*On a scale from 0 to 10, where 0 is left and 10 is right, where would you locate?*”, results not shown). In practice, the data show clear evidence that voters who are predicted to be less politically aligned are more likely to be swayed by the *PANES* transfer program in terms of their self-expressed political support for the government.

We also examined heterogeneous treatment effects along other dimensions (results not shown): although it appears that older individuals, those living in Montevideo and especially women are marginally less responsive to the transfer, these effects are generally statistically insignificant .

III.d Channels explaining greater political support

The estimates in the previous sections show a large increase in support for the government among households that received the *PANES* transfer program. The next question is why.

The theoretical model in section I links voting to utility, or well-being, so we would expect *PANES* program households to claim to be better-off overall. We first report the change in log per capita household income between the baseline and follow-up surveys, graphically in Figure 5 and as regressions in Table 5, row 1. Note that per capita income grows by a remarkable 57% even for *PANES* ineligible households, presumably due to Uruguay's rapid macroeconomic recovery after 2004, although mean reversion could also be playing some role. Income growth among *PANES* eligible households is even faster, at over 80%, and thus the estimated regression impact at the threshold is 23% (s.e. 0.04), an effect that is largely robust to additional poverty score polynomials and controls. Although the relative income gain is smaller than what we would have expected ex-ante, given that the *PANES* transfers were equivalent to approximately 53% of baseline income, probably due to being offsetting behavioral responses, eligible households' living standards still improve substantially relative to the ineligible.²⁰

In addition to the income transfer, beneficiaries also received in-kind transfers and services, not all easy to monetize and all potentially increasing well-being. Just by virtue of being included in the program, some beneficiary households might have experienced an improvement in their self-esteem and overall well-being.

To investigate these issues further, we consider an alternative subjective measure of household well-being, using the following question from the follow-up survey: "*on a scale 1 to 5, where 1 is very bad and 5 very good, how would you qualify the current situation of your household?*" (which we re-scaled from -2 to +2). Consistent with the model, the data clearly show an improvement in self-perceived well-being as a result of treatment. The average assessment of the household's current situation among the ineligible is -0.30, implying that respondents regard their current situation as being rather bad. However, this assessment is 0.29 points higher among *PANES* eligible respondents, and the

²⁰ Eligibility has a negative effect (-0.014) on income growth net of the transfer. It is unclear whether this is a truly behavioral response, such as a fall in labor supply, or partly the result of income underreporting among the eligible. We also find some positive effects of the program current school enrollment (for children aged 7-18) and medical visits in the last three months (for children aged 0-6 and women of childbearing age, 14-35). There is no evidence of impacts on durables ownership, home characteristics or self-reported health (Amarante et al., 2008).

difference is very precisely estimated (s.e. 0.04, Table 5). The effect comes in similar proportions from eligible respondents being more likely to declare their household situation “good” and less likely to declare their situation “bad” or “very bad” (not shown). Results are quite robust across specifications although standard errors increase in the more saturated specifications.

These improved objective and subjective measures of well-being still do not definitively explain why *PANES* households express more support for the current government, but there are numerous plausible explanations. Treated households might fear that the opposition party would deprive them of their *PANES* benefits if it came to power, and thus express greater support for the government. Another leading possibility is that many households are overweighting their own personal experiences in evaluating government performance and prevailing economic conditions, an issue that has found widespread support in experimental economics and behavioral economics in recent years (see Simonsohn et al 2008). Panel C in Figure 5, and Table 5, reports households’ satisfaction with the country’s current situation, using the question: “on a scale 1 to 5, where 1 is very bad and 5 very good, how would you qualify the current situation of the country?” (again rescaled from -2 to +2). *PANES* eligible households express a much more positive assessment of Uruguay’s current situation than the ineligible, with an RD coefficient estimate of 0.27 (s.e. 0.04), that varies somewhat across specifications (from 0.18 to 0.35) but remains positive and generally statistically significant.

III.e. Greater support among recipients – or bitterness among non-recipients?

A remaining issue is one of interpretation, namely, whether the estimated *PANES* impacts are due not only to treated households being more supportive of the government, but whether untreated households are bitter at their exclusion from the program, in which case our estimates are a combination of two distinct effects. Although there is no direct way to measure these effects in our data since there is no information on pre-program political orientation, we provide suggestive evidence that the embitterment effect is unlikely to be large.

We again use the *Latinobarómetro* opinion data to predict household’s support for the current government relative to the previous one. The *Latinobarómetro* asks: “Do you approve or disapprove the administration of the government headed by the President: 1 Approve, 2: Disapproves, 3: Does not know/does not respond”. We use a multinomial logit on the same covariates as those in Table A1 to predict the support for the current and the opposition government in the 2005 and 2006

Latinobarómetro and use the predictions of this model to derive counterfactual support for the current government among untreated households located away from the eligibility threshold.

Figure 6 reports the difference in the proportion of households predicted to approve of the current administration minus those predicted to disapprove it as a function of the normalized poverty score. We also present the level of support in the follow-up survey (as in Figure 2). The predicted data actually provide a level of support remarkably similar to the follow-up survey to the right of the discontinuity, clear evidence against the embitterment hypothesis.

IV. SUMMARY AND CONCLUSIONS

Consistent with the standard probabilistic voting model in political economy, we find that beneficiaries of a large government anti-poverty program in Uruguay are significantly more likely to support the current government than non-beneficiaries. We use individual level data on political support and a credible regression discontinuity research design to estimate these effects, constituting a methodological advance in this branch of the empirical political economy literature. We find large and robust effects on the order of 25 to 31 percentage points. We also find pronounced heterogeneity across income groups and those with different predicted political orientations, in line with the predictions of the theory. In particular, there are larger impacts of government transfers among the poorest beneficiary households – consistent with the point that the marginal utility of consumption is highest for this group – and among those households predicted to be least politically aligned. The finding that those near the center of the political spectrum are more responsive to government transfers than those with extreme views provides strong empirical support for the logic of targeting “swing voters” for redistribution.

An important issue is how likely these results are to generalize to other settings. While Uruguay is a middle income country, it has exceptionally well-developed democratic institutions and a long tradition of strong political parties. This suggests that the findings of this paper are relevant not only for Latin America but also possibly for high income countries like those in North America and Europe with similarly strong political institutions.

Yet there are several reasons to take the “cost per vote” figures with caution. First, it is impossible to know how much smaller the vote gains for the government would have been had the transfer amount been smaller (or larger). A more complicated program design that randomly varied transfer amounts across households would be needed for credible identification. It remains possible that the simple act of receiving a transfer of any amount boosts political support. Second, it is difficult to

extrapolate these results to the case where a right-wing party implemented a similar transfer policy, or if the policy had been implemented in a period of economic contraction, rather than the largely favorable macroeconomic environment that Uruguay experienced from 2005 to 2007.

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Appendix: PANES program components

Figure A1 presents the probability of ever having received each separate component of the *PANES* program. Panel A reports the probability of ever having received the main cash transfer (*ingreso ciudadano*), the central element of the program, consisting of a monthly transfer independent of household size initially set at UY\$1,360 (approximately US\$56) per month, equivalent to half the monthly minimum wage, and was later adjusted upward in nominal terms for inflation. Households in the treatment group received the monthly income provided they were not involved in public works employment (*trabajo por Uruguay*), which paid a monthly salary of UY\$2,720 in lieu of the cash transfer. Participation in this employment scheme was voluntary and, among households who applied for jobs, participants were selected by lottery. Nearly all eligible households declared having received the cash transfer at some point during the program while only a minority (13%) benefited from public works employment, as shown in Panel B.

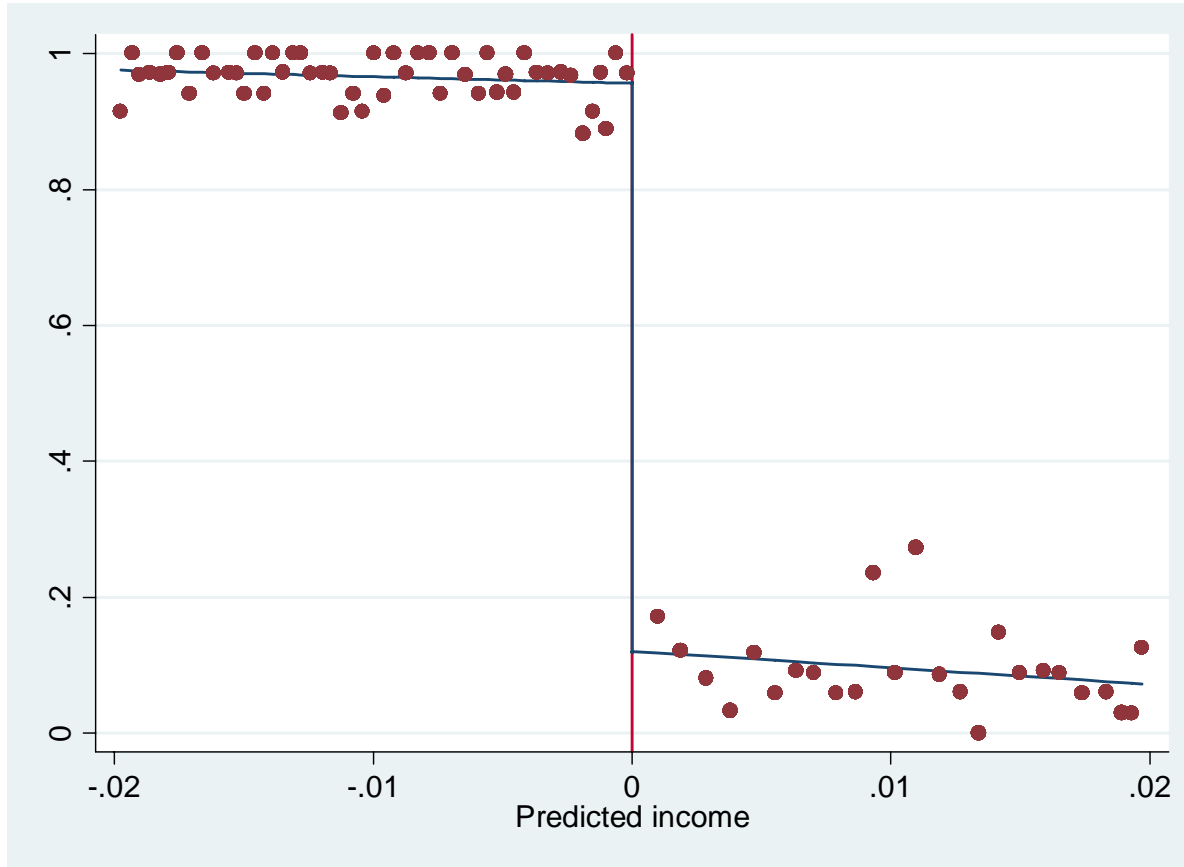
Panel C reports the proportion of households receiving the food card (*tarjeta de alimentos*). This was the second central element of *PANES* and covered households with children under age 18 and pregnant women. This was an in-kind transfer that operated through an electronic debit card, whose monthly value varied between UY\$300 and UY\$800 depending on household demographic composition. Purchases could be made in authorized stores. The program covered around 65% of eligible households while participation among the ineligible households was close to zero.

Around 12% of eligible households reported having participated in a job training program (*rutas de salida*). These were programs of six months duration implemented by NGOs, neighborhood commissions, and political and trade union organizations for groups of up to 25 participants. While participation for beneficiary households was compulsory in principle, no formal criterion was established regarding which member of the household had to participate, or the content of the training activities, and Panel D shows clearly that the aim of universal job training was far from being achieved.

For simplicity the remaining components of the *PANES* program are collected into an “other” category in the last panel of Figure A1. This category includes: connection to public utilities networks (water and electricity) for a nominal fee, in-kind transfers of building materials for home improvements, up to approximately US\$1,000; health care including free dental and eye care (e.g., cataract surgery performed in Cuba) and prostheses; micro-finance loans and technical assistance for small entrepreneurial activities; and temporary accommodation for homeless households. Overall, around 17% of beneficiary households reported having received at least one of these additional components.

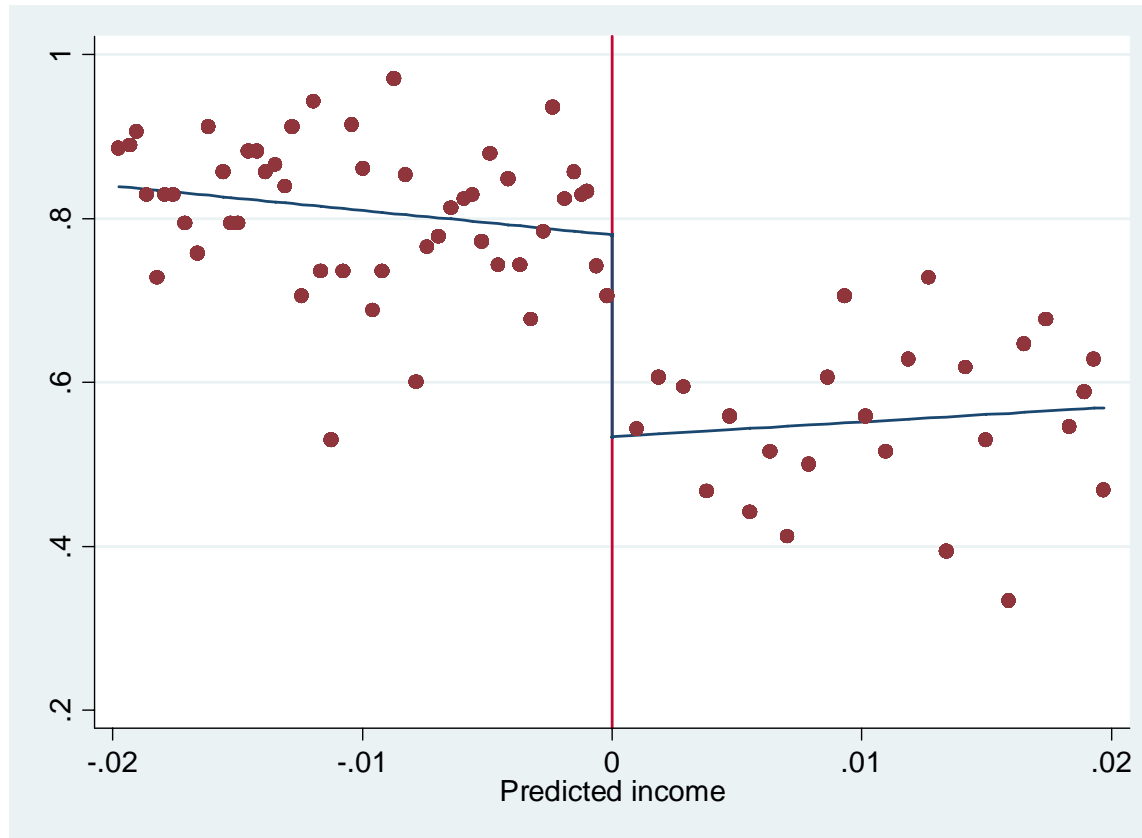
Additional government programs that affected both *PANES* beneficiary and non-beneficiary households included additional school teachers in disadvantaged neighborhoods (*maestros comunitarios*) and cheaper access to the public health sector.

Figure 1: Participation in *PANES* and eligibility



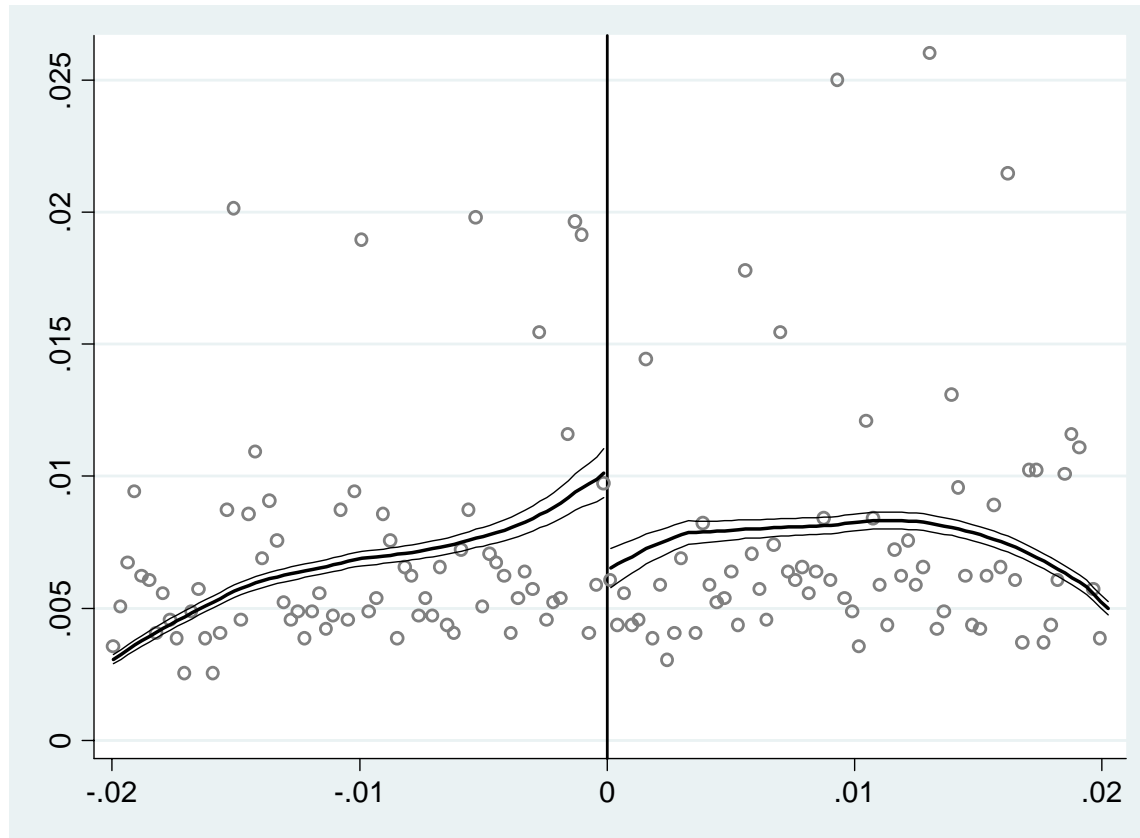
Notes. The picture reports the proportion of households ever enrolled in *PANES* as a function of the standardized score. The fitted plots are linear best fits on each side of the eligibility threshold.

Figure 2: Political support for the government and program eligibility



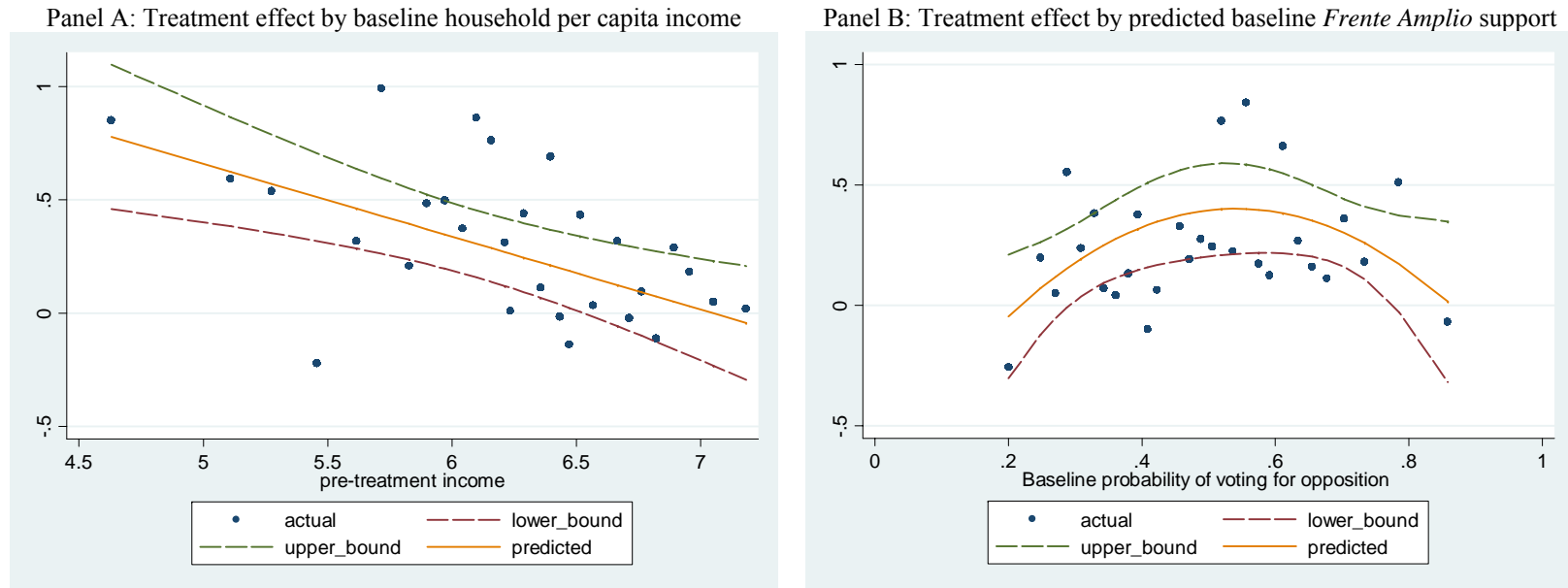
Notes. The figure reports the average support gap for the current government relative to the previous government as a function of the standardized score. Source: *PANES* follow-up survey. The fitted plots are linear best fits on each side of the eligibility threshold.

Figure 3: Distribution of the standardized *PANES* eligibility score



Notes. The graph reports the distribution of the standardized eligibility score for the universe of applicant households in the neighborhood of the discontinuity point (following McCrary 2008a).

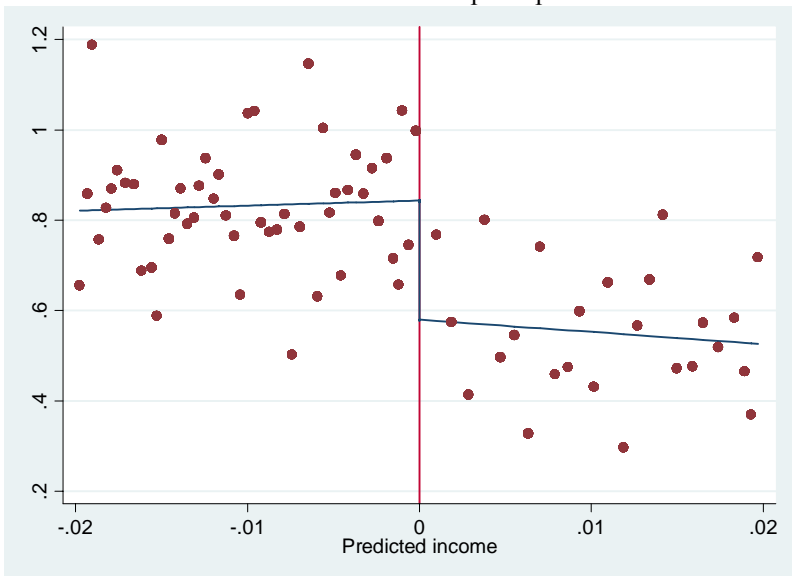
Figure 4: Program participation and Political support, Heterogeneous effects



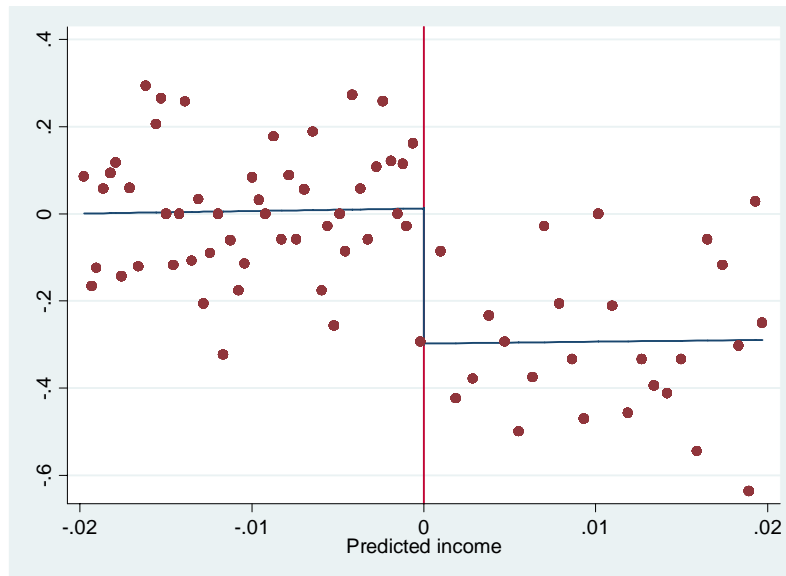
Notes. The left hand side panel reports fuzzy RD estimates of the effect of treatment on support for 30 bins of the pre-treatment income distribution and the best-fit linear regression (with associated confidence interval around the discontinuity point). The right hand side panel reports the same regression for 30 bins of the predicted baseline *Frente Amplio* support, with a quadratic fit. See text for details. Source: *PANES* Follow-up survey and *Latinobarómetro* 2001-04.

Figure 5: Eligibility, well-being and satisfaction

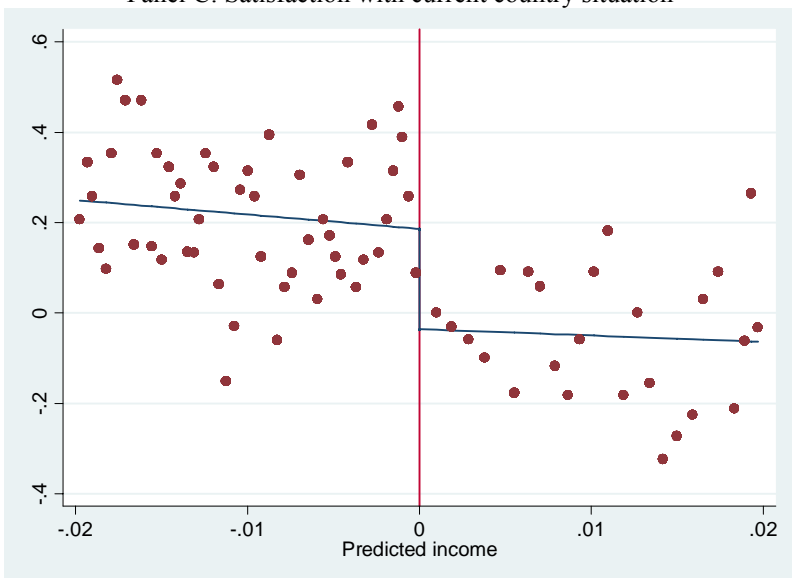
Panel A: Growth in household per capita income



Panel B: Satisfaction with current household situation

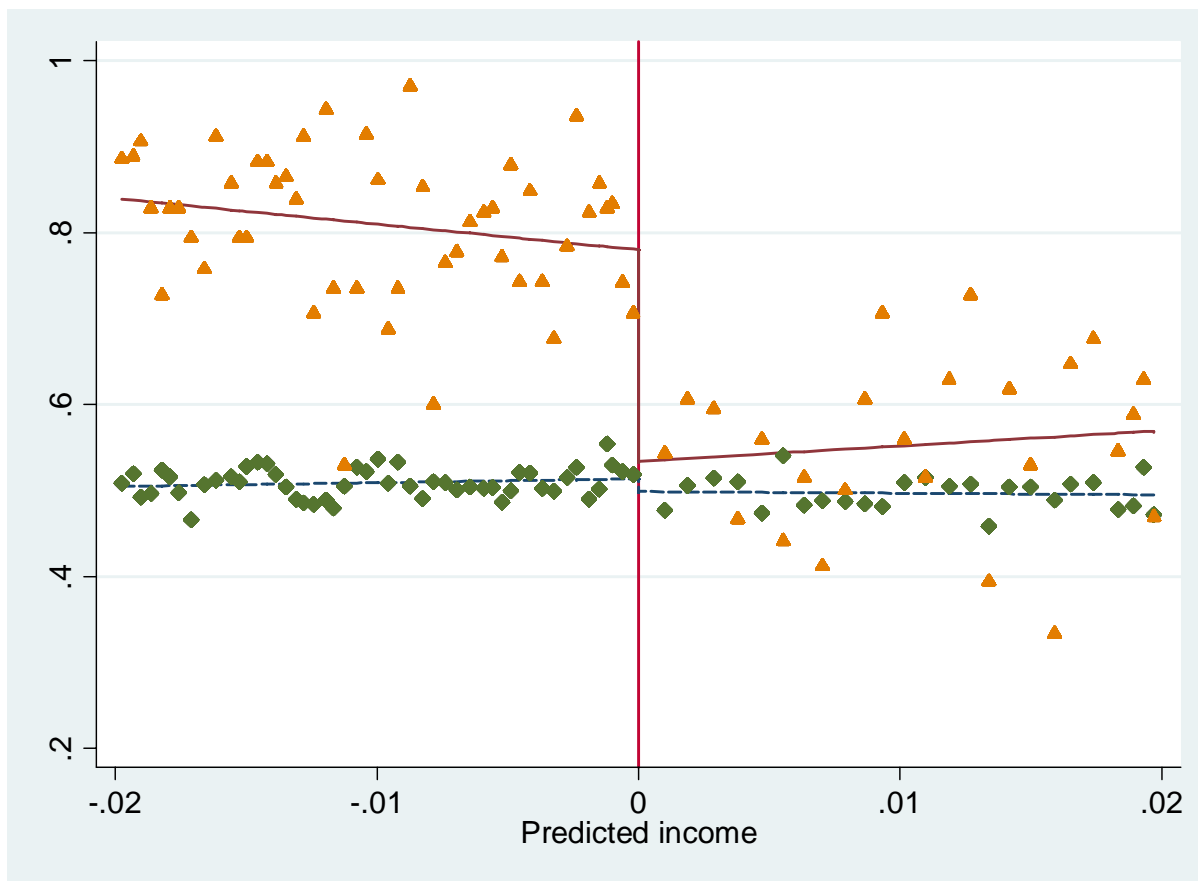


Panel C: Satisfaction with current country situation



Notes. Panel a reports growth in income between baseline and the follow-up survey. Panel B reports the respondents' assessment of - respectively - the current household's and country's situation. See also notes to Figure 1.

Figure 6: Proportion expressing preference for current government:
Actual (triangles / solid line) and predicted based on *Latinobarómetro* (diamonds / dashed line)



Notes. The figure reports the proportion of households favoring the current government minus those favoring the previous government (triangles / solid line) and those predicted to approve of the current government minus those predicted to disapprove using the *Latinobarómetro* 2005-06 (diamonds / dashed line) as a function of the standardized PANES eligibility score.

Table 1: Human development and democracy in Uruguay and selected countries

	UNDP <i>Human Development Report</i>				<i>The Economist Intelligence Unit</i> democracy index				
	Human Development Index	GDP per capita (PPP)	Life expectancy	Gross school enrolment rate	Democracy	Rank	Electoral process	Functioning of govt.	Political culture
Uruguay	0.852	9,962	75.9	88.9	Full	27	10.00	8.21	6.88
USA	0.951	41,890	77.9	93.3	Full	17	8.75	7.86	8.75
Argentina	0.869	14,280	74.8	89.7	Flawed	54	8.75	5.00	5.63
Brazil	0.800	8,402	71.7	87.5	Flawed	42	9.58	7.86	5.63
Chile	0.867	12,027	78.3	82.9	Flawed	30	9.58	8.93	6.25
Colombia	0.791	7,304	72.3	75.1	Flawed	67	9.17	4.36	4.38
Mexico	0.829	10,751	75.6	75.6	Flawed	53	8.75	6.07	5.00
Venezuela	0.792	6,632	73.2	75.5	Hybrid	93	7.00	3.64	5.00

Source: UNDP (2007) and The Economist Intelligence Unit (2007).

Table 2: Program eligibility, participation, and political support for the government

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A:	First stage: Ever received <i>PANES</i> (dep. var.)							
Program eligibility	0.872*** (0.012)	0.837*** (0.025)	0.841*** (0.045)	0.789*** (0.071)	0.866*** (0.013)	0.842*** (0.026)	0.835*** (0.046)	0.782*** (0.071)
Panel B:	Reduced form: Government support (dep. var.)							
Program eligibility	0.256*** (0.025)	0.244*** (0.052)	0.260*** (0.083)	0.199* (0.114)	0.242*** (0.026)	0.240*** (0.053)	0.278*** (0.085)	0.209* (0.116)
Panel C:	IV: Government support (dep. var.)							
Ever received <i>PANES</i>	0.294*** (0.029)	0.292*** (0.061)	0.309*** (0.097)	0.252* (0.141)	0.279*** (0.030)	0.285*** (0.062)	0.333*** (0.099)	0.267* (0.145)
Score controls	None	Linear	Quadratic	Cubic	None	Linear	Quadratic	Cubic
Other controls	No	No	No	No	Yes	Yes	Yes	Yes

Notes: the table reports first stage (Panel A), reduced form (Panel B), and IV (Panel C) estimates of the effect of *PANES* on political support. The instrument is an indicator for a household score below the eligibility threshold. The endogenous variable is defined as ever having received *PANES*. Columns 1 to 4 include, in order, a polynomial in the standardized score of degree 0, 1, 2, and 3, and these polynomials interacted with the eligibility indicator. Columns 5 to 8 additionally control for pretreatment characteristics (average household member age, average household education, number of household members, log per-capita income, interview month indicators, age, education and gender of the respondent, *departamento* indicators). Number of observations in columns 1 to 4: 2,552; in columns 5 to 8: 2,425. Standard errors clustered by score in brackets. Standard errors are almost identical (differing by roughly 1%) when we use the jackknife approach in McCrary (2008b).

Table 3: Participation, and political support for the government: Heterogeneous effects

By pretreatment income	
Log pre-treatment household income	-0.322*** (0.087)
By political orientation	
% voting opposition 01-04	4.158** (1.614)
% voting opposition 01-04 ²	-3.841** (1.5677)

Notes. The table reports the estimated change in the effect of the program on support by pre-treatment income (top panel) and by predicted level of support for the opposition coalition (bottom panel). See text for details.

Table 4: Eligibility and pre-treatment characteristics. Reduced form estimates

Dependent variable:	(1)	(2)	(3)	(4)
Log per-capita income at baseline	-0.118*** (0.026)	-0.060 (0.055)	-0.016 (0.089)	-0.019 (0.126)
Average years of education at baseline	0.056 (0.095)	0.022 (0.193)	-0.330 (0.288)	-0.306 (0.394)
Household size at baseline	0.372*** (0.108)	-0.221 (0.226)	-0.444 (0.336)	-0.792* (0.451)
Average age at baseline	-4.971*** (1.012)	-2.335 (2.029)	-3.098 (2.970)	-1.921 (3.945)
Beneficiary female	0.091*** (0.027)	0.000 (0.053)	-0.032 (0.082)	-0.030 (0.113)
Beneficiary years of education	0.244* (0.137)	0.221 (0.284)	0.444 (0.424)	0.581 (0.586)
Beneficiary age	-3.108*** (0.747)	-1.964 (1.490)	-3.152 (2.244)	-0.940 (3.086)
Survey non-response rate	0.002 (0.017)	0.061* (0.035)	0.031 (0.055)	-0.088 (0.076)
Score controls	None	Linear	Quadratic	Cubic

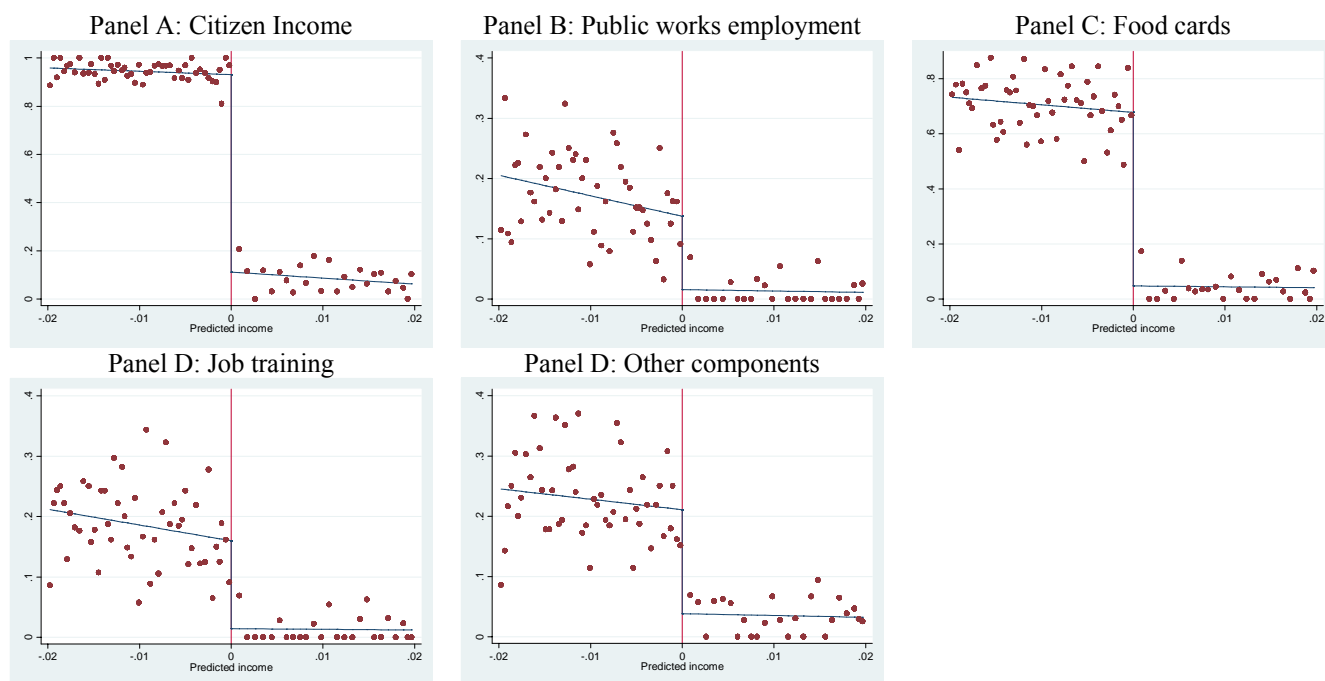
Notes. The table reports results from regressions of various pre-treatment characteristics on the program eligibility indicator. See also notes to Table 2. Number of observations 2,425 except in the last row that is 3,751.

Table 5: Program eligibility, participation, and additional outcomes. Reduced form estimates

Dependent variable:	(1)	(2)	(3)	(4)
Per capita income growth	0.233*** (0.035)	0.250*** (0.068)	0.211* (0.111)	0.137 (0.150)
Satisfaction with household situation	0.291*** (0.039)	0.332*** (0.083)	0.308** (0.130)	0.357* (0.197)
Satisfaction with country situation	0.265*** (0.039)	0.180** (0.080)	0.161 (0.127)	0.354** (0.180)
Score controls	None	Linear	Quadratic	Cubic
Other controls	Yes	Yes	Yes	Yes

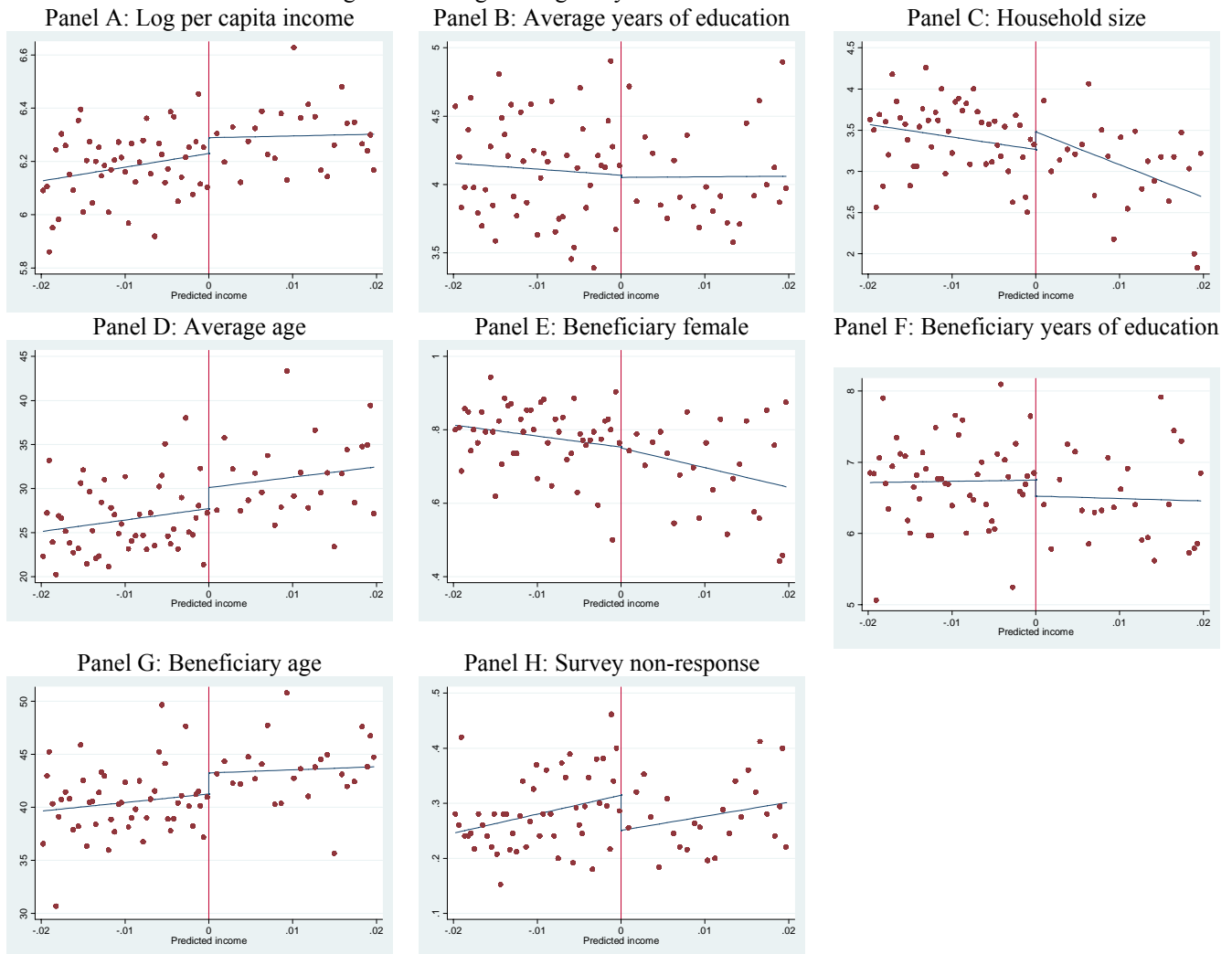
Notes. The table reports results from regressions of various outcomes on the program eligibility indicator. Regressions include controls as in columns 5 to 8 of Table 2. See also notes to Table 2.

Figure A1: Program take up rate by component



Notes. The figures report the proportion of households who had ever received each separate component of *PANES*, as a function of the standardized score.

Figure A2. Program eligibility and baseline characteristics



Notes. Panels A to G report the average value of a number of pre-treatment characteristics as a function of the standardized score. Panel H reports survey non-response.

Table A1: Probability of voting for the *Frente Amplio*: Marginal effects

	Marginal effect	s.e.
Female	0.126	(0.128)
(Age/10)	0.156***	(0.045)
(Age/10) x Female	-0.104*	(0.060)
(Age/10) ²	-0.022***	(0.004)
(Age/10) ² x Female	0.012**	(0.006)
Years of education	0.046***	(0.012)
Years of education ²	-0.002***	(0.001)
Home owner	-0.093***	(0.021)
<i>Departamento</i> indicators:		
Artigas	-0.334***	(0.055)
Cerro Largo	-0.096**	(0.041)
Colonia	-0.230***	(0.051)
Canelones	-0.151***	(0.057)
Durazno	-0.350***	(0.068)
Florida	-0.174***	(0.064)
Lavalleja	-0.339***	(0.058)
Maldonado	-0.219***	(0.045)
Paysandú	-0.111**	(0.045)
Rio Negro	-0.428***	(0.044)
Rivera	-0.236***	(0.060)
Rocha	-0.261***	(0.068)
Salto	-0.336***	(0.039)
San Jose	-0.194***	(0.069)
Soriano	-0.216***	(0.054)
Tacuarembó	-0.326***	(0.043)
Treinta Y Tres	-0.379***	(0.057)
Observations	2,909	

Notes. The table reports results from a probit model of voting intentions on a number of covariates. The excluded *departamento* is the capital, Montevideo. Source: *Latinobarómetro*, 2001-2004.