# 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. The program mainly consisted of a monthly cash transfer for a period of roughly two and half years. Using the discontinuity in program assignment based on a pre-treatment score, we find that beneficiary households are 21 to 28 percentage points more likely to favor the current government (relative to the previous government). Impacts on political support are larger among poorer households and for those near the center of the political spectrum, consistent with the probabilistic voting model in political economy. Effects persist after the cash transfer program ends. We estimate that the annual cost of increasing government political support by 1 percentage point is roughly 0.9% of annual government social expenditures.

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

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Are voters willing to trade-off some of their ideological attachments in exchange for higher consumption? This is a frequent assumption in leading models of individual voting behavior: 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 (Lindbek and Weibull, 1987, Dixit and Londregan, 1996, 1998, Persson and Tabellini, 2002), 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 more extreme 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 theory, empirical evidence on the impact of transfers on individual voting behavior is remarkably scant and rarely based on credible research designs. Identifying the effect of redistributive politics on individual political preferences 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, i.e., 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 base. 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 an economic crisis, a center-left coalition took power in

Uruguay for the first time and swiftly introduced a large anti-poverty program, called *PANES*. The main component of *PANES* was a conditional cash transfer, similar to those recently implemented elsewhere in Latin America (including the well-known Mexican *Progresa*/*Oportunidades* program). Household eligibility for the program was determined by a predicted income score based on a large number of pre-treatment covariates. Only households with a score below a predetermined threshold were eligible for the program. Indeed the data show almost perfect enforcement of the assignment rule and we can confidently rule out manipulation of program assignment on the part of the government.

Eighteen months following the start of the program, households with income scores in the neighborhood of the threshold were surveyed and asked a series of questions including their support for the current government. Because assignment to the program near the threshold was nearly "as good as random", 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 shed light on the trade-off between household consumption and political ideology.

In our main empirical finding, the regression discontinuity analysis indicates that *PANES* beneficiaries were 21 to 28 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 on the order of US$2,000 per year, or one third of national GDP per capita (though this estimate is an upper bound cost if political impacts persist after the program has ended). This implies that a government seeking to increase its vote share by 1 percentage point would need to increase spending by around 0.9% of total annual government social expenditures. Uruguay has highly developed democratic political institutions for a middle-income country, suggesting that some of the political findings could also be relevant for wealthier 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 U.S. 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 their instrumental variable strategy 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 positive impacts of government spending on support for the incumbent in Spain. Chen (2008a, 2008b) estimates the impact of government transfers on voting in the United States, and estimates the cost of an additional vote is on the order of US$7,000. Like us he finds that this cost is increasing in household income but argues that core supporters are cheaper to buy off, in contrast to our finding. Like Levitt and Snyder (1997), Chen uses aggregated voting data, rather than the individual level data we prefer, and finds that there is systematic targeting of government assistance as a function of baseline voting patterns (with Republican areas favored), complicating the interpretation of his econometric results, which rely on the quasi-random path of hurricanes to predict federal government transfers. 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 there. A related analysis using an observational design and U.S. data is Markus (1988).[1]

[1] A related literature explores the implications of voters' political ideology on political parties' transfers 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). We focus on the impact of government transfers on voting choices but there is also evidence of direct vote buying in Latin America, including Schaffer (2007) and Stokes (2005).

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. The final section concludes.

## I. THE PROBABILISTIC VOTING MODEL

The standard probabilistic voting model (Lindbeck and Wiebull, 1987; Dixit and Londregan, 1996) is useful for framing 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 to different social groups is a choice variable. For simplicity, we assume that the transfer schedule of the opposition party $B$ is fixed, for instance, at what it was when they were last in power, and focus on the policy decisions of the incumbent party.

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 $g \in \{1, 2, \ldots, G\}$, 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 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 $g$. Analogously, individual consumption 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 preference[2] for the governing party iff:

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

$X_g^*$ 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, $V_{Ag}$, thus depends on the distribution of underlying political affinities:

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

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

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 analogue in our empirical analysis:

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

Model (1) to (3) provides testable implications for voter behavior in response to government transfers. 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 currently 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 concavity assumption, 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

[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.

some redistribution to the poor independent of their ideological orientation. This theoretical implication is tested below by examining the interaction between pre-program income and transfer receipt.

Note finally that the $N_g$ term implies that more votes can be gained 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]

## II. THE *PANES* PROGRAM IN URUGUAY

Uruguay is a small Latin America country, home to 3.3 million individuals, half of whom live in the capital of 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 low levels of corruption, and free and fair elections (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%, unemployment reached its highest level in twenty years (at 17%), 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

[3] Related 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, $\Sigma_g\{N_g\, T_{Ag}\} = 0$. This generates an intuitive first order condition, in which the government equates the marginal vote gain from increased transfers across all social groups (taking the policy position of the opposition to be fixed, although the finding generalizes to the strategic game, see Dixit and Londregan, 1996): $f_g(X_g^*)\; U_g'(C_{Ag}) = \lambda_A$ 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 subset of the population, namely, the surveyed households near the *PANES* program eligibility threshold. This data limitation leads us to restrict our empirical focus to these voters' responsiveness to the 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. The Uruguayan electoral system is presidential with proportional representation in Congress.

focused on transfers to the elderly population.[5] Yet constrained in part by a severe fiscal adjustment, the ruling center-right *Colorado* party government (which had been in power since 1999 in coalition with the *Blanco* party) focused on expanding 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, capitalizing on widespread dissatisfaction with the economy and the previous government's 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 created the Ministry for Social Development (*Ministerio de Desarrollo Social, MIDES*) and swiftly moved to design and implement the National Social Emergency Plan (*Plan de Atención Nacional a la Emergencia Social*), or *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 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 poorest households in the country, namely the bottom quintile of the income distribution among those falling below the national poverty line. In all, 102,353 households eventually became program beneficiaries, approximately 8% of all households (and 10% of the population).

*PANES* included several distinct components. The largest 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. At US$672 per year, this is a very large transfer for the target population, amounting to approximately 50% of average pre-program household self-reported income. Households with children or pregnant women were also entitled to a food card (*tarjeta alimentaria*), an in-kind transfer that operated through an

[5] In 2002, total expenditure on elderly pensions represented 65% of all government social expenditures, 96% of government cash transfers and almost 13% of GDP. This is reflected in marked differences in poverty incidence by age: while nearly half of children under age five lived in poverty that year, the rate for those 65 and older was only 2% (UNDP, 2008).

electronic debit card, whose annual value varied between US$156 and US$396. Seventy percent of *PANES* beneficiaries also received the 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 publicly invited to apply and the government also made a large outreach effort, sending enumerators to poor communities with the intent of boosting applications. Eventually, 188,671 applicant households were visited by Ministry of Social Development personnel and administered a baseline survey, providing information on household characteristics, housing, income, work, and schooling.

To determine assignment to *PANES* among these applicants, the government used a predicted income score that depended only on household socioeconomic characteristics collected in the baseline survey, not directly on income itself. This choice was driven by a number of factors. First, many households had highly unstable income during the crisis, so current income was seen as a bad proxy for permanent income. Second, because the target 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 income score, as opposed to self-reported income, the government hoped to minimize strategic misreporting. The use of a predicted (as opposed to actual) income score also allows us to estimate heterogeneous impacts across reported income levels, an advantage of our approach that we elaborate on below.

The income score was devised by researchers at the University of the Republic (*Universidad de la República*), including one of the authors of this paper (Arim et al., 2005), 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.[6] The model was first estimated using the 2004 National Household Survey (*Encuesta Continua de Hogares*). The resulting coefficient

[6] 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 years of 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 and over) with gender, indicators for household head age, residential overcrowding, whether the household was renting, toilet facilities (no toilet, flush toilet, pit latrine, other) and a wealth index based on durables ownership (e.g., refrigerator, TV, car, etc.).

estimates were then used to predict an income score for each applicant household using *PANES* baseline survey data. Only households with predicted income scores below a predetermined threshold were assigned to program treatment.[7]

This discontinuous rule for program assignment was suggested to Ministry officials by researchers at the University of the Republic and 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 calculation of the eligibility score, which was computed by bureaucrats at the Social Security Administration (*Banco de Previsión Social*).[8] Similarly, neither the enumerators nor households were ever informed about the exact variables that entered into the score, the weights attached to them, or the program eligibility threshold, easing concerns about manipulation of the score.[9,10]

There was one additional participation condition although in practice it disqualified only a handful of applicants. Only those households with monthly per capita income below UY$1,300 (excluding old age pension earnings and any child benefits) could be included in the program. Hence, the predicted income score was not computed for households with income exceeding that threshold. All participating households were informed of this rule before applying.[11]

The program was fully rolled out within a year of its launch in April 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 an annual basis, the total is 0.41% of GDP and 1.95% of government social expenditures. The program was partially financed through a concessionary Inter-American Development Bank loan.

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[7] The eligibility thresholds were allowed to vary slightly across the country's five main administrative regions. The regional thresholds were set to entitle similar shares of poor households in each area to the program. The 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).

[8] There was one exception: when officials realized that relatively few one person households would receive program assistance, they asked for a slight adjustment to the predicted income score formula.

[9] A relatively small number of households (7,946) were included in the program before September 2005, before the predicted income score was even constructed. An additional 2,552 homeless households were also included in the program irrespective of their score. These households are excluded from the analysis that follows. These households were included in the analysis in an earlier version of this paper, and the main political support results are unchanged.

[10] The eligibility score components and weights were made public on the MIDES website only after the program ended (in January 2009).

[11] 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 conditional cash transfer programs (e.g., Mexico's *PROGRESA*). However, we have no record of any households losing *PANES* benefits for failing to meet these criteria. The cash transfers appear to have been unconditional *de facto*.

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.[12] The questionnaire was designed by the authors of this paper, in collaboration with Verónica Amarante in the Economics Department at the University of the Republic, Ministry of Social Development staff, and the Sociology Department at the University of the Republic. The latter 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.[13] The initial non-response rate was moderate at 30%, and replacement households with approximately the same score as the non-response households were subsequently interviewed; we discuss the implications of non-response later in the paper. Overall, our sample contains information on 2,089 households. [14]

To limit strategic responses, surveyed households were not informed about the exact scope of the follow-up survey. Both the title of the survey and information provided to respondents only referred to the university department and neither made specific mention of *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 collected 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, our key outcome variable.

II.d Program implementation

Figure 1 reports the proportion of households who benefited from the program at any point since its inception, as a function of the baseline predicted income score. The figure is based on

[12] A second follow-up survey with the same households was conducted in early 2008, as we discuss below.
[13] This main sample was supplemented 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.
[14] We restrict the sample to households that joined the program after September 2005 (and thus for whom inclusion was based on the predicted income score), with baseline social security income below UY$1,300, that were not homeless, and with a valid response to the question on support for the current government.

program administrative records. The score was 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 regression tables) the normalized predicted income score is discretized into intervals. Since there are approximately twice as many households to the left of the eligibility threshold (i.e., the *PANES* eligible households) as to the right, we present twice as many cells for eligible households (40) as for ineligible ones (20), such that each cell contains approximately the same number of observations (35 households). These cells thus correspond to equally spaced percentiles of the score distribution. A linear polynomial on each side of the discontinuity point is also fit to the data.

The figure demonstrates that program implementation was remarkably clean. Among applicants practically all potential beneficiaries - i.e., those with a standardized predicted income score below zero - benefited from the program. The opposite holds for ineligible households, and the discontinuity in the likelihood of program receipt at the threshold is 98 percentage points. This implies that enforcement of the rule was nearly as strict as implied by the letter of the law.

Although the program included a variety of components, we do not attempt 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 in some cases) to explore program effects on political support, the main outcome of interest. We first present average treatment effects, and 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 nearly "as good as random", as envisioned in the prospective program evaluation design. A leading concern is manipulation of program assignment by either officials or enumerators, due to strategic responses, or a correlation between survey non-response and political views. We also highlight the channels through which the program affects attitudes by investigating respondents' post-program income, as well as subjective assessments of their own well-being and the country's current situation.

III.a. Political support for the government

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 worse (-1), the same (0), better (+1)?*".[15] Figure 2 presents answers to this question as a function of the normalized predicted income 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. The effect can also be thought of as the net gain in votes for the government relative to the political opposition.[16]

*PANES* households are significantly more likely to be pro-government: among eligible households relative support for the current government is around 81%, compared to 55% 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 25 percentage point increase in support for the government over the opposition coalition. This figure provides evidence that households' political views are extremely responsive to the receipt of government transfers.

To refine the analysis, we present regression results to examine robustness to different parametric specifications and to the inclusion of baseline control variables. Let $S_i$ be the predicted income score assigned to household $i$ (where a higher score denotes higher predicted income) and let $E$ denote the eligibility threshold, such that in principle only households with scores below $E$ are eligible for treatment. Let $N_i=S_i-E$ be the normalized income score. Following Card and Lee (2008), we regress the variable of interest (here being a *PANES* beneficiary) for household $i$, $y_i$, on a constant, an indicator for households below the threshold $1(N_i<0)$, and two parametric polynomials in the normalized score ($f(N_i)$ and $g(N_i)$), on each side of the threshold, such that $f(0)=g(0)=0$:

$$y_i=\beta_0 + \beta_1\, 1(N_i<0) + f(N_i) + 1(N_i<0)\, g(N_i) + u_i \qquad (4)$$

[15] The questionnaire presents responses in the following order "*1: the same, 2: worse, 3: better, 9: does not know?*". We recode the few "*does not know*" answers as "*the same*", though results are nearly identical if we ignore them.
[16] This is *1*x*Pr*(*Prefer current government*) + *0*x*Pr*(*Indifferent between previous and current government*)+ (*-1*) x *Pr*(*Prefer previous government*) = *Pr*(*Prefer current government*) - *Pr*(*Prefer previous government*).

The impact of program assignment is captured by $\beta_1$, i.e., the change in $y$ at the eligibility threshold. The two fitted plots in Figures 1 and 2 (and subsequent figures) are obtained by letting $f(.)$ and $g(.)$ be linear functions, though in the regressions we also allow for quadratic functions.

The top panel of Table 2 reports first-stage regression discontinuity (RD) estimates of equation (4) with an indicator for being a *PANES* beneficiary household as the dependent variable; 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 3 present specifications with different parameterizations of the functions $f(.)$ and $g(.)$: no polynomial, a first order polynomial (as in Figure 1), and a second order polynomial. The first stage is strong and estimates vary minimally, between 0.96 and 0.99 across specifications, including those that also control for a variety of baseline household controls (columns 4-6).

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 21 to 27 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 (IV) estimates of the local average treatment effect at the threshold, and these are reported in the bottom panel of Table 2. Not surprisingly, given the almost exact compliance with program assignment, the ITT and IV estimates extremely similar. Being a *PANES* recipient increases support for the government by 21 to 28 percentage points. We strongly reject the hypothesis that government transfer income does not affect support for the government. Note that this effect is driven mainly by a shift among beneficiaries from indifference between the two parties to support for the government (not shown); there is a relatively little opposition support even among the ineligible (9 percent).

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 an average of US$880 per beneficiary household per year. This figure is an upper bound on transfers received 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=US$495.

Since *PANES* treatment increases political support by 0.21 to 0.28 (Table 2), the annual cost per additional government supporter is 495/0.28 = US$1,768 to 495/0.21 = US$2,357, assuming that the impact on other adults in the household is similar to that among survey respondents.

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 *PANES*, under the assumption that the survey responses translate directly into votes, and that the same treatment effect applies among all beneficiaries. Because 102,353 households were eventually admitted to the program (with 1.78 voting age adults per household), and using the conservative treatment effect estimate of 0.21, this gives a gain of 38,260 votes for the *Frente Amplio* relative to the opposition, implying that perhaps 19,130 voters would shift from supporting the opposition to supporting the FA. In the 2004 Uruguayan general the FA received 1,124,761 votes,[17] so this shift would be equivalent to an increase in the votes for the FA coalition of 1.7% (=19,130/1,124,761).[18] Since the program cost was roughly 1.95% of total government social expenditures,[19] increasing support for the government by 1 percentage point would cost roughly 0.9% of government social expenditures.

We estimate the cost of using a government transfer program to secure one additional political supporter to be approximately US$1,768 to 2,357 per year, or 32% to 43% 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, note that 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 up to twice our estimate.

The sample households may not be representative of the Uruguayan population as a whole: they have very low average monthly income (only US$81 at baseline) and are also aligned with the political left, as confirmed by the high degree of support for the government even among *PANES* ineligible households. We explore the sensitivity of responsiveness to the transfer across income levels and political orientation within our sample below.

III.b Validity of the regression discontinuity design

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[17] This is 50.04% of all votes cast. Turnout in the 2004 election was typically high for Uruguay, at 90% of all adults. (Source: University of the Republic, School of Social Science database 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.
[19] Government social expenditures are 21% of per capita GDP, the largest proportion in pensions and social security.

An alternative explanation for the patterns in Figures 1 and 2 is that assignment to *PANES* favored households with higher 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. Unfortunately, we lack data on baseline household political orientation, which prevents us from directly testing this alternative hypothesis; however, a variety of evidence makes it implausible.

Evidence in Figure 1 that virtually all eligible households received the program while nearly all ineligible households did not, suggests that blatant patronage is unlikely to have occurred. An alternative possibility is that the variables recorded in the baseline survey, and that determined the predicted income score for *PANES* eligibility, 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 predicted income score formula was developed by researchers at the University of the Republic and never publicly disclosed or directly shared with Ministry for Social Development officials during the program. An additional concern could arise if non-response rates (to either the survey or to the specific question about government support) were systematically related to program eligibility.

As a first check for non-random assignment around the eligibility threshold, we estimate equation 4 for multiple pre-treatment covariates as well as survey non-response in Table 3 (and present the results graphically in appendix Figure A1). 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. Focusing on our preferred specification with the linear polynomial control (column 2), we fail to find evidence of a discontinuity at the threshold for most household covariates, including: average household members' age and education (for individuals over 18), income, 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 results in Table 2 are almost unchanged when household controls are included (columns 4-6). Similarly, there is no evidence of a difference in voter turnout in the previous national election at the eligibility threshold: self-reported turnout in the previous national election was 93% for both eligible and ineligible households, in line with the consistently high turnout in Uruguay, where voting is mandatory.

As an additional check for manipulation around the eligibility score threshold, we non-parametrically present the distribution of the standardized score. If manipulation occurred so that some ineligible households were assigned a low predicted income 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, for the population of households (20,463) in the neighborhood of the threshold (-0.02, 0.02), computed with the full baseline sample. Following McCrary (2008a) we augment this graph with a local linear estimator of the density function on either side of the threshold. There is no indication of households just below the eligibility threshold being overrepresented relative to those just ineligible.[20] Manipulation of the eligibility score does not appear responsible for the 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 then briefly explore some other possible sources of heterogeneity.

We first split the household sample into 30 equally sized groups corresponding to baseline income, where each group contains roughly 70 household observations. Since reported 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.[21] The $R^2$ of the regression of baseline per capita income on the score is only 0.01 in our sample, leaving considerable variation at each predicted income score. Since the predicted income score was designed to capture permanent income, the residual variation in income at a given score can be thought of as temporary income shocks (e.g., due to job loss) as well as prediction and

[20] The point estimate of the log difference at the threshold in Figure 3 is just 0.041 (s.e. 0.027).

[21] 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 another.

measurement error. The extensive variation in reported income at each predicted income level allows us to estimate heterogeneous impacts across a wide range of income levels, a strength of our empirical setting.

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 point corresponds to the estimated fuzzy RD effect for each of the 30 income groups as a function of log baseline income (on the horizontal axis), 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. Regression is performed on the grouped data via GLS with weights equal to the number of observations in each cell.

The effect of *PANES* on political support falls with the level of pre-treatment income: the estimated coefficient is -0.238 (s.e. 0.138, Table 4) implying that a 10% increase in baseline income reduces the gain in government support due to the program by 2.4 percentage points. While at the lowest of the observed household per capita incomes in our sample the estimated coefficient on receiving *PANES* is nearly 0.5, towards the upper end – which corresponds roughly to the national poverty line – it falls close to zero. These estimates are likely to be a lower bound on the true income effect, since household income is likely to be somewhat mis-measured for a poor population with considerable informal sector and self-employment, leading to attenuation bias (although it is difficult to quantify the extent of this bias in our data).

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 interactions with gender), years of education and its square, an indicator for homeownership, and indicators for geographic *departamentos*.[22] The probability of voting for the FA increases with age, peaking at around age 40 and then declining (appendix Table A2), while

[22] There is evidence that political support expressed in surveys lines up closely with actual votes: the correlation coefficient across Uruguayan *departamentos* between support for the Frente Amplio in the 2004 *Latinobarómetro* survey and their actual election vote share was very high, at 0.85.

education is positively associated with being left-leaning, and gender differences appear minor. There are large and significant differences across *departamentos*, and predicted support ranges widely, between roughly 20% and 80%. We use this model to predict pre-program political orientations for sample households, using the same covariates available in the *PANES* baseline survey. Then using a procedure analogous to that used across income groups, we estimate heterogeneous effects of *PANES* treatment across individuals with different predicted pre-program political support for the government.

Panel B of Figure 4 shows that the effect of *PANES* varies considerably with respect to predicted political affinity. Voters 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. The effect is small and close to zero for voters with very high propensity to vote for the FA, then moving to the right on the horizontal axis it rises for groups with similar probabilities of voting for either the FA or the center-right coalition, and then declines again for voters who seem strongly aligned with the opposition. In the figure we report a best fit quadratic regression plot, together with 95% confidence intervals. The estimated coefficients in Table 4 (panel B) imply that the influence of *PANES* transfers peaks at a 44% 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 ("*On a scale from 0 to 10, where 0 is left and 10 is right, where would you locate?*", results not shown).

A leading question is why conditional cash transfer programs so often designate women as the transfer recipient (Rawlings and Rubio, 2005). Although this is generally justified with an aim of empowering women and improving child wellbeing, if (as often argued) resources given to women are more likely to be spent on children (Adato et al., 2000), electoral considerations are an alternative explanation. We find that Uruguayan female headed households are no more responsive to cash transfers than other households in our sample (not shown). If the same gender pattern were to hold in Mexico and other countries with large cash transfer programs, this would suggest that electoral considerations alone are not driving the decision to target women. We examined heterogeneous treatment effects along other dimensions, but while older individuals and those living in Montevideo are marginally less responsive to the transfer in some specifications, these effects are generally not statistically significant (results not shown).

We also examined whether there were differential treatment effects using variation in the per capita *PANES* transfer generated by household size. However, due to the fact that the food card transfer increases with the number of children in the household, and larger households are also more likely to receive additional benefits from smaller program components, there is insufficient variation in per capita transfers to draw firm conclusions (results not shown).

III.d Income and labor market impacts and other channels explaining 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 in regressions in Table 5, row 1. Note that per capita income grows by a remarkable 56% even for *PANES* ineligible households, presumably due to Uruguay's rapid macroeconomic recovery after 2004, although mean reversion could also be playing a role for some households. Income growth among *PANES* eligible households is even faster, at 78%, and the estimated regression impact at the threshold is 25% (s.e. 0.073) in our preferred column 2 specification with the linear polynomial controls. This is on the order of what would be expected in the absence of offsetting behavioral responses to the transfer.[23]

Consistent with the lack of offsetting behavioral effects, row 2 of Table 5 shows no effect of the program on labor supply as measured by hours of work (with zeros for those not in work), coefficient estimate 1.811 hours (standard error 1.495). While the income transfer alone might have depressed household labor supply due to an income effect, other *PANES* components (e.g., job training and public works employment) likely acted in the opposite direction, and these two effects appear to have roughly cancelled, leading to no discernible program effect on work hours. Although this limited adult labor supply response is consistent with results from Mexico's similar *Progresa* program (Parker and Skoufias, 2000), the finding is in contrast to recent work by Card et al. (2007), who show excess sensitivity of job search behavior to cash-in-hand. We also find some modest and only marginally statistically significant positive effects of the

[23] Household income in the follow-up survey among ineligible households was US$142. The implied increase due to the transfer is on the order of 33 log points (=log (1 + 56/142))

program (not reported) on 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), perhaps due to the conditions officially attached to program receipt, which may have swayed some households. However, there is no evidence of impacts on durables ownership, home characteristics or self-reported health (Amarante et al., 2008).[24]

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 also experienced an improvement in their self-esteem and psychic 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-scale 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 -0.29, implying that respondents regard their current situation as being rather bad. However, this assessment is 0.31 points higher among *PANES* eligible respondents, and the difference is very precisely estimated (s.e. 0.087, Table 5, row 3, column 2). 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" relative to ineligible households (not shown). Results are quite robust across 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 national economic conditions, an issue that has found widespread support in behavioral economics in recent years (see Simonsohn et al 2008 for one example). Panel D in Figure 5 and the bottom row of Table 5 report households' satisfaction with the country's current situation, using the

[24] Although there is no detailed consumption or savings information in the survey, treated households declare having spent the transfer primarily on food and clothes (71%), to pay utility bills (10%) and to repay debts or loans (10%).

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). There is limited support for this conjecture: *PANES* eligible households express a somewhat more positive assessment of Uruguay's current situation than the ineligible but the estimate is not statistically significant in our preferred specification, at 0.097 (s.e. 0.086, column 2). We present further evidence on channels below in our discussion of the second follow-up survey round.

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 the ineligible are also bitter at their exclusion, in which case the estimates are a combination of two distinct effects. Although there is no direct way to measure these effects since we lack data 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 of the government administration headed by the President: 1 Approves, 2: Disapproves, 3: Does not know/does not respond*", which we again code up as a support gap for the government, as above. We use a multinomial logit on the same covariates as those in Table A2 plus a linear time trend to predict the support for the current and the opposition government in the 2005 and 2006 *Latinobarómetro*, and use the predictions of this model in 2007 to derive counterfactual support for the current government among households in our sample.

Figure 6 reports predicted government support as a function of the normalized income score, as well as the level of support in the follow-up survey (as in Figure 2). The predicted support for the government is remarkably similar to the follow-up survey among ineligible households (to the right of the discontinuity), evidence against the embitterment hypothesis.

III.f. Persistent impacts: the 2008 post-program survey round

A second follow-up household survey round was collected in February and March 2008, after the temporary *PANES* program had already ended. Attrition is a minor concern, as 92% of households from the first follow-up round were successfully re-surveyed. Yet despite the time that had elapsed since the cut-off of *PANES* transfers in late 2007, the impact of receiving *PANES* on government support remains large and statistically significant, at over 20 percentage points (figure 7). The *PANES* cash transfer program we study thus had persistent impacts on political support for the government, suggesting that lagged transfers also factor meaningfully into voters' decision-making. These voting effects of lagged transfers could greatly reduce the cost per vote gained through a government program if they persist through several election cycles, although we cannot accurately assess the degree of persistence given our single post-program follow-up survey.

The follow-up survey also contains detailed information on respondent views towards *PANES* as well as five other government policy reforms. The discontinuity in support for *PANES* remains large and statistically significant (figure 8, panel A), perhaps as expected. However, support among *PANES* beneficiaries for five other FA government initiatives – pension reform (panel B), health care reform (panel C), the *plan de equidad* (a newer anti-poverty program that was less generous and more broadly targeted than *PANES*, covering both *PANES* eligible and ineligible households, panel D), income tax reform (panel E), and wage council reform (panel F) – are nearly identical among *PANES* eligible and ineligible households. This suggests a fair degree of political sophistication among these voters, helping rule out a particularly naïve form of survey bias, where beneficiaries simply say that all government policies are "good"; and highlights that it is in fact the *PANES* cash transfer program that is responsible for growing pro-government sentiment among beneficiaries.

## 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 21 to 28 percentage points. We also find pronounced heterogeneity across income groups and those with different political orientations, in line with the predictions of the theory. In particular, the same nominal cash transfer has a larger impact 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 most responsive to government transfers provides strong empirical support for the logic of targeting "swing voters" for redistribution.

We estimate that the cost to the government of obtaining an additional vote through the cash transfer program was approximately US$1,768 to 2,357 (32% to 43% of annual per capita income). Yet there are several reasons to take these "cost per vote" figures with caution. First, given the research design, it is impossible to know how different the vote gains for the government would have been had the transfer amount been smaller (or larger). A more intricate 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 support. Persistent impacts of the program on pro-government views across election cycles would also substantially reduce this cost figure.

Second, it is difficult to extrapolate these results to the case where a right-wing party would have 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. Finally, we estimate a local treatment effect in this paper at the program eligibility threshold, and thus extrapolating treatment effects to other populations requires stronger assumptions. We cannot rule out the possibility that the government lost some votes among better-off voters who had to pay for the policy though higher taxes, offsetting the vote gains we document among the poor; our dataset and research design does not allow us to measure any such effects. Another important validity issue is how likely these results are to generalize to other settings. While Uruguay is a middle income country, it has well-developed democratic institutions and a long tradition of strong political parties, suggesting that the findings of this paper are relevant not only for Latin America but also possibly for wealthier countries with similarly strong political institutions.

With these caveats in mind, this paper indicates that government economic policies can have large impacts on political and social attitudes (see DiTella et al 2007 for a related result from Argentina). The heterogeneous responses to the transfer that we find suggest that shrewd vote-maximizing politicians will carefully select which populations will benefit from government programs. In fact, in Uruguay the poverty score threshold for the *PANES* program varied slightly across the country's five regions, with the program being somewhat more generous in the interior of the country where baseline support for the *Frente Amplio* government was lower. While we should be cautious about over-interpreting a result based on only five regions, and have no direct evidence that blatant political considerations directly entered into the setting of the eligibility thresholds, this pattern is consistent with the government choosing to deliberately target more program resources to "swing voters" in the interior and away from their "core supporters" in the capital of Montevideo, a reasonable political strategy given our findings.

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## Appendix: *PANES* program components

The table below presents the probability of ever having received each separate component of the *PANES* program as reported by respondents in the first follow-up survey. The first row 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 (17%) benefited from public works employment, as shown in row 3.

Row 2 reports the proportion of households receiving the food card (*tarjeta alimentaria*). 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 67% of eligible households while participation among ineligibles was close to zero.

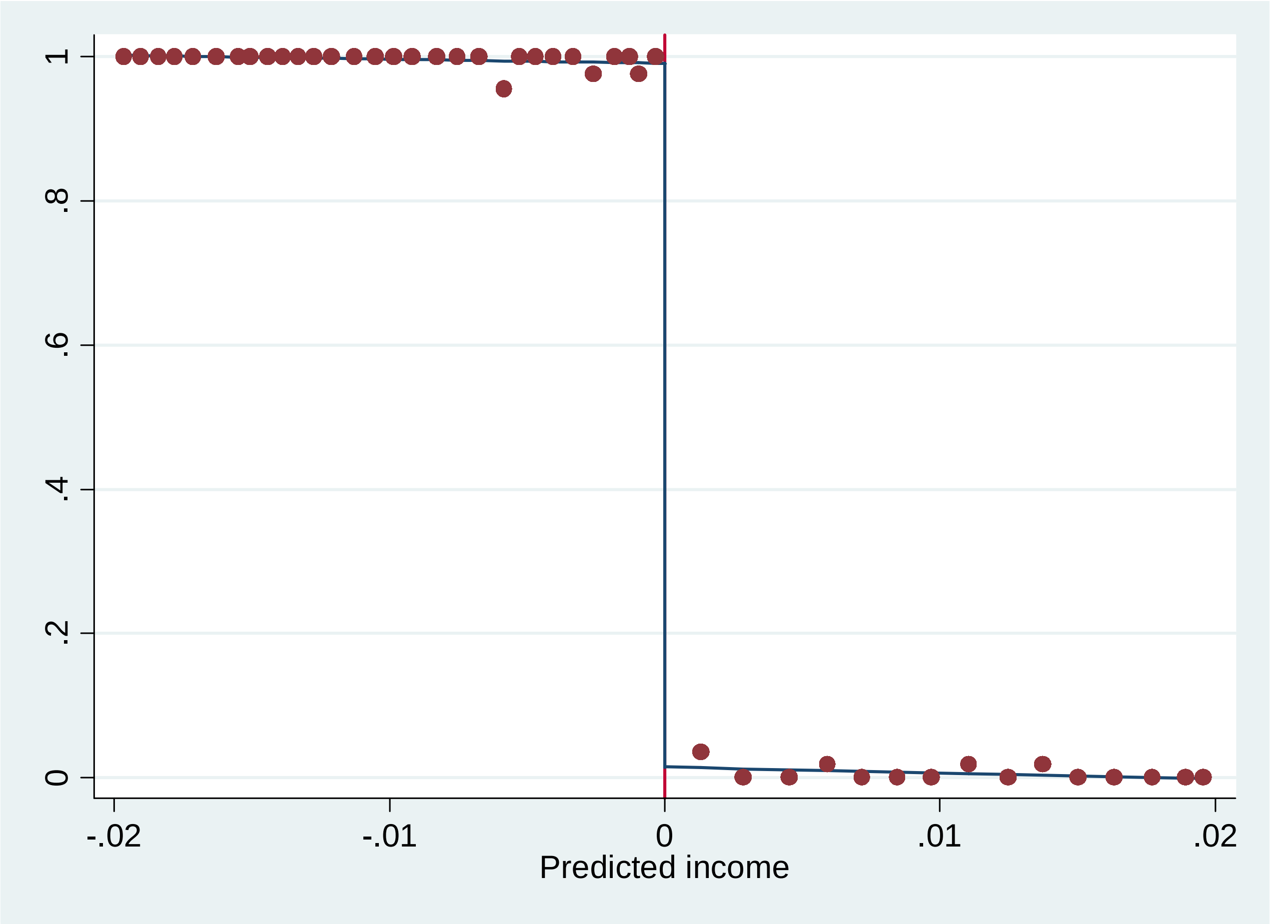
Around 15% of eligible households reported having participated in a job training program (*rutas de salida*). These were programs of six months duration implemented by NGOs, neighborhood commissions, and political and trade union organizations for groups of up to 25 participants. While participation 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 row 4 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 row of the table. 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 21% 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 improved access to the public health sector.

Appendix Table A1: Self-reported *PANES* take-up among beneficiaries, by component (%)

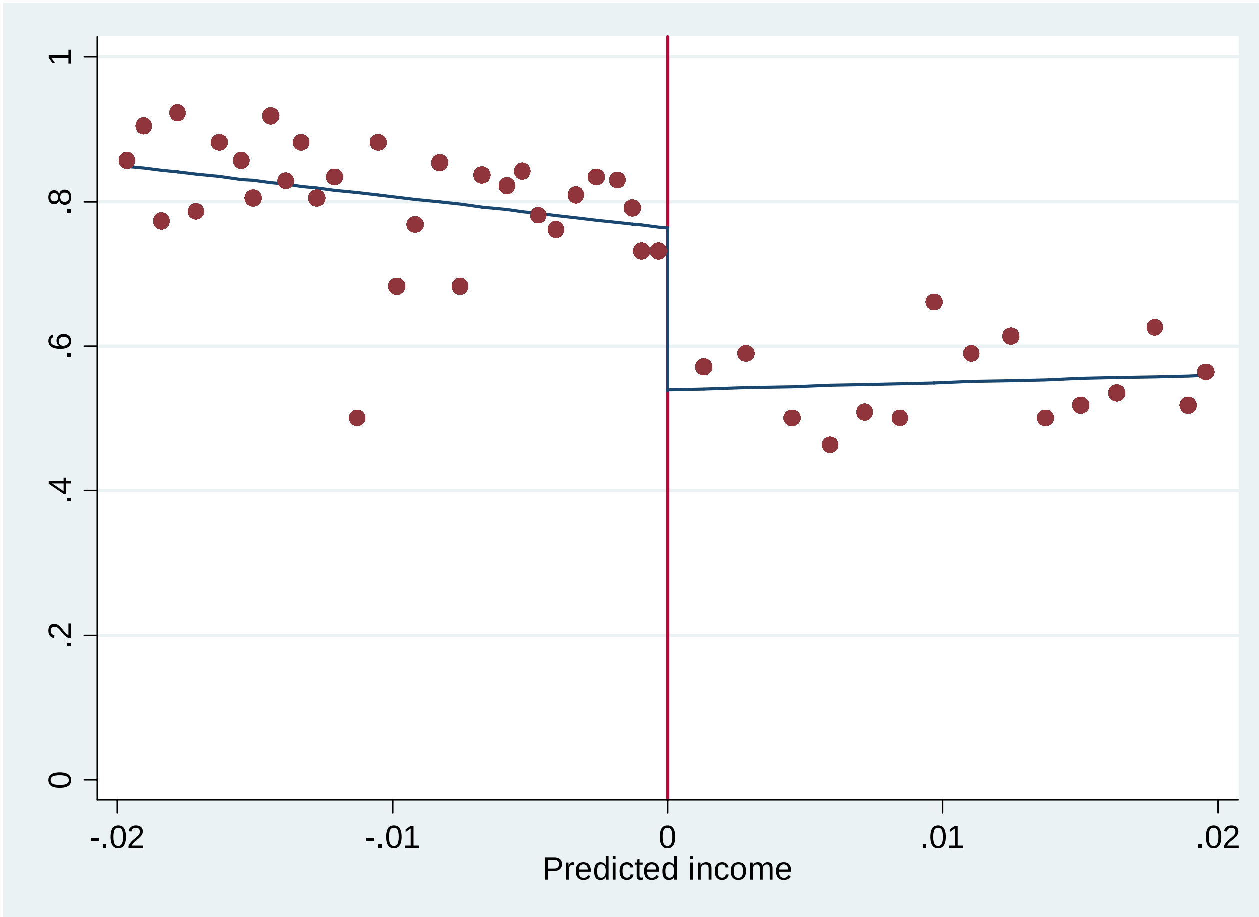
| | |
|---|---|
| Citizen Income | 97.6 |
| Food card | 66.9 |
| Public works employment | 17.0 |
| Job training | 15.1 |
| Other components | 21.3 |

Figure 1: *PANES* program eligibility and participation



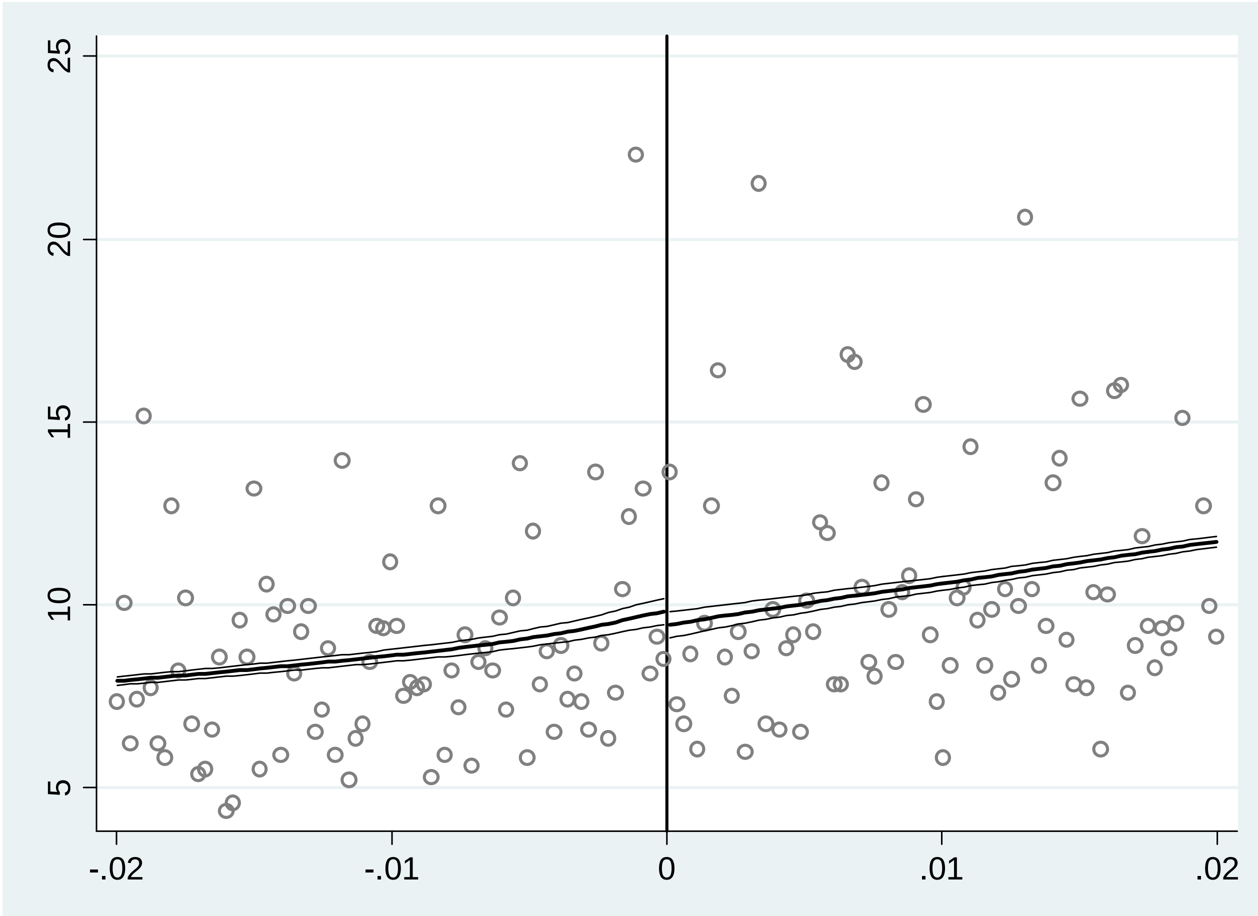

Notes. The picture reports the proportion of households ever enrolled in *PANES* as a function of the standardized score. The fitted plots are linear best fits on each side of the eligibility threshold.

Figure 2: Program eligibility and political support for the government




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.

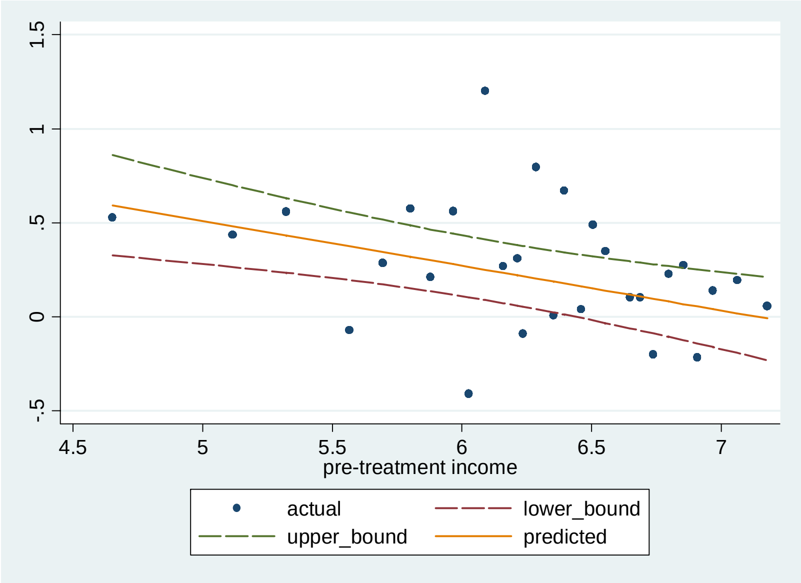
Figure 3: Distribution of the standardized *PANES* eligibility score



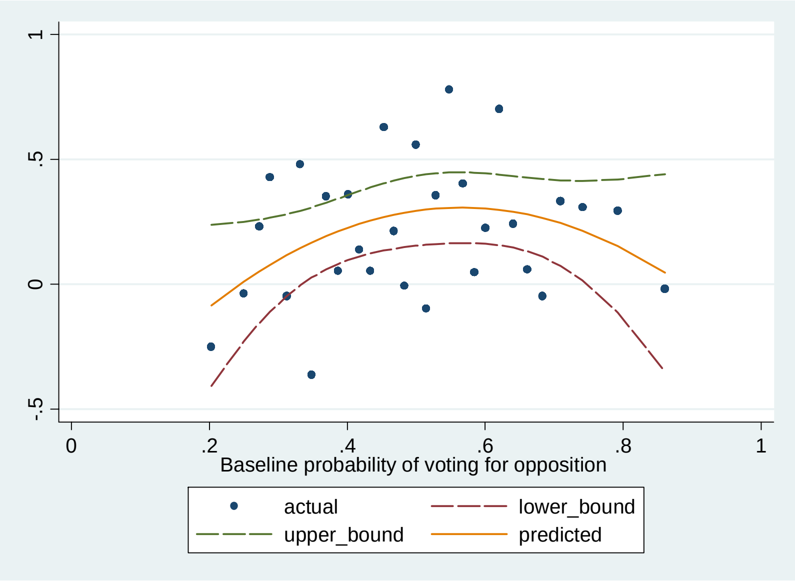

Notes. The graph reports the distribution of the standardized eligibility score for the universe of applicant households in the neighborhood of the discontinuity point (following McCrary 2008a).

Figure 4: Program participation and political support for the government, heterogeneous effects

Panel A: Treatment effect by baseline household per capita income



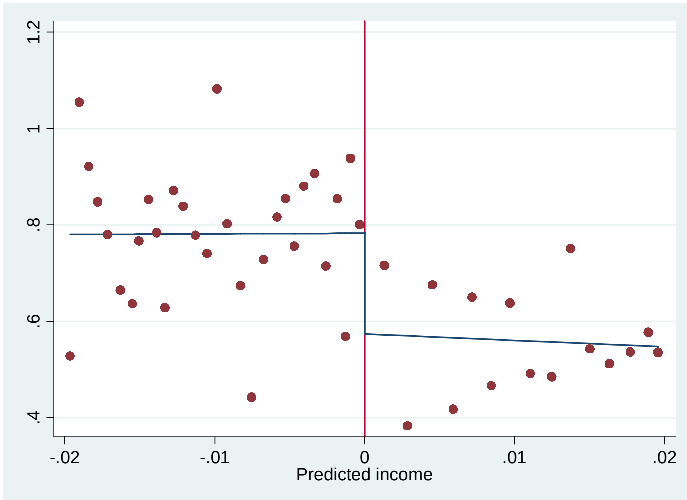

Panel B: Treatment effect by predicted baseline support for the opposition



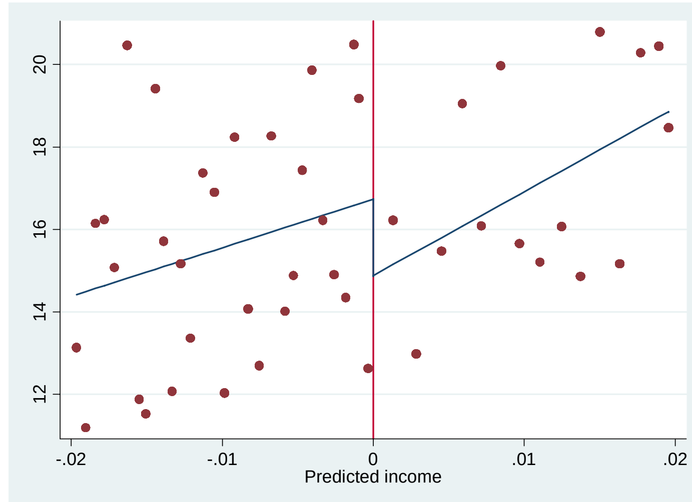

Notes. The left hand side panel reports fuzzy RD estimates of the effect of treatment on support for 30 bins of the pre-treatment income distribution and the best-fit linear regression (with associated confidence interval around the discontinuity point). The right hand side panel reports the same regression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

Figure 5: Program eligibility, household welfare and satisfaction

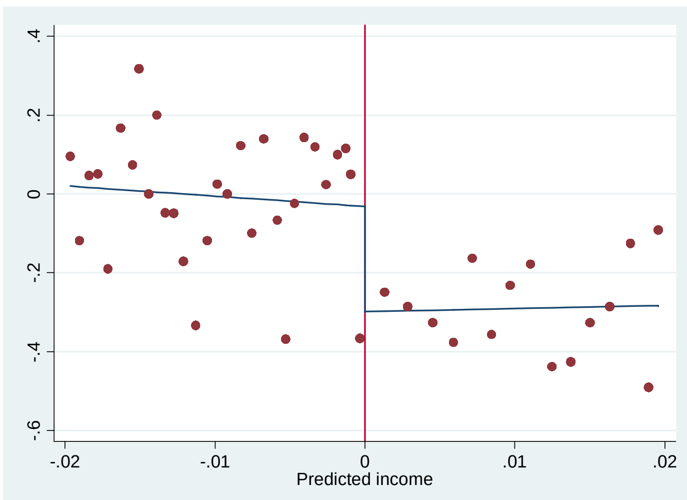
Panel A: Growth in household per capita income



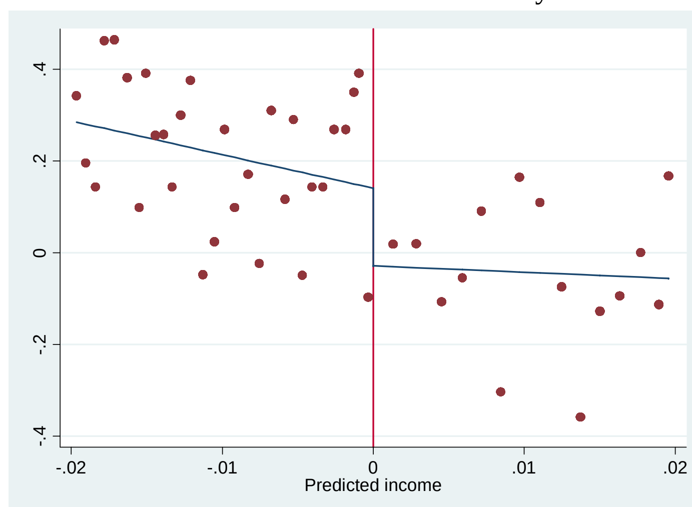

Panel B: Average weekly hours of work




Panel C: Satisfaction with current household situation

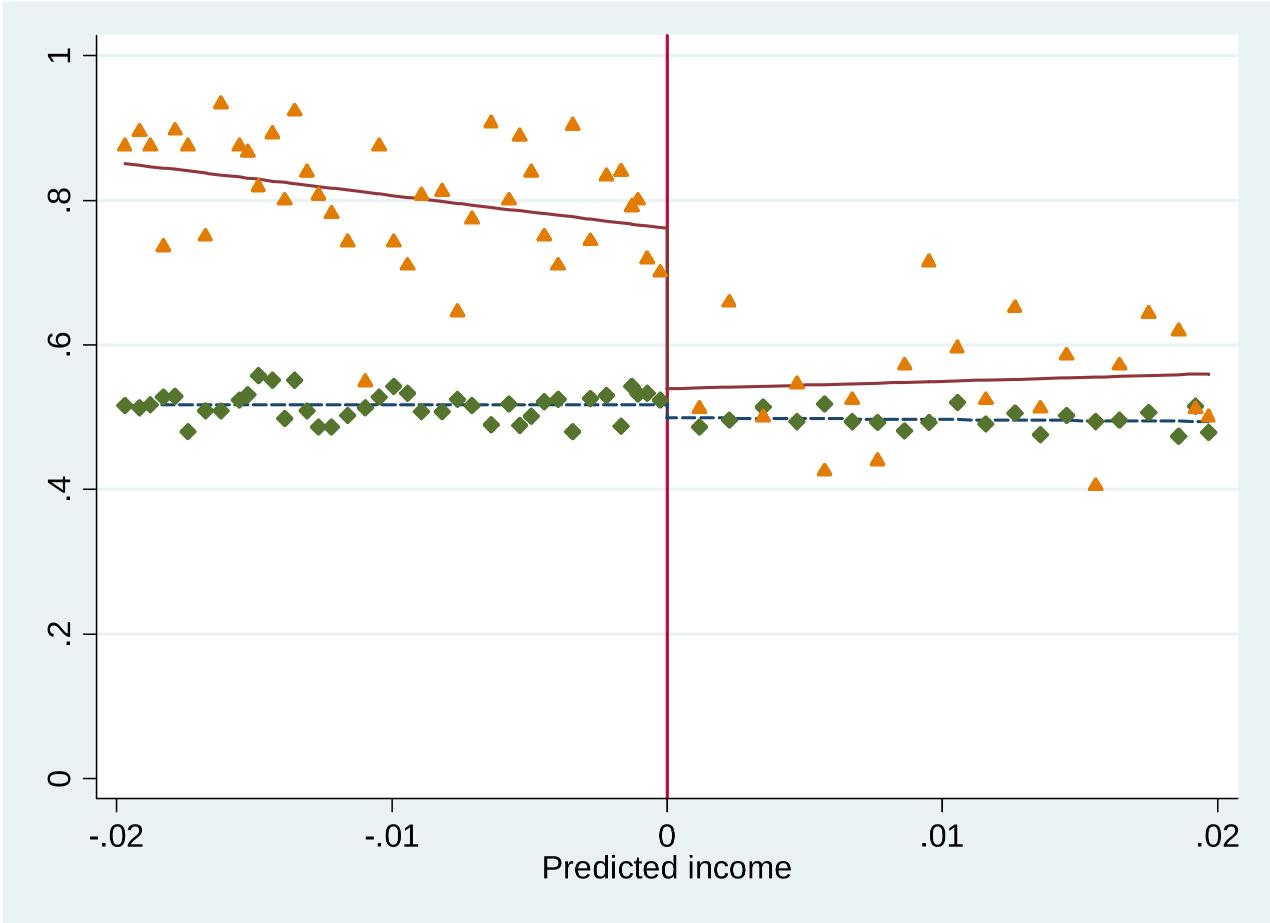



Panel D: Satisfaction with current country situation



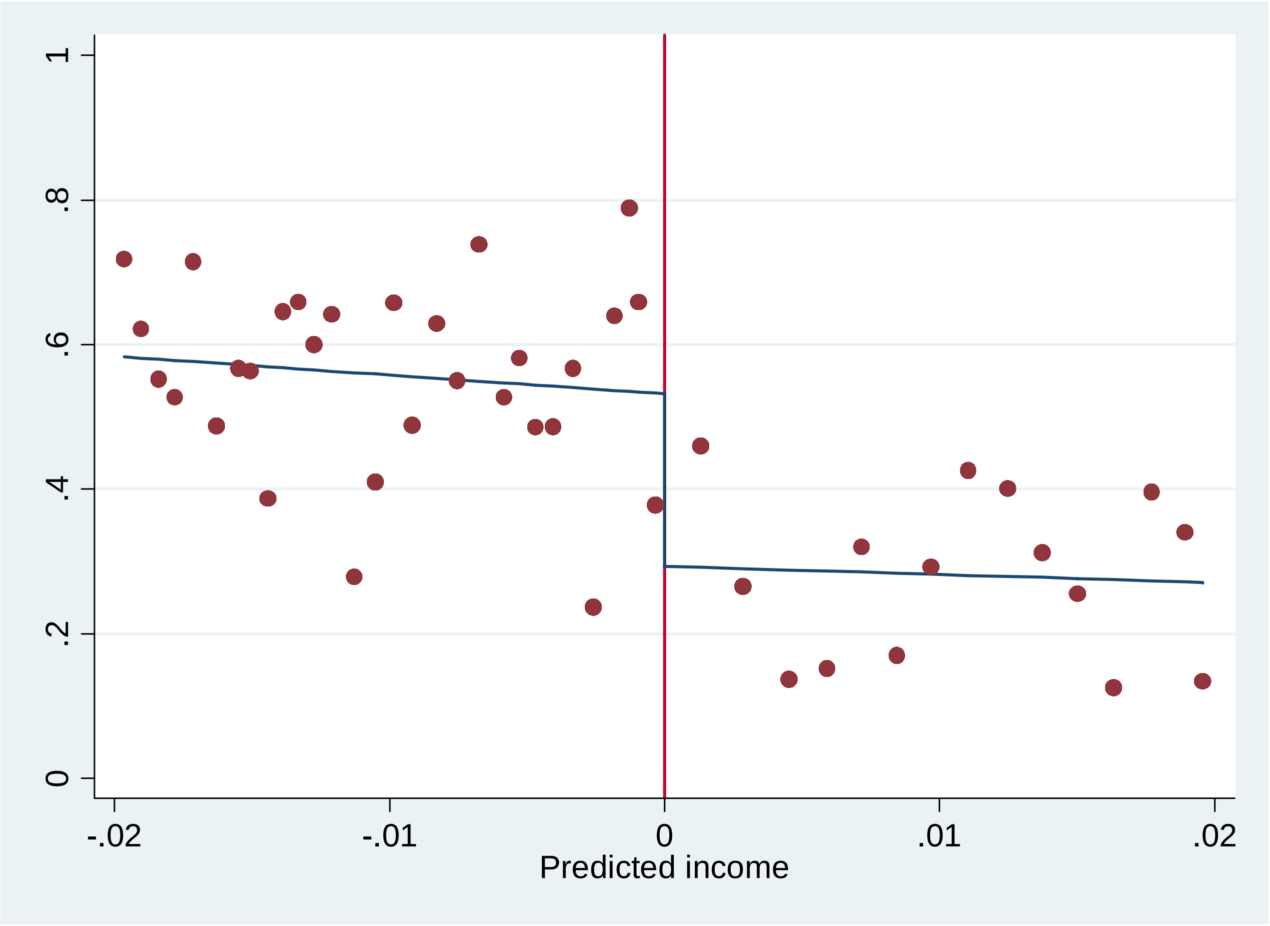

Notes. Panel A reports growth in income between baseline and the follow-up survey. Panels B and C report the respondents' assessment of - respectively - the current household's and country's situation. Panel D reports the household's average total hours of work (for individuals aged 14-75). See also notes to Figure 1.

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



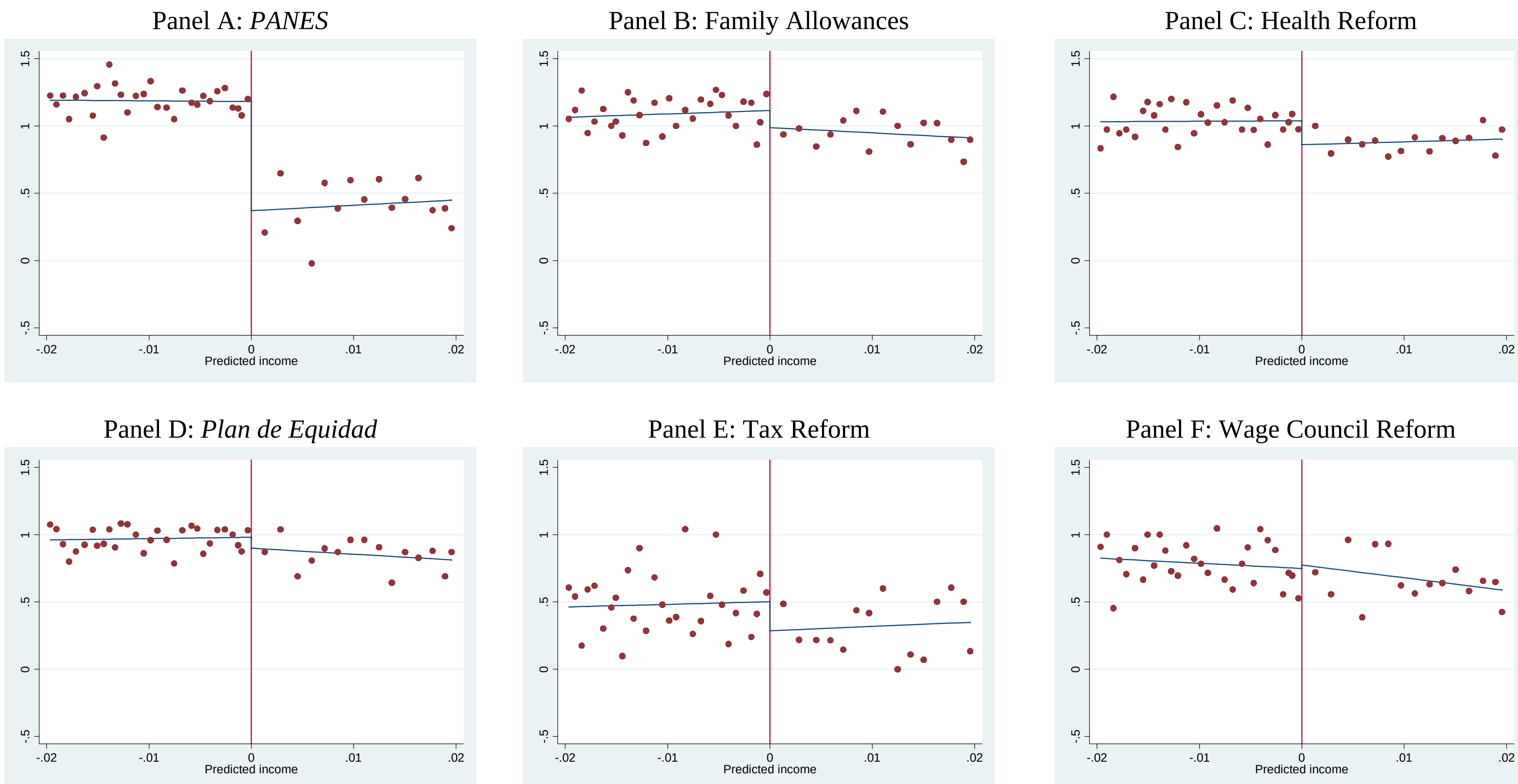

Notes. The figure reports the proportion of households favoring the current government minus those favoring the previous government (triangles / solid line) in the first follow-up survey and those predicted to approve of the current government minus those predicted to disapprove using the *Latinobarómetro*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

Figure 7: Program eligibility and political support for the government, 2008 follow-up survey round




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: the second *PANES* follow-up survey (2008). The fitted plots are linear best fits on each side of the eligibility threshold.

Figure 8: Support for *PANES* and other governement reforms: 2008 follow-up survey round




Notes. The figure reports the average support (on a scale -2 to 2) for a number of government reforms. Source: the second *PANES* follow-up survey (2008).

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

| | UNDP *Human Development Report 2007* | | | | *The Economist* Intelligence Unit 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) |
|---|---|---|---|---|---|---|
| Panel A: | First stage: Ever received *PANES* (dep. var.) | | | | | |
| Program eligibility | 0.991*** | 0.976*** | 0.964*** | 0.991*** | 0.977*** | 0.964*** |
| | (0.003) | (0.010) | (0.021) | (0.003) | (0.010) | (0.024) |
| Panel B: | Reduced form: Government support (dep. var.) | | | | | |
| Program eligibility | 0.256*** | 0.223*** | 0.249*** | 0.231*** | 0.209*** | 0.269*** |
| | (0.026) | (0.054) | (0.087) | (0.028) | (0.056) | (0.090) |
| Panel C: | IV: Government support (dep. var.) | | | | | |
| Ever received *PANES* | 0.258*** | 0.229*** | 0.258*** | 0.234*** | 0.214*** | 0.279*** |
| | (0.026) | (0.055) | (0.089) | (0.028) | (0.057) | (0.093) |
| Score controls | None | Linear | Quadratic | None | Linear | Quadratic |
| Other controls | No | No | No | 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 3 include, in order, a polynomial in the standardized score of degree 0, 1 and 2, and these polynomials interacted with the eligibility indicator. Columns 4 to 6 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 3: 2,098; in columns 4 to 6: 1,987. 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). Statistically significant at 90% (*), 95% (**), and 99% (***) confidence.

Table 3: Program eligibility and pre-treatment characteristics, reduced form estimates

| Dependent variable: | (1) | (2) | (3) |
|---|---|---|---|
| Log per-capita income at baseline | -0.046* | 0.002 | 0.011 |
| | (0.027) | (0.057) | (0.093) |
| Average years of education at baseline | 0.056 | -0.046 | -0.216 |
| | (0.101) | (0.208) | (0.308) |
| Household size at baseline | 0.303*** | -0.296 | -0.599* |
| | (0.116) | (0.244) | (0.359) |
| Average age at baseline | -3.928*** | -0.826 | -2.104 |
| | (1.087) | (2.170) | (3.173) |
| Beneficiary female | 0.077*** | -0.020 | -0.037 |
| | (0.029) | (0.058) | (0.090) |
| Beneficiary years of education | 0.185 | 0.107 | 0.279 |
| | (0.150) | (0.306) | (0.445) |
| Beneficiary age | -2.449*** | -0.599 | -2.138 |
| | (0.795) | (1.565) | (2.363) |
| Survey non-response rate | -0.011 | 0.047 | 0.026 |
| | (0.018) | (0.037) | (0.057) |
| Voted in 2004 elections | -0.002 | 0.021 | 0.037 |
| | (0.012) | (0.025) | (0.044) |
| Score controls | None | Linear | Quadratic |

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 is 2,089, except for survey non-response rate, where it is (3,085).

Table 4: Program participation and political support for the government, heterogeneous effects

| | |
|---|---|
| Panel A: RD estimates by household pre-treatment income | |
| Log pre-treatment household income | -0.238* |
| | (0.138) |
| Panel B: RD estimates by predicted respondent political orientation | |
| Predicted likelihood of voting for the opposition 2001-04 | 3.366** |
| | (1.640) |
| (Predicted likelihood of voting for the opposition 2001-04) $^2$ | -2.979* |
| | (1.560) |

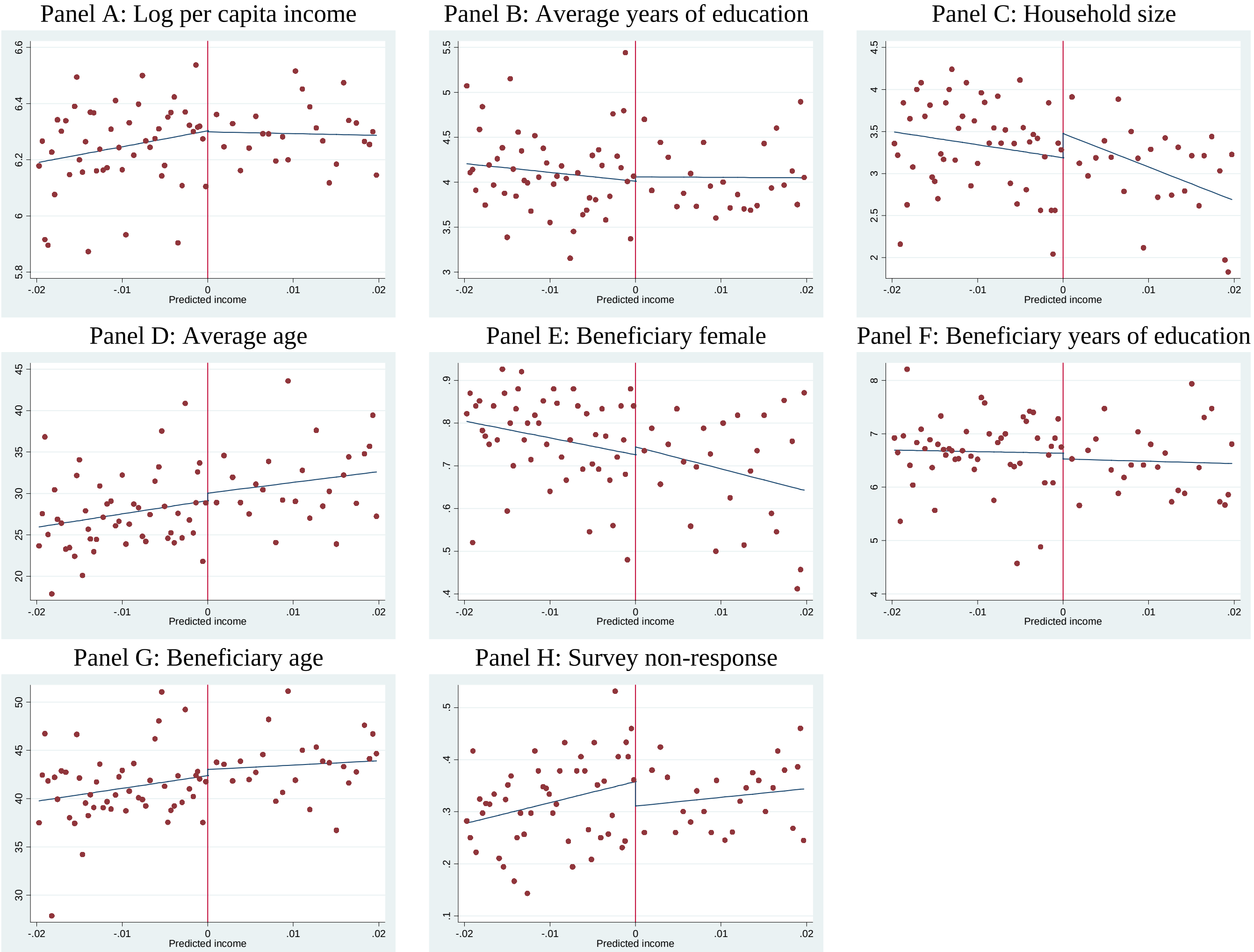
Notes. The table reports the estimated effect of program participation on support for the government, as a function of by pre-treatment income (panel A) and by predicted level of support for the opposition coalition (panel B). Regressions performed by GLS with weights equal to the sample size by cell. Number of observations: 30. See text for details.

Table 5: Program eligibility and additional outcomes, reduced form estimates

| Dependent variable: | (1) | (2) | (3) |
|---|---|---|---|
| Per capita income growth | 0.221*** | 0.251*** | 0.188 |
| | (0.036) | (0.073) | (0.120) |
| Household average weekly hours of work | -1.659** | 1.811 | 0.254 |
| | (0.754) | (1.495) | (2.337) |
| Satisfaction with household situation | 0.291*** | 0.312*** | 0.266** |
| | (0.041) | (0.087) | (0.134) |
| Satisfaction with country situation | 0.246*** | 0.097 | 0.043 |
| | (0.041) | (0.086) | (0.138) |
| Score controls | None | Linear | Quadratic |
| Other controls | Yes | Yes | Yes |

Notes. The table reports results from regressions of various outcomes on the program eligibility indicator. Regressions include other controls as in columns 4 to 6 of Table 2. See also notes to Table 2.

Figure A1. 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.

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Table A2: 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) |
| *Departamento* (state) 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.