

Government Transfers and Political Support

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Abstract: In this article we estimate the impact of a large anti-poverty cash transfer program, the Uruguayan *PANES*, on political support for the government that implemented it. Using the discontinuity in program assignment based on a pre-treatment eligibility score, we find that beneficiary households are 11 to 13 percentage points more likely to favor the current government relative to the previous government. Political support effects persist after the program ends. Our results are consistent with theories of rational but poorly informed voters who use policy to infer politicians' redistributive preferences or competence, as well as with behavioral economics explanations grounded in reciprocity.

Keywords: Cash transfers, redistributive politics, voting, regression discontinuity.

JEL codes: H53, D72.

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This paper analyzes the effect of a large anti-poverty program, the Uruguayan *Plan de Atención Nacional a la Emergencia Social (PANES)*, on expressed support for the government. We exploit the quasi-random assignment of applicants to the program based on a sharp discontinuity in a predicted income score in order to identify the effect of receiving transfers on support for the incumbent government, and to ultimately advance understanding of voter decision-making.

The interaction between government policies and voters' choices is central to debates in political economy, social choice and public economics. The notion that voters respond to policy outcomes underpins the theory of democracy, by creating a mechanism for government accountability. Since the work of Kramer (1971), Nordhaus (1975), Fair (1978) and Fiorina (1981), many scholars have documented voters' responsiveness to macroeconomic conditions and policies. A robust empirical finding is that aggregate economic conditions around election time have predictive power for incumbents' re-election success. Yet, the existing empirical work faces obvious econometric concerns, as it typically relies on aggregate data with few observations and, most importantly, rarely relies on exogenous sources of policy variation.

Even less is known about the effect of household specific economic circumstances, and in particular targeted government transfers, on voters' choices. While it is conventional wisdom that government transfers sway votes, and thus could be used strategically by incumbents seeking re-election, there remains little convincing evidence on the magnitude of these effects. Beyond immediate concerns about the existence of suitable individual-level data combining political preferences with government transfer receipt, omitted variables and reverse causality are likely. For instance, if targeting political "core supporters" is more effective, as predicted by some pre-electoral competition models (Lindbeck and Weibull, 1987; Cox and McCubbins; 1984, Verdier and Snyder, 2002), a positive correlation between transfer receipt and political support does not imply causality, since parties are making tactical decisions about precisely which groups will respond most to transfers. A related difficulty arises if certain social groups, such as the poor, are more likely to benefit from transfers while at the same time displaying partisan political preferences, in particular for left-wing parties that favor redistribution.¹

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 voters being targeted (Case, 2001, Schady, 2000, Green 2006b). There is also evidence of direct vote buying in Latin America, including Schaffer (2007) and Stokes (2005).

Indeed, while some observational and quasi-experimental studies that attempt to address causality find an effect of targeted transfers on individual voting intentions and behavior (Markus, 1988, Chen, 2008a, 2008b; Elinder et al., 2008; Levitt and Snyder, 1997), with Pop-Eleches and Pop-Eleches (2009) the closest to the current paper in terms of its research design, this body evidence is not uncontested (Green, 2006a). Just as importantly, little is known about the mechanisms that underpin the exchange of votes for transfers between voters and politicians, especially since the secrecy of the ballot in modern democracies makes “vote-swaying” through targeted government transfers difficult to enforce due to inter-temporal commitment problems.

To contribute to understanding of voter decision-making, in this paper we measure the extent of voters’ responsiveness to targeted public transfers by exploiting the quasi-random targeting of the Uruguayan *PANES* - a large temporary anti-poverty program - together with individual survey data on expressed support for the incumbent government. *PANES* was launched by a newly elected center-left government in response to an major economic crisis, and the program lasted from April 2005 to December 2007. Program eligibility was determined by a predicted income score based on a large number of pre-treatment covariates and only households with scores below a predetermined threshold were eligible. This targeting rule was designed both to prevent discretion in program assignment as well as for the purpose of rigorous impact evaluation. To avoid manipulation, the predicted income score formula was not disclosed to recipients, enumerators or government bureaucrats until the program had ended. Because the targeting rule was thus insulated from political considerations, and its implementation was remarkably strict, assignment to the program near the threshold is “as good as random”.

Around 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. A second similar follow-up survey took place in 2008 after the program had already ended. The quasi-random assignment of applicants in the neighborhood of the threshold allows us to circumvent the problems of reverse causality, endogenous political targeting, and omitted variables highlighted above and thus credibly estimate the impact of transfers on support for the incumbent. To preview our main findings, individuals who received the transfer were much more likely to support the incumbent, by 11 to 13 percentage points, relative to those who barely failed to qualify for the transfer. While Uruguay is a middle income country, it has well-developed democratic institutions (Table A1)

and a long tradition of strong political parties, suggesting that the main findings might be relevant not only for Latin America but also possibly for wealthier countries with similarly capable political institutions.

Beyond estimating the impact of government transfers on voters' political support, the findings also allow us to test – and reject – two standard theories of voter behavior, and instead point to alternative theoretical frameworks that are more empirically grounded.

A prominent older tradition in political economy emphasizes the centrality of voters' contemporaneous real disposable income as the key driver of voting outcomes, in what is often called *pocketbook voting* (Kramer, 1971; Hibbs, 1982). Although this framework yields predictions that are consistent with observed correlations, it has been criticized as simplistic and lacking in theoretical grounding, since it implies that strategic governments could systematically “fool” voters by adopting expansionary policies on the eve of re-election, even if these policies impose later costs on society, which rational voters should anticipate and dismiss (Stigler, 1973).

Using data collected after the *PANES* program had ended, we find that political support for the incumbent government that created the program remains significantly higher among former program beneficiaries, despite the fact that their income levels quickly fell back down to the same level as non-beneficiaries. Since the simplest form of myopic pocketbook voting implies that beneficiaries and non-beneficiaries should have equal levels of support for the incumbent party once their incomes are equalized, our findings are inconsistent with this model.

Precisely because of the widespread intellectual dissatisfaction with such naïve models of behavior, modern political economy models assume rational voters, consistent with the standard economic approach to decision-making (see for example Ferejohn, 1986; Persson and Tabellini, 2002; Besley, 2006). Under asymmetric information about politician characteristics, voters use policy outcomes as signals to infer politicians' competence (as in Rogoff, 1990) or their preferences for redistribution towards particular social groups (as in Drazen and Eslava's 2006 model of targeted transfers).

Beyond rejecting the simple pocketbook voting model, our empirical results also argue against these more sophisticated theories of *rational and well informed voters* who are able to observe policy signals and reverse engineer them to infer politician or party characteristics. Given the quasi-random targeting of the *PANES* transfer near the eligibility threshold, rational voters on both sides of the threshold should hold the same views of the incumbent's competence

and redistributive preferences, and hence their support for the incumbent should not be affected by their own personal transfer receipt. Yet precisely because their support for the incumbent is so strongly affected by past transfers, alternative theoretical explanations are needed.

One obvious alternative is a model with *rational but poorly informed* voters who are unaware of the quasi-random nature of the *PANES* targeting rule and (incorrectly) use their past personal program receipt as a signal of the government's redistributive preferences towards "people like them", or perhaps of its ability to successfully follow through on its electoral promises. This interpretation squares well with empirical evidence from the U.S. that voters are only partially able to extract a signal of incumbent ability based on local economic outcomes (Wolfers, 2009), and with their tendency to punish incumbents for natural disasters (Cole et al., 2008) and even shark attacks (Achen and Bartels, 2004), events that well-informed voters would presumably recognize are outside politicians' control.

We also emphasize, however, that the persistent post-program support for the incumbent we observe among transfer beneficiaries is equally consistent with models of *reciprocity* in which voters support politicians who have favored them in the past. Issues of reciprocity, fairness and gratitude (Rabin, 1993; Cox et al., 2007) that are empirically relevant in real-world labor market situations (Gneezy and List, 2006; Kube et al., 2007; DellaVigna, 2009) have recently been shown to play a role in voters' political decision making (Finan and Schechter, 2010). In the conclusion, we discuss a research design that might allow future empirical work to distinguish between the competing theoretical explanations for our findings.

The paper proceeds as follows. Section I presents details of the *PANES* program and the data. Section II investigates the effect of the transfer program on political support for the government, and section III provides insight into the channels behind the increase in political support. The final section concludes.

I. THE *PANES* PROGRAM IN URUGUAY

Uruguay, a small Latin American country with 3.3 million inhabitants, experienced rapid economic growth in the first decades of the twentieth century, and was among the first countries in the region to implement universal primary education and establish a generous old age pension system. Although Uruguay is still among the most developed Latin American countries according to the UNDP Human Development Index, with high life expectancy and schooling

indicators (Appendix Table A1),² economic growth stagnated in the second half of the twentieth century. Currently, PPP-adjusted annual per capita income is just below US\$10,000.

The country experienced a severe economic crisis in the early 2000s.³ Yet, constrained in part by a severe fiscal adjustment, the ruling center-right *Colorado* party - which had been in power since 1995 - mainly focused on expanding existing programs rather than adopting new measures. The opposition centre-left *Frente Amplio (FA)* coalition took power in March 2005 capitalizing on widespread dissatisfaction with the economy and the previous government's management of the crisis. The *FA* campaigned on a platform of extensive pro-poor redistribution and structural economic reforms.

I.a PANES objectives and components

The new *FA* government swiftly implemented the National Social Emergency Plan (*Plan de Atención Nacional a la Emergencia Social*), or *PANES*. This was a temporary social relief program, running from April 2005 to December 2007. The program had two main aims: first, providing direct assistance to households that had experienced a rapid fall in living standards since the onset of the 2001-2002 crisis; and second, and in light of rising poverty during the 1980s and 1990s, strengthening the human and social capital of the poor, to enable them to eventually climb out of poverty on their own. *PANES* was the most generous anti-poverty program in the country's history up to that time, and was heavily publicized by the government and the mass media.

PANES included several distinct components. The largest element was a monthly cash transfer (*ingreso ciudadano*, "citizen income"), whose value was set initially at UY\$1,360 (US\$70 at the January 2008 real exchange rate), independent of household size. This was a very large transfer for the target population, amounting to more than 50% of average self-reported pre-program household income among program recipients.⁴ Households with children or pregnant women were also entitled to a food card, an in-kind transfer that operated through an

² *The Economist* ranks Uruguay as one of only two "full democracies" in Latin America (the other is Costa Rica). *Transparency International* ranks Uruguay second only to Chile in the region in perceived control of corruption (see Appendix Table A1).

³ Between 2001 and 2002 per capita income fell 8%, 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.

⁴ One should be cautious in interpreting this figure as some households might have perceived an incentive to underreport baseline income. As noted below, self-reported baseline income is not used in the predicted income score that determined *PANES* program eligibility.

electronic debit card, whose monthly value varied between UY\$300 and US\$800 (UY\$15 and US\$41) depending on the number of children and pregnant women in the household. Around seventy percent of *PANES* beneficiaries eventually received the food card.⁵

I.b *PANES* eligibility, enrollment and baseline data

After an initial enrollment phase, nearly 190,000 applicant households were visited by Ministry for Social Development (*Ministerio de Desarrollo Social, MIDES*) personnel and administered a baseline survey, providing information on household characteristics, housing, income, work, and schooling, characteristics among the applicant households, Over 102,000 eventually became program beneficiaries, around 10% of all Uruguayan households and 14% of the population. The program was fully rolled out within a year of its launch.⁶

To determine program assignment, the government used a predicted income score that depended only on household socioeconomic characteristics collected at baseline. 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 below a critical per capita income level, using a highly saturated function of household variables.⁷

⁵ Nearly 85% of applicant households had at least one child and/or a pregnant woman. However, this component of the program took same time to be implemented due to logistical difficulties. Additional but less common components included public works employment opportunities, education and training, and health care subsidies (further details on *PANES* are in Supplementary Appendix B). The transfer program continued alongside a system of family allowances that had been in place since 2004. Both *PANES* eligible and ineligible households maintained access to that program, which was much less generous.

⁶ On an annual basis, the total cost of the program was 0.41% of GDP and 1.95% of government social expenditures. The program was entirely financed through Uruguayan government revenue. Note that although payments did not begin until the second half of 2005, beneficiaries were paid arrears back to the date of enrolment.

⁷ The underlying probit model was estimated using the 2004 National Household Survey (*Encuesta Continua de Hogares*). The resulting coefficient estimates were then used to predict an income score for each applicant household using *PANES* baseline survey data. Variables used to predict income 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.). The weights attached to the observed covariates to determine the predicted income score differed between Montevideo and the rest of the country. The eligibility thresholds varied across the country's five main administrative regions in order to entitle the same proportion 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). Only households with predicted income scores below a predetermined threshold were assigned to program treatment. The eligibility

This discontinuous rule for program assignment was suggested to the Minister by researchers including some of the authors of this paper for the purpose, among other things, of carrying out a prospective evaluation of *PANES*. Government officials proved remarkably receptive to the proposal and uninvolved in the design and calculation of the eligibility score, which was computed with the assistance of bureaucrats at the Social Security Administration (*Banco de Previsión Social*). To avoid potential manipulation, neither the enumerators nor households were ever even informed that a score was used to determine eligibility, nor consequently about the exact variables that entered into the score, the weights attached to them, or the program eligibility threshold.⁸ We return to this issue in the empirical analysis below.

I.c Follow-up surveys in 2007 and 2008

Figure 1 presents a timeline of the *PANES* program and data collection. After baseline data collection in the spring 2005, the first *PANES* follow-up survey was carried out between October 2006 and March 2007, roughly eighteen months after the start of the program. The questionnaire was designed by the authors of this paper, in collaboration with Verónica Amarante at the University of the Republic, Ministry of Social Development staff, and colleagues in the Departments of Economics and Sociology at the University of the Republic, the latter also being in charge of data collection.

score components and weights were eventually made public on the *MIDES* website after the program ended. The choice of using predicted income rather than actual reported income 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, and thus less likely to target the chronically poor. 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 risk of misreporting. By using a predicted income score, as opposed to self-reported income, the government hoped to minimize such strategic misreporting. Martinelli and Parker (2009) discuss the risks of both under- and over-reporting of assets in the context of a similar anti-poverty program eligibility score in Mexico. There were two additional participation conditions. Only households with actual monthly per capita income below UY\$1,300, excluding pension earnings and child benefits, were administered the baseline survey and could thus apply. Household income for eligibility purposes was computed as the maximum of self-reported income and earnings reported in official social security records. All participating households were informed of this rule before applying. Beneficiary households whose social security income later exceeded the UY\$1,300 threshold eventually lost eligibility. 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/Oportunidades*). However, due to lack of monitoring capacity, the program was unconditional *de facto*, a fact publicly acknowledged by *MIDES* after the end of the program, and there is no record of any household losing *PANES* benefits for failing to meet these criteria.

⁸ A relatively small number of households (7,946) were included in the program before August 2005, before the predicted income score was even constructed, but were later removed if their score exceeded the eligibility threshold. An additional 2,552 homeless households were included in the program irrespective of their income score. The score was slightly modified in September 2005 when *MIDES* realized that few one person households would receive program assistance, and the new formula (which we use) applied from that point forward.

In addition to information on housing, household composition, durables, work, income, schooling, health care utilization, knowledge of political rights and participation in social groups, the survey collected information on economic satisfaction, opinions about the *PANES* program's design and targeting, and respondents' political attitudes, including support for the government, our key outcome variable.

PANES ended as planned in December 2007 and a second follow-up household survey round was collected between February and March 2008, three months after the program had ended and 18 months before the next national elections. This survey was similar in content to the first follow-up, though with several additional questions on social and political attitudes.

In the remainder of the paper, we focus on a sample of 2,232 households that applied for *PANES*, including both recipients and non-recipients in the neighborhood of the program eligibility threshold score (namely, households with predicted probabilities of falling below the target income level within plus or minus two percentage points of the cutoff). Because there was a desire to gather additional information on recipient households, the sample was split between eligible and ineligible households in a 2:1 ratio.⁹

Although the initial non-response rate was relatively high, at 36%, replacement households with roughly the same score as the non-response households were subsequently interviewed; we discuss the implications of non-response below.

I.d Program implementation

Figure 2 reports the proportion of sample households who benefited from the program at any point since its inception as a function of the baseline predicted income score. The beneficiary data is based on program administrative records. The 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.¹⁰ In this and all subsequent figures (though not in the regression

⁹ The original sample contained data on around 3,000 households in the neighborhood of the threshold. This main sample was also 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. Since the eligibility formula was slightly modified in the early months of the program, we restrict the sample to households whose score was computed after September 2005 (thus using the final eligibility formula), who were not homeless, and with a valid response to the question on support for the current government. These criteria disqualified almost 800 households. Results that include all households were presented in an earlier version of this paper, and the main political support impacts and statistical significance levels remain unchanged.

¹⁰ Official Uruguayan government documents report these graphs on a reverse horizontal axis, i.e., with a predicted "poverty score". Obviously, this is only a presentational issue and makes no difference to the estimates.

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 (30) as for ineligible ones (15), such that each cell contains approximately the same number of observations (43 households). These cells thus correspond to consecutive percentiles of the score distribution. A linear polynomial on each side of the discontinuity 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 probability of program receipt at the threshold is 99 percentage points. This implies that enforcement of the rule was nearly as strict as implied by the letter of the law.¹¹

II. RESULTS

In this section, we use the two follow-up surveys, together with the baseline survey (and the *Latinobarómetro* public opinion surveys in some cases) to explore *PANES* program effects on political support for the *FA* government, the main outcome of interest. We first present average treatment effects (in Table 1), then 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 evaluation design (Table 2). In the analysis we do not attempt to disentangle what roles the different program ingredients played in shaping outcomes since there was potentially non-random selection into some of them. We concentrate instead 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.

II.a Impact on reported support for the government during the program

¹¹ Self-reported information from the follow-up surveys is highly correlated with official records. Self-reports indicate that 97% of beneficiary households report having participated in the program and only 7% of non-eligible households report ever having participated, for a discontinuity at the threshold of over 90% (compared to a discontinuity of 99% using official administrative records).

The variable used to measure support for the incumbent government is based on responses to the following question from the follow-up surveys: “*In relation to the previous government, do you believe that the current government is worse (0), the same (1/2), better (1)?*”¹²

Figure 3 presents responses to this question as a function of the normalized predicted income score. The discontinuity at zero provides an estimate of the gap in support for the current government in the *PANES* eligible group versus the ineligible. As of 2007, *PANES* households were significantly more pro-government: among eligible households support for the current government was around 0.90, compared to 0.77 for ineligible households (still a high level of support, as might be expected since the center-left coalition is widely supported by the poor). The estimated discontinuity implies that program eligibility was associated with a 13 percentage point increase in support for the government over the opposition coalition. This provides evidence that households’ political views are responsive to government transfers.

To refine the analysis, we next present regression results to explore robustness to different parametric specifications and the inclusion of baseline control variables. Let S_i be the predicted income score for 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 Lee and Card (2008), we regress a 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 ($f_1(N_i)$ and $f_2(N_i)$), on each side of the threshold, such that $f_1(0) = f_2(0) = 0$:

$$(1) \quad y_i = \beta_0 + \beta_1 1(N_i < 0) + f_1(N_i) + 1(N_i < 0) f_2(N_i) + u_i$$

The impact of program assignment is captured by β_1 , i.e., the change in y at the eligibility threshold. The two fitted plots in Figures 2 and 3 (and subsequent figures) are obtained by letting $f_1(\cdot)$ and $f_2(\cdot)$ be linear, though in the regressions we also allow for quadratic functions. Equation (1) is estimated based on OLS with disturbance terms clustered by score level.

Row 1 of Table 1 reports regression discontinuity (RD) estimates of equation (1) with an indicator for ever being a *PANES* beneficiary household as the dependent variable. Columns 1 to

¹² The questionnaire presents responses in the following order “1: the same, 2: worse, 3: better”. Supplementary Appendix Table A2 provides exact wording (translated) and codes for this question and the other main survey questions included in the analysis. We separately combined responses into a simple indicator for responding that the current government is strictly “better” than the opposition and get nearly identical results (not shown).

3 present specifications with different parameterizations of the functions $f_1(\cdot)$ and $f_2(\cdot)$: no polynomial, a first order polynomial (as in Figure 2), and a quadratic polynomial, respectively. The relationship is strong and robust, across specifications, with a point estimate of 99%.

In row 2 of Table 1 the dependent variable is political support for the government in the first follow-up survey in 2007. Here we use observations with valid responses to the political support variable, reducing the sample slightly. All estimates are of similar magnitude and statistically significant, suggesting a higher level of support for the government - of between 11 to 13 percentage points - among those eligible for *PANES*. This effect is mainly driven by a shift from indifference between the two parties to support for the government, although there is also a small reduction in expressed support for the opposition (not shown).

II.b Effects after the end of the *PANES* program

Row 3 of Table 1 extends the analysis to the 2008 survey, which was collected after *PANES* had ended, and finds similar though somewhat smaller gains in *FA* support - of between 8 and 12 percentage points - in columns 1 to 6.¹³ This result is displayed graphically in Figure 4. The main implication is that the program generated persistent impacts on political support for the government, suggesting that past transfers also factor meaningfully into voters' decision-making.

II.c Potential threats to the validity of the RD estimates

One potential concern with the results in Table 1 is the possibility that assignment to *PANES* somehow favored households with higher underlying support for the governing *Frente Amplio* (*FA*) party (for an example of politically motivated mistargeting in Colombia, see Conover and Camacho, 2007). Unfortunately, we lack data on baseline household political orientation, which

¹³ In results not shown, we find that expressed political support for the government is highly persistent at the household level across the two follow-up survey rounds. To check whether the discontinuity at the true cut-off provides the best fit for the data, we have run 30 additional RD regressions using the political support variable (in specifications like that in Table 1 column 2), where we “incorrectly” set the threshold at equally spaced intervals around the true eligibility threshold (ranging from -0.015 to 0.015, where the true threshold is zero). The true eligibility threshold provides the best fit to the data as measured by the regression R^2 (not shown), providing reassurance that the discontinuity we exploit is a genuine feature of the data. As an additional robustness check, we take advantage of the fact that the *PANES* eligibility threshold differs slightly across Uruguayan regions to estimate a difference-in-differences model, conditioning on the un-standardized income score and regional fixed effects and focusing on the coefficient estimate on an indicator for *PANES* eligibility in that region. Political support impacts are statistically significant at 95% confidence and are remarkably similar to those in Table 1 (not shown).

prevents us from directly testing this alternative hypothesis; however, a variety of evidence makes it appear implausible.

First off, the evidence in Figure 2 that virtually all eligible households received the program while nearly all ineligible households did not, suggests that blatant patronage is unlikely to have occurred.

The possibility 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 answer the questionnaire strategically to gain eligibility, can also safely be ruled out. This is because the poverty score and the assignment rule were developed after the baseline survey had already been collected from households in our sample, the timing making it impossible for enumerators or households to know exactly how to manipulate their responses.¹⁴ As noted above, the predicted income score formula was also never publicly disclosed or shared with the *MIDES* officials implementing *PANES* until after the program had ended (although the formula was of course presented to and approved by the Minister and her high-level staff).

Nonetheless, we present formal tests for non-random assignment around the eligibility threshold. First, we estimate equation 1 for multiple baseline covariates measured in 2005 in Table 2 (and present the results graphically in supplementary appendix Figure A1). If eligibility score manipulation occurred systematically, 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 fits (as in Table 1, column 2), we fail to find evidence of a discontinuity at the threshold for any pre-treatment household covariate including: average household members' age and education (among those age 16 or older), income, and for the gender, age and years of education of the survey respondent. Consistent with this validity check, the results in Table 1 are almost unchanged when household controls are included (columns 4-6). As for political behaviors, 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 (row 10) was 92% for ineligible households

¹⁴ Households were able to apply to *PANES* the entire duration of the program, so later applicants might potentially have learned the household characteristics that made eligibility more likely. However, this is not a source of concern in the present analysis as the sample we use in the two follow-up surveys was drawn from among early applicants.

and 93% for the eligible (and the difference is not significant), in line with the consistently high turnout in Uruguay, where voting is mandatory.¹⁵

As an additional check for manipulation around the eligibility threshold, we also present the non-parametric distribution of the standardized score. If manipulation occurred so that some ineligible households were mis-assigned a low predicted income score, one would expect excess bunching of households just below the threshold (DiNardo and Lee, 2004; McCrary, 2008a). Supplementary appendix Figure A2 reports the proportion of households with different score levels, for the population of households (20,463) in the neighborhood of the threshold (-0.02 to 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 no indication that households just below the eligibility threshold are overrepresented relative to those just ineligible.¹⁶

A final concern is that non-response rates could be systematically related to program eligibility. This could be a concern even though non-respondents were in practice replaced by households with a similar predicted income score if non-responding households differ in their unobservables. In rows 8 and 9 of Table 2 we report the relationship between survey non-response (defined as either not responding to the survey at all, which is 36% in the first follow-up survey, or having missing data for the question on political support for the government, which is slightly higher at 41%) and *PANES* eligibility among households in the both the first and second follow-up survey. The relevant population here is given by households in the original sample frame (i.e. excluding replacement households). Neither coefficient estimate is statistically significant at conventional confidence levels, ruling out selective non-response.

II.d Measuring political support

One issue with interpreting the results from the previous analysis pertains to the phrasing of the survey question on political support for the government. This question refers to support for the incumbent relative to the previous government, not to the current opposition coalition, hence implicitly not allowing for any policy repositioning by the opposition. The framing of the question may also fail to accommodate expectations about government performance going

¹⁵ Uruguayan citizens who fail to vote (other than for justified reasons, i.e., hospitalization or living abroad) are officially barred from receiving public benefits and transfers, enrolling in public education, accessing public employment or leaving the country, unless they pay a non-trivial fine.

¹⁶ The point estimate of the log difference at the threshold in Figure 3 is just 0.041 (s.e. 0.027).

forward. Actual voting data at the individual level would be the ideal outcome, but is typically impossible to collect in democracies given the secret ballot. Moreover, no national elections were held in Uruguay during the 2005-2008 period. A second best alternative is stated voting intentions in a survey, although these were not collected in the follow up surveys at doing so was deemed politically inappropriate.

We also use a question in the 2008 follow-up survey on “confidence in the President” (coded 0=“Little” 1/2=“Some confidence”, 1=“A lot”) as discussed in further detail below. In the *Latinobarómetro* survey (2005-2007), which contains an analogous question, the correlation between confidence in the President and stated *FA* voting intentions is very strong, at 0.50 (statistically significant at 99% confidence).¹⁷ Moreover, we matched up *Latinobarómetro* data to actual vote share at the Uruguayan *departamento* level, roughly equivalent to a U.S. county, and find a correlation between stated *FA* voting intentions and actual *FA* votes in the 2004 election at 0.85. Thus while we cannot translate the gains in self-expressed *FA* support due to *PANES* into a precise number of additional votes, expressed support and actual votes are likely to be closely related.¹⁸

A related concern is that *PANES* households might have expressed higher support for the government in the follow-up survey for fear of losing their program benefits and thus their responses might not reflect actual voting intentions. Precautions were taken during data collection to address this concern. Households were not informed about the precise objectives of the follow-up survey: both the title of the survey and information provided to respondents only referred to the University departments administering the survey and neither made specific mention of *PANES* or *MIDES*.¹⁹ Questions about the *PANES* program were only asked at the very end of the questionnaire and after the questions on political views. Both follow-up survey

¹⁷ *Latinobarómetro* is a survey conducted every year in 18 Latin American countries by the *Latinobarómetro* Corporation, a non-profit organization based in Chile. The survey gathers information on public opinion, attitudes, behavior and values. Every year around 19,000 households are interviewed throughout the continent, with a nationally representative sample of approximately 1,200 households in Uruguay.

¹⁸ There is a large literature in political science documenting the strong link between voting intentions and actual voting behavior, see Rothschild and Wolfers (2010), Granberg and Holmberg (1990), among many others.

¹⁹ The wording used by enumerators in the consent statement was: “Good morning/afternoon, my name is ___ and I’m a student at the University of the Republic. We are currently in this neighborhood carrying out a survey of families who live here. Your name and address were randomly chosen from a list of neighbors (provided by the National Statistics Bureau). Could I ask you a few questions? I remind you that all information that you give me is confidential (Statistics Secret Law #16.016) and will only be used for statistical purposes.” (authors’ translation).

questionnaires were modeled after the National Household Survey, further easing concerns that respondents would associate the survey with *PANES*.

Perhaps most important is the fact that any direct incentive to respond strategically to the survey questions based on *PANES* status should have disappeared once the program had ended. Since the effect of transfers on expressed political support is found to persist into 2008, this further eases concerns regarding strategic misreporting.

II.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 from the program, in which case the estimates are a combination of two distinct effects. A finding that those who barely lost out on receiving the *PANES* transfer have lower political support due to bitterness is not a threat to our overall strategy, though, since the RD design still allows us to test our overarching empirical hypothesis, namely, that differential transfer receipt due to *PANES* at the eligibility score threshold significantly impacts political support. However, it would have implications for understanding the net support the *FA* gained or lost among those households near the threshold.

Although there is no direct way to measure these effects, we provide suggestive evidence that any embitterment effect is unlikely to be large, and that most of the support difference we estimate is due to gains among *PANES* beneficiaries. We use the *Latinobarómetro* opinion data to predict household support for the President. As in the second follow-up survey of February-March 2008, the September 2007 *Latinobarómetro* asks: “*How much confidence do you have in the President?*”, which we again code up to take on values from zero to one.²⁰ If, as appears reasonable, the *Latinobarómetro* sample contains relatively few *PANES* applicants (as they were a relatively small share of all Uruguayan households), one can estimate the counterfactual level of confidence in the President among beneficiaries and non-beneficiaries by simply extrapolating what is observed in the population at large. To do so, we run a regression of confidence in the president on a range of household covariates in the 2007 *Latinobarómetro*, and

²⁰ Because the 2007 *Latinobarómetro* provides four possible answers to this question (1: None, 2: Little, 3: Some, 4: Much) while the 2008 follow-up survey provides three possible answers (1: Little, 2: Some, 3: Much), we reclassify the *Latinobarómetro* data by lumping the first two answers into one (1: None or little, 2: Some, 3: Much). As in the other regressions we rescale these variables between 0 and 1. Note that there is no question on confidence in the President in the 2008 *Latinobarómetro*, unfortunately.

use the predictions from this model to derive counterfactual support among households in our 2008 sample.²¹ It is reassuring that the correlation between the predictions from this model and actual responses in the follow-up survey is large (at 0.49) and significant at 99% confidence.

Figure 5 reports predicted confidence in the President as a function of the normalized income score, as well as presenting the actual level of confidence in the President in the 2008 follow-up survey. Unsurprisingly, support for the President as predicted based on the *Latinobarómetro* is smooth around the *PANES* eligibility discontinuity, since we have access to only a limited set of common respondent covariates (in both the follow-up surveys and the *Latinobarómetro*). Nonetheless, predicted confidence in the President among ineligible households (to the right of zero) is very similar to the levels in the follow-up survey and far below the support expressed by *PANES* beneficiaries, providing some suggestive evidence against the hypothesis that embitterment is responsible for most of the difference in government support between *PANES* eligible and ineligible households.

III. EXPLAINING THE EFFECT ON REPORTED POLITICAL SUPPORT

The estimates in the previous section show significantly higher support for the government among households that received the *PANES* transfer program that persists after the program ended. In this section we explore different possible channels behind the estimated effects. The richness of the data allows us to assess various theories of voter decision making.

III.a The impact of contemporaneous income transfers

Consistent with its stated objectives, *PANES* had a positive effect on living standards. Table 3 reports responses from the first follow-up survey and shows sizeable program impacts on log per capita household income (including transfer income) in 2007, on the order of 22% (in row 1).²²

²¹ To predict confidence in the President, we use a quadratic in respondent age, a quadratic in years of schooling and interactions of these variables with the gender indicator, a home ownership indicator, an indicator for whether the household has a color television set, and a car ownership indicator.

²² Note that self-reported per capita income grew by a remarkable 25% for *PANES* ineligible households from 2005 to 2007, presumably due to Uruguay's rapid macroeconomic recovery after 2004, although mean reversion or underreporting of baseline income could also be playing a role. The estimated income gains in 2007 (UY\$452) among *PANES* beneficiaries are smaller than the transfer amount (UY\$1,360) suggesting some offsetting behavioral responses in terms of reduced labor income from other sources, although note that impacts on hours of labor supply are not statistically significant (not shown). While the income transfer alone might have depressed household labor supply due to an income effect or due to the program being means tested, other *PANES* components (e.g., education and 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. This limited adult labor supply

In addition to the income transfer, beneficiaries also received in-kind transfers and services, not all easy to monetize and all potentially enhancing well-being. For instance, just by virtue of being included in the program, beneficiary households might have experienced an improvement in their self-esteem and psychic well-being. Row 2 of Table 3 reports a subjective measure of household satisfaction, on a scale 0 to 1, and *PANES* beneficiaries show a statistically significant higher level of satisfaction than non-beneficiaries (coefficient 0.073).²³

One obvious explanation for the effects on support for the government in 2007 (in Table 1, row 2) is that in 2007, when *PANES* was still ongoing, households simply responded myopically to contemporaneous income transfers, but this simple explanation can be rejected.²⁴ Consistent with the end of the program in December 2007, row 1 of Table 4 shows that there is no effect of *PANES* eligibility status on post-program income levels in 2008 (including transfer income), with a coefficient of -0.070 that is not significant at conventional levels. The continued support for the government that former *PANES* recipient express in 2008 is thus strong evidence against simple pocketbook voting.

III.b Persistent gains in living standards

A related possibility is that earlier program participation had a persistent impact on living standards, even if former *PANES* beneficiaries' income fell back down to the same level as non-beneficiaries by early 2008,. However, there is no effect of the program on household durable

response is consistent with results from Mexico's *Progres*a program (Parker and Skoufias, 2000). We also find modest positive effects of the program 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), not shown, perhaps due to the conditions officially attached to program receipt, which may have swayed some households even though they were never enforced. Although there is no detailed consumption or savings information in the survey, *PANES* households claim to have spent the transfer primarily on food and clothes (71%), to pay utility bills (10%) and to repay debts or loans (10%). According to World Bank (2008) *PANES* contributed to halve the incidence of extreme poverty in Uruguay (from from 4.7 in 2004 to 2.6 in 2007).

²³ We use 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?" (We re-scale this and all questions that follow from 0 to 1, as described in Supplementary Appendix Table A2).

²⁴ An alternative explanation for the 2007 effects is that households were unaware of the program's temporary nature and anticipated permanent income transfers. This explanation can be ruled out as, in the 2007 survey data, we find that only 3% of *PANES* beneficiaries believed the program was permanent, while 57% knew it was temporary (and the remaining respondents were unsure). It does not appear that the majority of households expected the program to last past December 2007.

asset ownership (Table 4, row 2),²⁵ suggesting that the program affected contemporaneous consumption but did not meaningfully boost post-program income or consumption.

There is also no evidence that that *PANES* served as a passport into other government social programs, another potential channel behind persistent effects. In January 2008, *PANES* was replaced by a new transfer program, the *Plan de Equidad (PE, or Equity Program)* aimed specifically at households with children.²⁶ Using 2008 program administrative data, we find that equal proportions of households on both sides of the *PANES* eligibility threshold had enrolled in *PE* (coefficient -0.060 and not significant in Table 4, row 3), at a level of roughly 34%.²⁷

While enrollment in the main *PE* program was equal, former *PANES* beneficiaries were still more likely to receive a food card in early 2008 (point estimate 0.141, s.e. 0.032, in row 4 of Table 4). Yet this constitutes a relatively small monetary amount relative to the main *PANES* and *PE* cash transfers and, even when the *PANES* beneficiaries who continued receiving a food card are excluded from the analysis, the effect on incumbent government support in 2008 is positive, statistically significant and almost unchanged (estimate 0.113, s.e. 0.044, not shown in table).

III.c Rational but poorly informed voters

In this sub-section we argue that a model of voter learning about politician preferences, which is formally presented in Appendix C, can account for the main empirical results, namely the finding of both contemporaneous and persistent effects of *PANES* transfers on support for the incumbent government. The framework we develop is related to Drazen and Eslava (2006) and assumes asymmetric information, with voters having imperfect knowledge of politicians' true redistributive preferences for certain individuals or social groups. Departing from Drazen and Eslava (2006) and some other models, we do not solve for optimal government transfer policies: consistent with our empirical setting, where the targeting of transfers is exogenous with respect

²⁵ The assets measured include: heaters, stoves, microwaves, refrigerators, washing machines, dishwashers, TV's, VCR's, DVD players, landline phones, cell phones, computers, motorcycles, and cars.

²⁶ *PE* included a generous cash transfer for poor households with children, with an average monthly transfer of UY\$1,300 (US\$67 at January 2008 real exchange rates), nearly at *PANES* levels. A revised predicted income score was computed for all original *PANES* applicant households (whether beneficiaries or not) based on the same baseline characteristics measured in 2005, but using a new formula, and also featured a different threshold score. Households did not need to reapply for the *PE*, as inclusion was automatic among eligible households. *PANES* households were informed by mail of the program's end date in late 2007 and both *PANES* and non-*PANES* households who were admitted to *PE* received a formal written letter from *MIDES* about their inclusion.

²⁷ There are similarly no significant differences in self-reported participation in the *PE* (results not reported).

to voters' political preferences near the program eligibility threshold, we only focus on one side of the market, namely, voters' response to receiving transfers.

In the absence of a binding commitment technology, politicians' campaign promises in the model are a form of cheap talk, leaving room for uncertainty about their true redistributive preferences. Voters are self-interested and care only about future transfers to themselves. They learn about politicians' preferences by observing their own receipt of transfers in actual government programs, and update their beliefs about politicians' redistributive preferences in a standard Bayesian fashion. Hence past policies affect voters' current political support by shaping expectations about future transfers. This channel can explain the increased support for the incumbent recorded among *PANES* beneficiaries during the life of the program (in 2007), as well as their persistently positive - although eroding - post-program support (in 2008).

Similar results can be obtained if voters' beliefs about government competence - and in particular, its ability to follow through on its electoral promises to enact social programs that provide assistance to poor households - are influenced by the household's own transfer receipt.

III.d How informed were voters about *PANES*?

The assumption that voters were imperfectly informed about the quasi-random nature of the *PANES* program targeting rule is key to the model results discussed above. Indeed, a natural criticism to the model sketched out in the previous sub-section is that voters in the neighborhood of the eligibility threshold should rationally deduce that their chance of receiving future government assistance is effectively the same regardless of which side of the program eligibility threshold they happened to fall on. This re-shuffling near the threshold is likely to occur because of natural fluctuations in household income over time, as well as changes to eligibility criteria. Indeed this was exactly the case with the *PE* program launched in 2008, which enrolled equal numbers of former *PANES* beneficiaries and non-beneficiaries near the threshold. This insight that the likelihood of future transfer receipt should be the same on both sides of the discontinuity implies that current support for the incumbent government should be the same for forward-looking voters on both sides of the threshold, sharply contradicting our empirical findings.

There are several other related, but distinct, reasons why voters aware of the nature of *PANES* targeting should not rationally increase their support for the incumbent government. First, the fact that the targeting rule was designed by outside researchers, and thus that allocation

of transfers near the eligibility threshold does not necessarily reflect the government's own redistributive preferences, should lead rational voters not to base their beliefs about future transfers on targeting in *PANES*. Similarly, if voters are fully aware that households on both sides of the eligibility threshold are equally poor and thus deserving of the transfer, then receiving the transfer personally should not alter their beliefs on the government's ability to fulfill its campaign promises regarding redistribution to the poor.

Yet the underlying assumption that voters are very well-informed about the *PANES* targeting rule appears unrealistic in our context, and possibly in many other instances where the procedure by which government transfers are allocated is either not disclosed to applicants or where the technical eligibility criteria are poorly understood. In the case of *PANES*, assignment to the program was purposely made opaque to avoid manipulation of the predicted income score. In particular, although some households might have inferred that targeting would ultimately be based on the observed characteristics measured in the baseline survey, applicants were never told explicitly that they had been assigned a score nor what that score was, that eligibility would be determined based on a strictly discontinuous rule at an eligibility threshold, nor that this procedure had been designed by outside researchers. As a result respondents in our sample did not know whether they were even "close" to the eligibility threshold or not.

Perhaps due to this lack of objective information about the eligibility criteria for *PANES*, we find that *PANES* beneficiaries had starkly different perceptions about the nature of program targeting than non-beneficiaries. Beneficiaries were significantly less likely to believe that program transfers were poorly targeted, i.e. that some beneficiaries should not have received the transfers, that some non-beneficiaries should have, or that the program should have spread around transfers to additional households (Table 3, rows 3 to 5). These differences are best interpreted, in our view, as evidence of just how limited objective information about program targeting was in Uruguay in 2007, although the emergence of self-serving beliefs among transfer recipients is an obvious alternative explanation. Given that information on the exact targeting rule was not publicly released during the life of the program, acquiring information - perhaps by observing the eligibility status of a large number of neighbors and friends and gauging how it related to their characteristics - would clearly have been very costly for individual households. In the plausible case in which program applicants on both sides of the eligibility threshold felt that they were equally deserving of the *PANES* transfer, but due to information constraints were only

able to infer the degree of program mistargeting from their own eligibility status, it is sensible that those who actually received the transfer would believe that the extent of mistargeting was lower, as we found. This could also explain their higher levels of support for the program: even in 2008 after the program had ended, former *PANES* beneficiaries still held a much more positive opinion about the program, at 83%, relative to non-beneficiaries (at 58%, Table 4, row 5).

That applicants households overweighted their own experience in making sense not only of the *PANES* targeting rule but also of current economic conditions is similarly attested by the finding that beneficiaries were significantly less likely to have perceived increased social differences over the past year (-0.102, s.e. 0.041, in Table 4, row 7) and that they were more likely to express positive expectations not only about their own household's overall situation but also of the country's (gains of 0.045 and 0.055, respectively in rows 8 and 9). These latter findings are provocative insofar as they suggest that greater support for the incumbent among program beneficiaries is potentially driven not by their personal living standards alone, but also in part by their perceptions about society at large. Altered perceptions about society - for instance, beneficiaries' optimistic views about the evolution of social inequality - are a factor that could contribute to the persistent gains in support for the government among former *PANES* beneficiaries into the post-program period.

III.e Effects on other political attitudes

The finding that beneficiaries take a more optimistic view of government policies is borne out in several other dimensions. *PANES* beneficiaries are not only more supportive of *PANES* itself but also more supportive of the new *PE* program (coefficient 0.054, Table 4, row 6), despite the fact that they are no more likely to benefit from it than former *PANES* non-beneficiaries. Former *PANES* beneficiaries express greater support not just for the incumbent government, the President, *PANES* and *PE*, but especially for the Ministry of Social Development (0.185, s.e. 0.038, Table 4, row 10), the agency that designed and administered *PANES*. However, they do not express significantly more support for other institutions and organizations that are not directly related to *PANES*, for instance, the Parliament or local councils, or even for the social security administration that disbursed *PANES* transfers (rows 12 to 15), although point estimates tend to be positive, suggesting small spillovers. We take this evidence to indicate that former

PANES beneficiaries are relatively discerning and are not simply casting a more optimistic eye on all political institutions and organizations across the board.

There are a number of other intriguing impacts of *PANES* receipt, including a statistically significantly higher level of national pride at being Uruguayan (coefficient estimate 0.049, s.e. 0.024, from an already high level of patriotic feeling, row 16 in Table 4), and significantly higher self-expressed interest in politics in general (0.065, s.e. 0.031, row 17).

III.f Behavioral and psychological explanations

We argue above (and in supplementary appendix C) that a model of rational but poorly informed voters can rationalize the broad patterns in the Uruguay data, and in particular the persistence of *FA* support gains into the post-*PANES* period. However, this does not necessarily rule out behavioral economics explanations. Models of reciprocity from psychology and economics appear especially appropriate. In particular, we cannot rule out a gift-exchange model between voters and politicians, where voters feel a sense of indebtedness to the politicians and political parties who favored them in the past, albeit an effect that might decay as time elapses since the transfer (as Gneezy and List, 2006 find evidence for empirically in a labor market setting).

Yet we do not find evidence that *PANES* transfers affected political preferences, another possible non-standard, behavioral channel behind our estimated effects. We find no evidence of an ideological shift to the left, at least based on responses to a question about whether “hard work pays off in life” (0.022, s.e. 0.025, in row 18 in Table 4), which is generally thought to capture more conservative views.²⁸ This indicates that a change in left-right ideology is unlikely to be the key driver behind the increased political support for the *FA*.

IV. SUMMARY AND DISCUSSION

This paper finds that beneficiaries of a large government anti-poverty program were significantly more likely to support the current government than non-beneficiaries. Using individual survey data on expressed political support and a credible regression discontinuity research design, we

²⁸ The *Latinobarómetro* 2006 and 2007 ask the same question: “Do you believe that in (country X) a person who is born poor and who works hard can become rich? (1: Born poor working hard can become rich 2: Born poor can never become rich)”. We correlate this variable to self-expressed political ideology (“In politics, people normally speak of “left” and “right”. On a scale where 0 is left and 10 is right, where would you place yourself?”). The correlation coefficient (among households in all countries) is negative and statistically significant at 99%, implying that left-leaning individuals are indeed less likely to believe that hard work pays off in life (not shown).

find large and robust effects - on the order of 11 to 13 percentage points - that last into the post-program period. The results indicate that government economic policies can have large and persistent impacts on beneficiaries' political and social attitudes.²⁹ The research design only allows us to estimate a local treatment effect at the program eligibility threshold, namely, among relatively poor households. Because of the local nature of the estimator, our analysis is largely uninformative about the overall change in support for the government as a result of the policy.

It is worth emphasizing that the Uruguayan government's decision to delegate design of the targeting rule to outside researchers, their consent to a quasi-random regression discontinuity design, and their decision not to disclose the targeting rule to the general public during the life of the program are critical to the econometric identification strategy and credibility of results in this paper. Their decision to forgo the opportunity to allocate *PANES* transfers strategically or engage in patronage-style targeting might appear puzzling in light of textbook models of opportunistic politicians. Simply put, a government only interested in maximizing its re-election chances should have targeted voters for transfers strategically rather than through a quasi-random targeting rule, given voters' likely positive response to a transfer. Although one immediate objection to this line of reasoning is that existing empirical evidence on the extent of voters' responsiveness to targeted government transfer is scant (as we argued in the introduction), the Uruguayan government's decisions still suggest that their objective function included factors beyond immediate re-election motives. In the case of the Uruguayan government in 2005 when *PANES* was launched, this may have included an ideological goal of extensive redistribution towards the poor (by the center-left coalition that had just come to power), a preference for credibly estimating the impact of the *PANES* program, which was the first of kind in Uruguay, and perhaps a desire to grant this and other programs more popular legitimacy by taking a relatively hands-off and technocratic approach to implementation, targeting and evaluation.

We are able to rationalize our main empirical results in a model of rational but poorly informed voters. In particular, we argue that persistent support for the incumbent can be explained by voters inferring the government's redistributive preferences for people like themselves based on past targeting. A key to this model is the notion that voters do not fully understand their quasi-random assignment to the program and - erroneously, as it turns out - interpret their program beneficiary status as signal of government redistributive preferences.

²⁹ DiTella et al. (2007) reach a similar conclusion about the long-run impacts of a land reform program in Argentina.

Indeed, poor information about program targeting is particularly plausible in our setting: in order to avoid manipulation of the eligibility score, the program targeting rule was not publicly disclosed until after the program had ended, and survey evidence indicates that program applicants had poor knowledge about program targeting in practice.

The finding that voters respond strongly to their own program eligibility status does not necessarily imply that their political attitudes are entirely self-serving. Given their limited knowledge about the quasi-random program targeting rule, *PANES* applicants could reasonably have interpreted their own receipt of transfers as a strong signal of the government's willingness and ability to assist the poor, which might in turn increase their support for the incumbent. We find in the paper that transfer recipients do show increased confidence in the government and its policies, greater optimism about the future of both their household and the country as a whole, and even perceive that social inequalities are becoming narrower. This optimism about country's direction is a plausible contributor to *PANES* recipients' greater support for the government.

While models of rational but poorly informed voters can make sense of our findings, we certainly cannot rule out alternative explanations rooted in the psychology and economics concept of reciprocity. Establishing the relative importance of these two explanations – poorly informed voters versus voter reciprocity towards politicians – in generating the persistent increases in political support among government transfer recipients that we observe is an important area for future research.

The results of this paper are particularly interesting in light of the increasing popularity of field experiments where households are randomly assigned benefits for the purpose of credible impact evaluation. The concern arises as to whether participants should be explicitly informed about the randomized nature of assignment to program assistance. Leaving aside any ethical concerns, it is possible that public support for the intervention (and perhaps ultimately for the government or organization that implemented it) could be affected by the disclosure of this information. In fact, a promising approach to disentangling the two distinct theoretical mechanisms highlighted above – poorly informed voters versus reciprocity – in a program like ours would be to provide information to only a subset of beneficiaries about the true random (or quasi-random) nature of program assignment. If voters are rational but poorly informed about the targeting rule, then providing better information on targeting should dampen differences in stated political support for the incumbent between beneficiaries and non-beneficiaries, since both

groups would then realize that transfer receipt does not in fact provide a meaningful signal of government redistributive preferences, competence, or anything else, for that matter. However, if the gap in political support for the incumbent between program beneficiaries and non-beneficiaries is unaffected by complete knowledge of the targeting rule, this would suggest that voter reciprocity is a more compelling explanation for our empirical findings. The authors hope to carry out a randomized information intervention along these lines in Uruguay in the future in an attempt to distinguish between the competing models and to advance understanding of voter decision-making.

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Figure 1: *PANES* program and data collection timeline

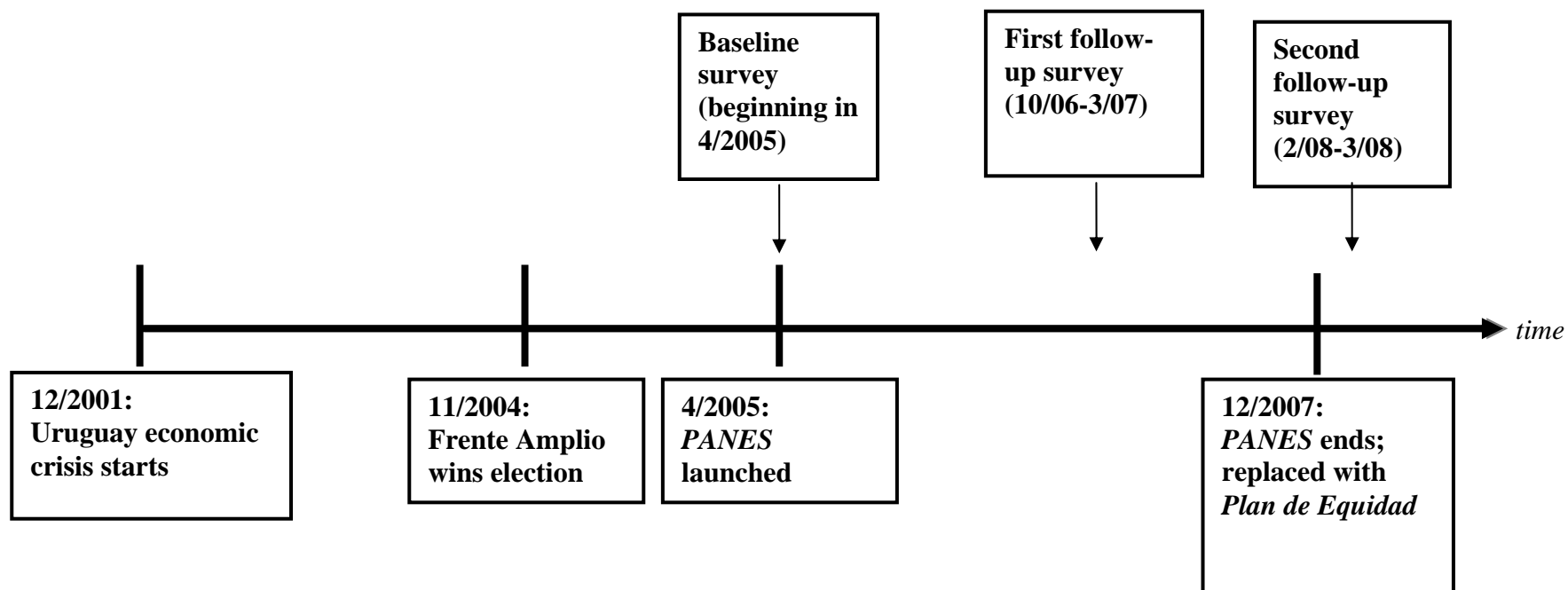
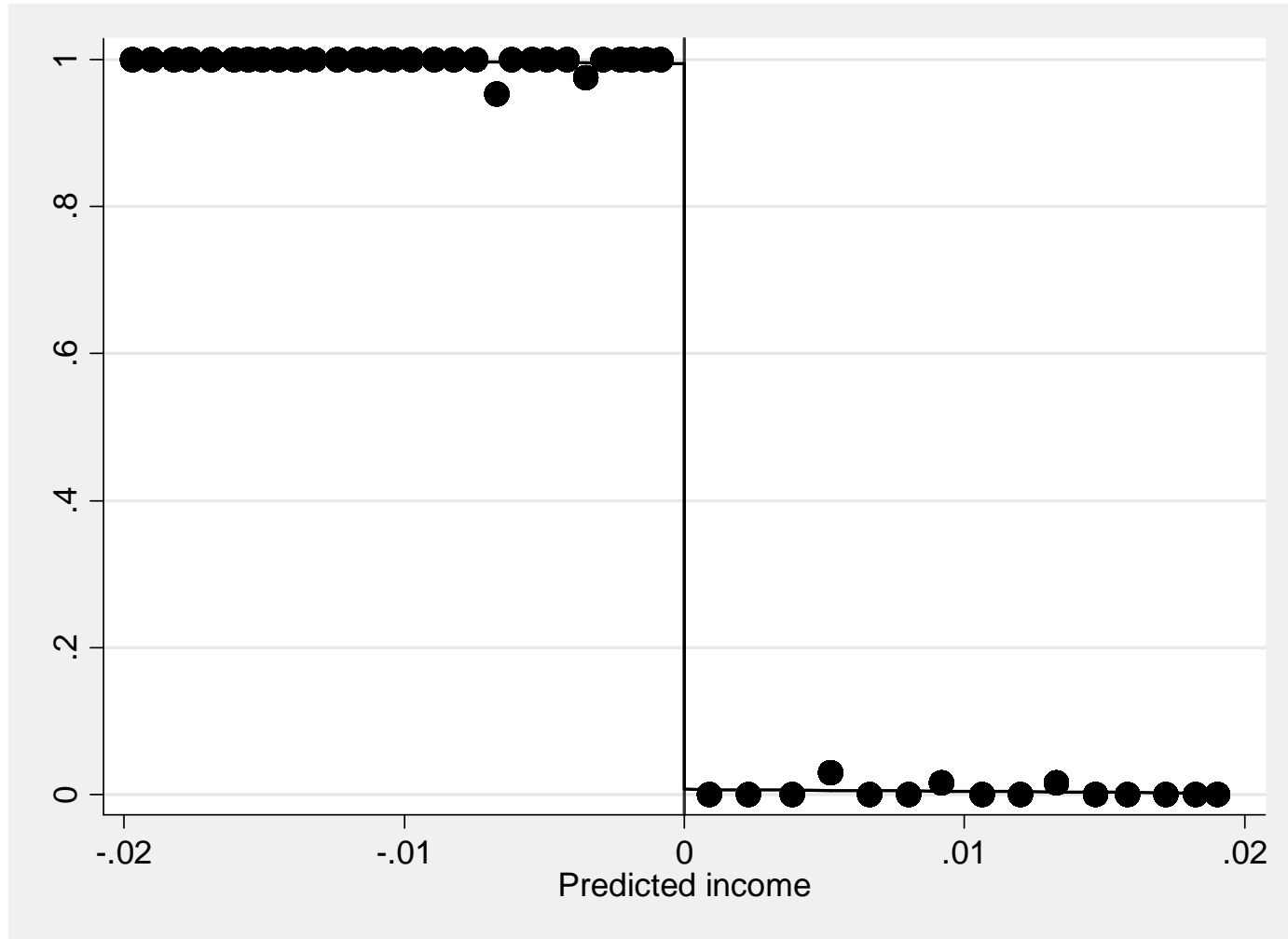
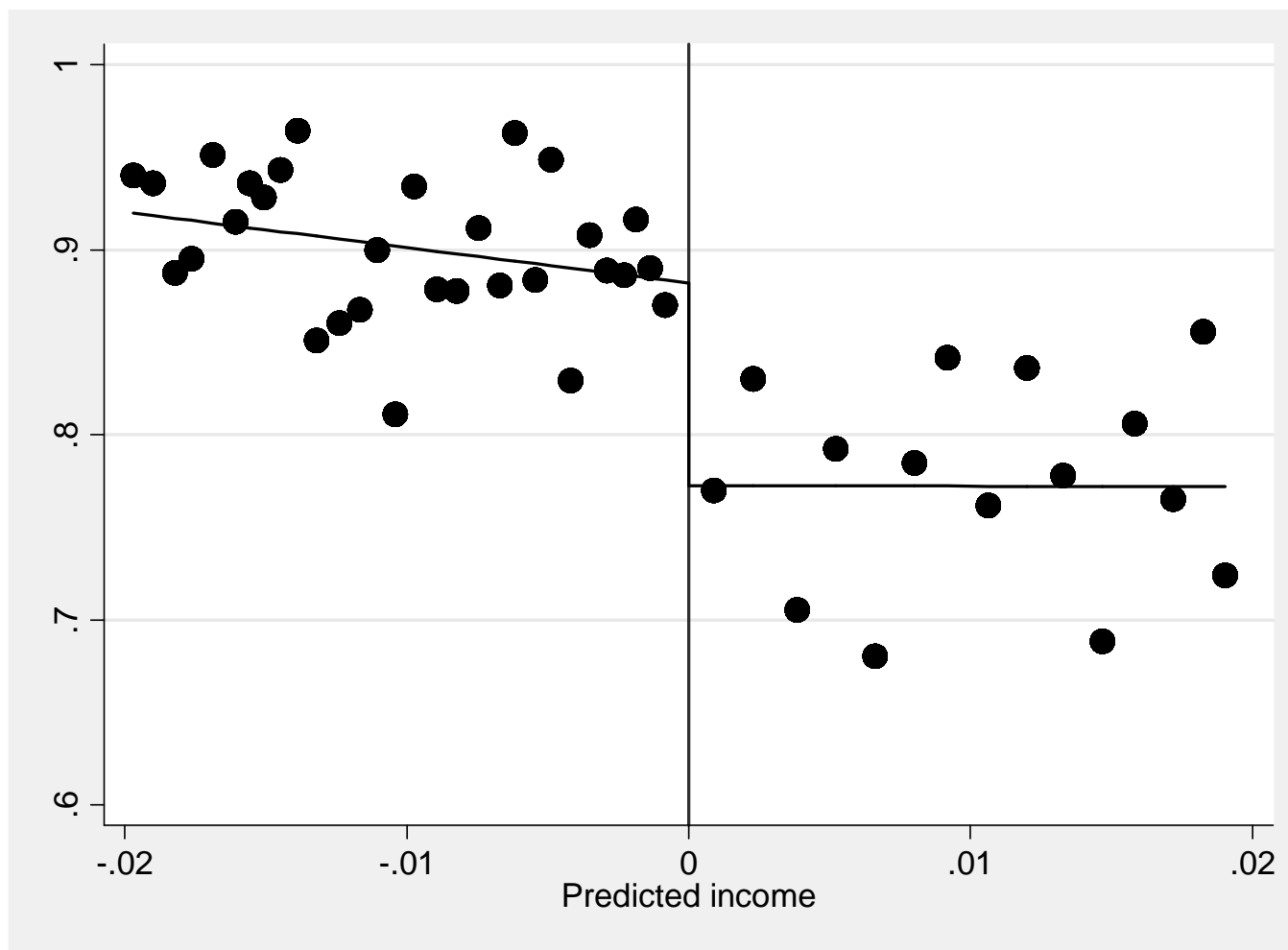


Figure 2: *PANES* program eligibility and participation



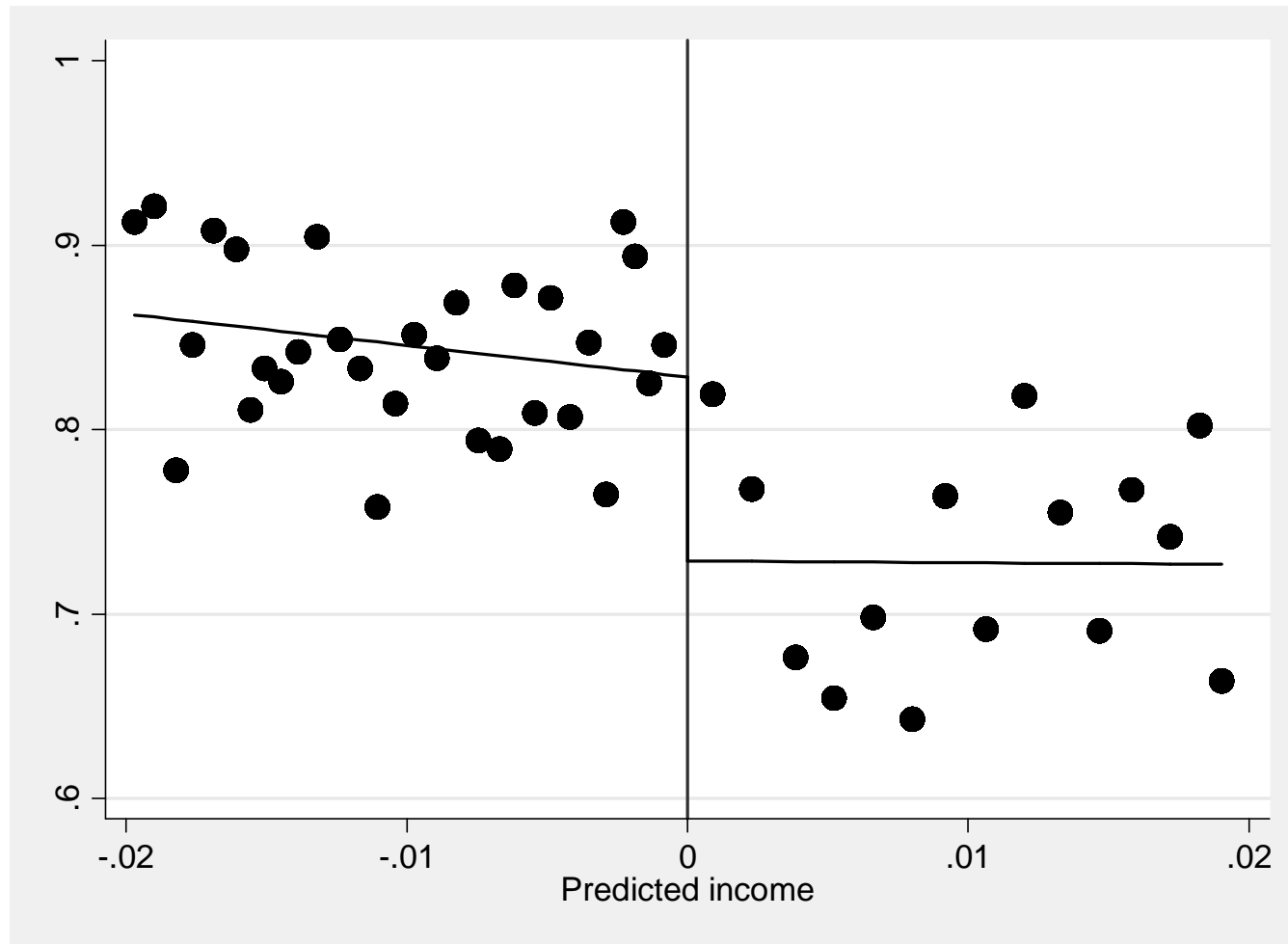
Notes. The picture reports the proportion of households ever enrolled in *PANES* as a function of the standardized score (based on administrative data). The fitted plots are linear best fits on each side of the eligibility threshold.

Figure 3: *PANES* Program eligibility and political support for the government, 2007 follow-up survey round



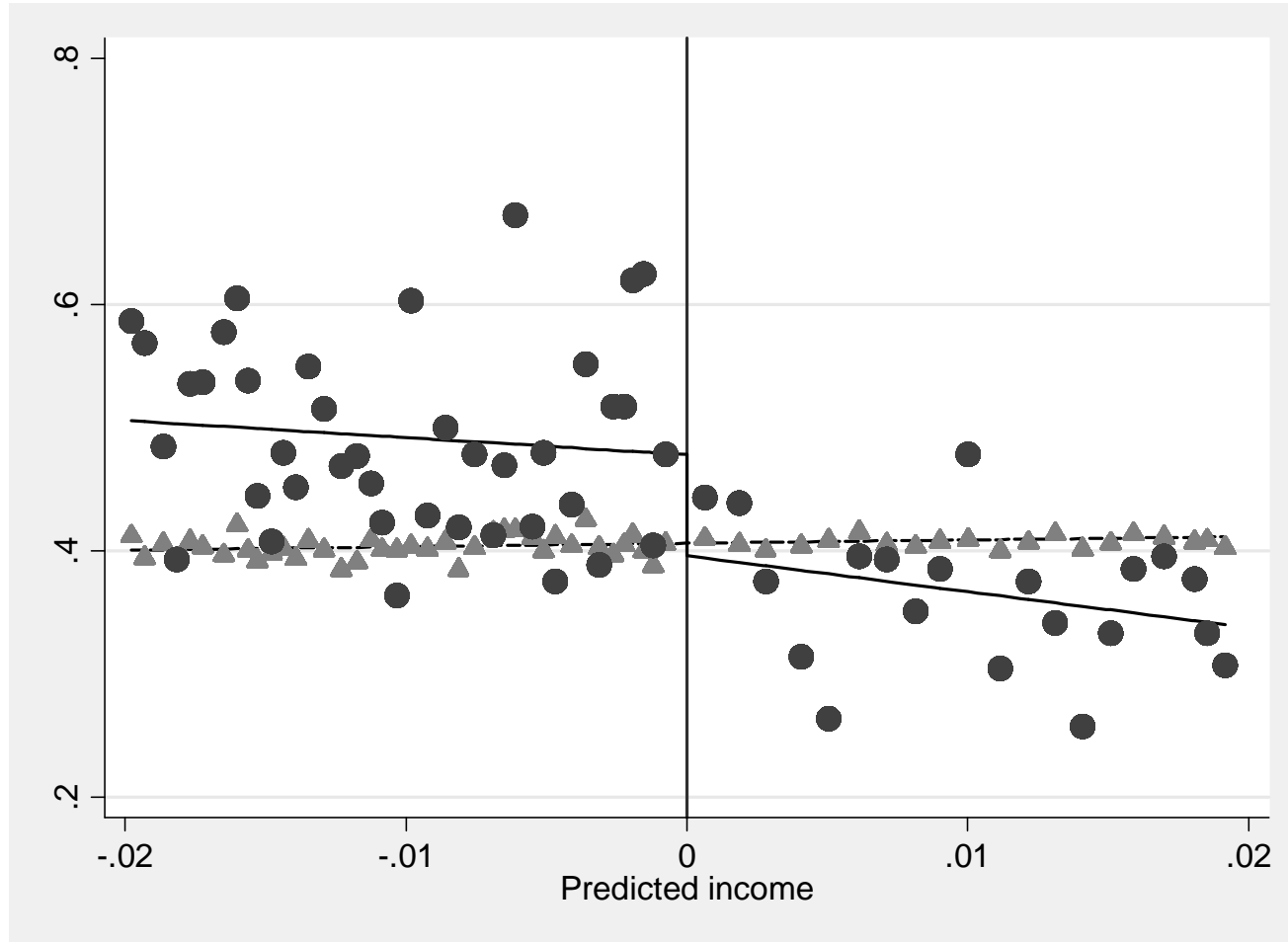
Notes. The figure reports the average support for the current government (compared to the previous government) as a function of the standardized score. Source: first *PANES* follow-up survey (2007). The fitted plots are linear best fits on each side of the eligibility threshold.

Figure 4: *PANES* Program eligibility and political support for the government, 2008 follow-up survey round



Notes. The figure reports the average support for the current government (compared 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 5: Confidence in President:
Actual (circles / solid line) and predicted based on *Latinobarómetro* (triangles / dashed line)



Notes. The figure reports the average actual confidence in the President (circles / solid line) in the second follow-up survey (2008) and the predicted probability based on the *Latinobarómetro* 2007 (triangles / dashed line) as a function of the standardized *PANES* eligibility score and respondent demographic characteristics. See text for details.

Table 1: *PANES* program eligibility, participation, and political support for the government

Dependent variable:	Mean non-eligibles	Coefficient (s.e.)						Observations
		(1)	(2)	(3)	(4)	(5)	(6)	
1. Ever received <i>PANES</i> , 2005-2007	0.004	0.993*** (0.002)	0.987*** (0.005)	0.995*** (0.005)	0.993*** (0.003)	0.988*** (0.005)	0.998*** (0.005)	2,232
2. Government support, 2007 (during the program)	0.77	0.129*** (0.013)	0.110*** (0.026)	0.130*** (0.040)	0.126*** (0.014)	0.103*** (0.027)	0.125*** (0.043)	2,089
3. Government support, 2008 (post-program)	0.73	0.118*** (0.030)	0.100*** (0.043)	0.093** (0.016)	0.119*** (0.031)	0.096*** (0.045)	0.081* (0.045)	1,948
Score controls		None	Linear	Quadratic	None	Linear	Quadratic	
Other controls		No	No	No	Yes	Yes	Yes	

Notes: The table reports estimates of the effect of *PANES* eligibility on program receipt (row 1) and political support in 2007 and 2008 (rows 2 and 3, respectively). Eligibility is an indicator for a household score below the eligibility threshold. 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 pre-treatment characteristics of household members, log per-capita income, age, education and gender of the household head, *localidad* indicators and separate indicators for missing values of each of these variables. Standard errors clustered by score are in brackets. Standard errors are almost identical (differing by roughly 1%) with the jackknife approach in McCrary (2008b). Statistically significant at 90% (*), 95% (**), and 99% (***) confidence level.

Table 2: Program eligibility, baseline characteristics and response rates in 2005 (pre-program)

Dependent variable:	Mean non-eligibles	Coefficient (s.e.)	Observations
1. Log per-capita income	6.34	-0.062 (0.059)	2,150
2. Household average years of education (among those 16 years and older)	4.05	0.135 (0.198)	2,162
3. Household size	3.03	-0.350 (0.242)	2,232
4. Household average age	31.68	-1.195 (2.159)	2,232
5. Respondent is female	0.70	-0.025 (0.057)	2,231
6. Respondent years of education	6.43	0.228 (0.307)	2,206
7. Respondent age	43.63	-0.929 (1.512)	2,231
8. Non-response/missing response on political support question (2007)	0.41	0.037 (0.044)	2,372
9. Non-response/missing response on political support question (2008)	0.46	0.049 (0.048)	2,372
10. Voted in 2004 elections	0.92	0.013 (0.023)	2,200

Notes. The table reports results from regressions of various pre-treatment (2005) characteristics on the program eligibility indicator. The specification is the same as the one in column 2 in Table 1. See also notes to Table 1.

Table 3: Program eligibility, income and view on *PANES* in 2007 (during the program)

Dependent variable:	Mean non-eligibles	Coefficient (s.e.)	Observations
1. Log household per-capita income	6.87	0.216*** (0.071)	2,031
2. Satisfaction with household situation	0.43	0.073*** (0.021)	2,079
<i>PANES</i> mistargeting:			
3. There are people who received <i>PANES</i> who should not have	0.91	-0.068** (0.030)	1,997
4. There are people who did not receive <i>PANES</i> who should have	0.98	-0.048*** (0.016)	2,024
5. Beneficiaries should have received less so that more people could benefit	0.87	-0.087*** (0.033)	2,024

Notes. The table reports results from regressions of various outcomes and survey responses on the program eligibility indicator. The specification is the same as the one in column 5 in Table 1. See also notes to Table 1.

Table 4: Program eligibility, income, participation in other program, and political and social attitudes in 2008 (post-program)

Dependent variable:	Mean non-eligibles	Coefficient (s.e.)	Observations
1. Log household per-capita income	7.12	-0.070 (0.067)	1,903
2. Durables	0.25	0.016 (0.013)	1,948
3. Received <i>Plan de Equidad</i>	0.34	-0.060 (0.041)	1,948
4. Received food card	0.04	0.141*** (0.032)	1,945
Positive opinion about:			
5. <i>PANES</i>	0.58	0.253*** (0.026)	1,916
6. <i>Plan de Equidad</i>	0.70	0.054** (0.024)	1,256
7. Relative to last year, are social differences greater?	0.51	-0.112*** (0.041)	1,729
Expectations:			
8. Of the households situation	0.64	0.045*** (0.016)	1,823
9. Of the country's situation next year	0.60	0.055*** (0.018)	1,798
Confidence in the:			
10. Ministry of Social Development	0.39	0.185*** (0.038)	1,732
11. President	0.37	0.091** (0.040)	1,854
12. Political parties	0.12	0.035 (0.028)	1,804
13. Social Security administration	0.47	0.022 (0.036)	1,812
14. Local councils	0.30	0.027 (0.036)	1,773
15. Parliament	0.21	0.017 (0.037)	1,370
16. National pride	0.79	0.049** (0.024)	1,900
17. Interest in politics	0.20	0.065** (0.031)	1,939
18. Believes that hard work pays off in life	0.35	0.022 (0.025)	1,910

Notes. The table reports results from regressions of various outcomes and survey responses on the program eligibility indicator. The specification is the same as the one in column 5 in Table 1. See also notes to Table 1.

Supplementary Appendix [NOT INTENDED FOR PUBLICATION]

Supplementary Appendix A: Additional figures and tables [not intended for publication]

Appendix Table A1: Human development and democracy in Uruguay and selected countries

	<i>UNDP Human Development Report 2007</i>				<i>The Economist Intelligence Unit democracy index</i>				
	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

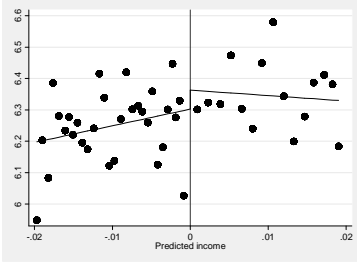
Source: UNDP (2007) and The Economist Intelligence Unit (2007).

Appendix Table A2: Description of categorical attitude variables

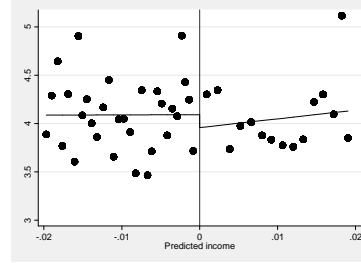
Variable	Range of values	Question wording (translated from Spanish by the authors)
Supports current government	1 to 3	Compared to previous government is the current government: 1: worse, 2: same, 3: better?
Positive opinion about <i>PANES</i>	1 to 5	At a general level how do you feel with respect to <i>PANES</i> : 1: very bad, 2: bad, 3: neither good nor bad, 4: good, 5: very good?
Positive opinion about <i>PE</i>	1 to 6	At a general level what did you think of the <i>PANES/PE</i> : 1: very bad, 2: bad, 3: decent, 4: neither good nor bad, 5: good, 6: very good?
Confidence in: President, Minister of Social Development, local councils, political parties, Social Security administration, Parliament	1 to 3	How much confidence do you have in __: 1 little, 2: some, 3: much?
Relative to last year, are social differences higher?	1 to 3	Relative to two years ago, do you think that social differences in Uruguay are: 1: lower, 2: the same, 3: higher?
Assessment of current household / country situation	1 to 5	What is the current situation of your household / the country: 1: very bad, 2: bad, 3: neither bad nor good, 4: good, 5: very good?
Expectation of future household / country situation next year	1 to 5	Next year, do you expect that the situation of your household/ the country will: 1: worsen very much, 2: worsen, 3: be the same, 4: improve, 5: improve very much?
National pride	1 to 4	How proud are you of being Uruguayan: 1: not at all, 2: little, 3: somewhat, 4: very?
Interest in politics	1 to 4	How interested are you in politics: 1: not at all, 2: not very, 3: somewhat, 4: very?
Hard work pays off in life	1 to 5	Do you believe that through hard work a poor person can make a lot of money: 1: Very much in disagreement, 2: in disagreement, 3: neither in agreement nor in disagreement, 4: in agreement, 5: very much in agreement

Appendix Figure A1: Program eligibility and baseline (2005) characteristics

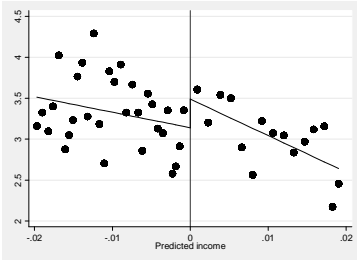
Panel A: Log per-capita income



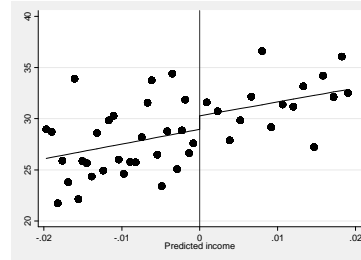
Panel B: Household average years of education



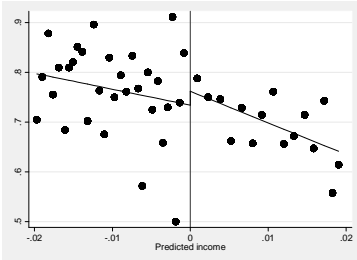
Panel C: Household size



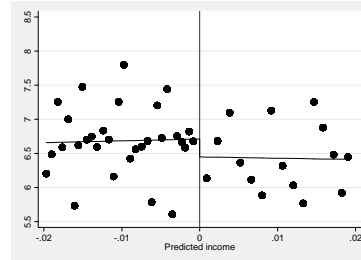
Panel D: Household average age



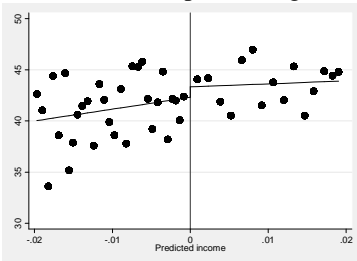
Panel E: Respondent is female



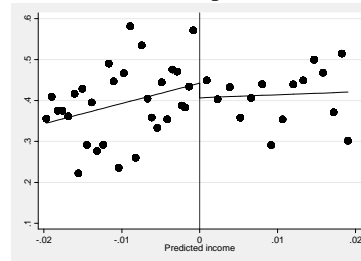
Panel F: Respondent years of education



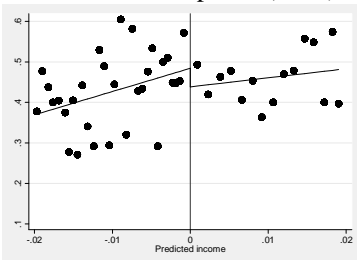
Panel G: Respondent age



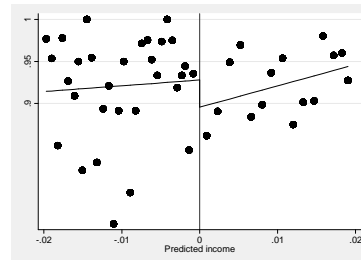
Panel H: Non-response (2007)



Panel I: Non-response (2008)

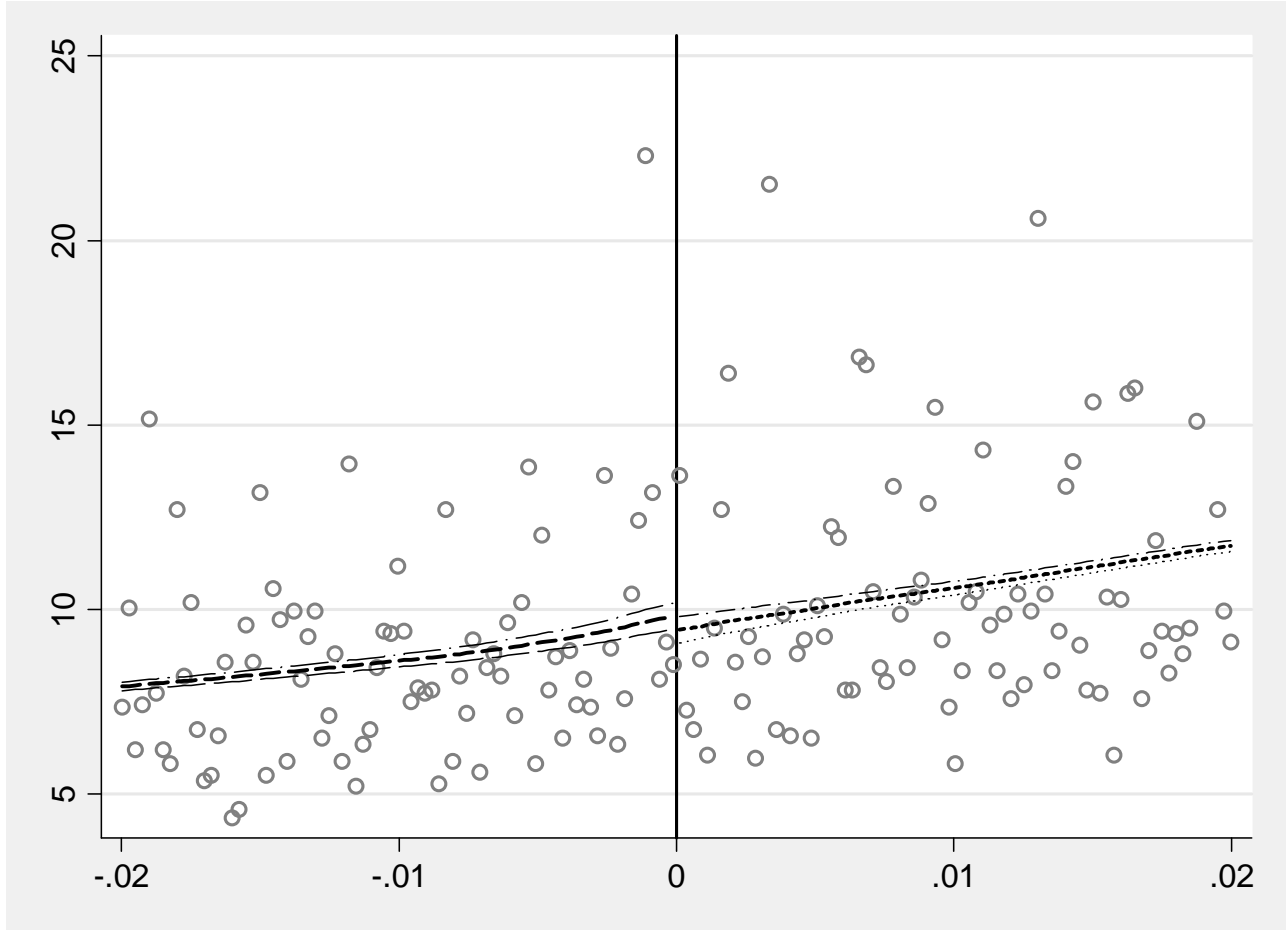


Panel J: Voted Last Elections



Notes. The graphs report the average value of a number of pre-treatment characteristics as a function of the standardized score. See Table 2 in the text for the analogous regression results.

.Appendix Figure A2: Distribution of the standardized *PANES* eligibility score



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

Supplementary Appendix B: PANES program components [not intended for publication]

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 (2007). Data refer only to households who report having participated in *PANES* at some point. 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 per month, equivalent to half the monthly minimum wage, and 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 (17.6%) 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 US\$800 depending on household demographic composition. Purchases could be made in authorized stores. The program covered around 71% of eligible households while participation among ineligibles was close to zero.

Around 16% of eligible households reported having participated in training and educational activities (*rutas de salida*) intended to foster social “inclusion” by strengthening work habits, promoting knowledge of individual rights and strengthening social ties. 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, and row 4 shows clearly that the aim of universal 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: regularization of beneficiaries’ connection to public utilities networks (water and electricity) for a nominal fee, in-kind transfers of building materials for home improvements; 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 13% 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 public health investments.

Appendix Table B1: Self-reported *PANES* take-up among beneficiaries, by component (%)

1.	Citizen Income	96.7
2.	Food card	70.9
3.	Public works employment	17.6
4.	Education and training	16.0
5.	Other components	12.7

Supplementary Appendix C: Voter learning about politician preferences: a model and calibration
[not intended for publication]

The goal of this subsection is to calibrate parameter values in a standard political economy model to assess whether it provides a reasonable fit to the *PANES* data. The attraction of this approach is that, although transfer levels fell in 2008 for former *PANES* beneficiaries, earlier transfers (from 2005-2007) may leave a legacy of greater government support if voters only partially updated their beliefs about future transfers downwards after the introduction of the *PE*.

The framework we develop, which is related to Drazen and Eslava (2006), assumes asymmetric information between voters and politicians, with imperfect knowledge of politicians' true redistributive preferences across population subgroups, i.e., those of different social classes, different regions, by gender, education, and disability, etc. Politician campaign promises are a form of cheap talk in the absence of a binding commitment technology, leaving room for uncertainty about these preferences, and thus noisy priors before the *FA* came into power. Voters then learn about politician preferences by observing the targeting of their social group in actual government programs, and update beliefs about politician redistributive preferences according to a standard Bayesian approach. Note that we refer to politician and political party preferences interchangeably in what follows. We leave an extension of this model that distinguishes between individual politician versus party preferences to future research.

The assumption that voters have poor information about the *PANES* targeting criteria is also critical in what follows. Individuals fully informed about the *PANES* targeting rule, who also knew their own predicted income score lay just to the right of the eligibility score threshold, should rationally deduce that their chance of receiving a future government program is effectively the same as a household located just to the left of the threshold. This would lead expected future transfers to be equal for both groups of households, and thus no meaningful difference in incumbent political support looking forward. However, these assumptions about voters' program knowledge seem unrealistic in this context. In the case of *PANES*, the opacity of the program targeting rule, which was not publicly released until the end of the program, means that the observed targeting of the program delivers only an imperfect signal about government preferences. This is true even for households, like those in our analysis, who lie near the program eligibility score threshold (for whom the program inclusion criteria might appear particularly unclear). Note that households were never provided with their predicted income score (the variable used internally for program assignment) and thus do not even know whether they were "close" to the threshold or not. It is also unrealistic for them to derive the formula on their own through personal observation (of themselves and other households in their social circle, say) given the many different household factors that entered into the predicted income model.

We begin by describing politicians' preferences in the model. The government in power has true preferences over net transfers to socio-demographic subgroup i denoted γ_i . The transfers to each group in an actual government transfer program in period t , g_{it} , yields a noisy signal of this underlying preference parameter: $g_{it} = \gamma_i + \mu_{it}$ where voters' prior belief on the preference parameter is distributed $\gamma_{i0} \sim N(\gamma_i, \sigma_0^2)$, and $\mu_{it} \sim N(0, 1)$. The assumption that $1/\sigma_0 < 1$ implies that prior beliefs are less informative than actual policies in capturing true politician preferences, perhaps due to the cheap talk problem alluded to above.

Bayesian updating by voters implies that voters' expected future transfer after t signals from actual government programs is:

$$(C1) \quad E_t[g_{i,t+1}] = \gamma_{i0} \left(\frac{1/\sigma_0^2}{1/\sigma_0^2 + t} \right) + \left(\sum_{s=1}^t \frac{g_{is}}{t} \right) \left(\frac{t}{1/\sigma_0^2 + t} \right)$$

where $E_t[g_{i,t+1}]$ captures expected future transfers at time $t+1$. Given the uncertainty in government targeting criteria and preferences, and voters' only partial information on program design, we assume below that voters use the transfer they personally receive as the signal of government redistributive preferences towards people "like them". Thus while voters are perfectly rational and use standard Bayesian updating, we assume they are operating in an environment with limited information on politician intentions and program implementation.

Expected voter utility from supporting a particular political party is a function of many factors, including voter ideology and a range of time-varying policies beyond transfers. In particular, voter expected utility from supporting the *Frente Amplio* is:

$$(C2) \quad V_{FA,it} = \pi_{FA,t} + bE_t[g_{i,t+1}] - \varepsilon_{it},$$

where overall population support for the *FA* in period t is captured by $\pi_{FA,t}$, the impact of future expected transfers targeted to group i is $bE_t[g_{i,t+1}]$, and ε_{it} denotes an idiosyncratic determinant of individual support for the *FA*, for instance from individual political ideology or other life circumstances, and is assumed to be distributed extreme value to allow for the use of the logit model. For simplicity, expected utility from supporting the opposition in period t is $V_{OP,it} = \pi_{OP,t}$.

We assume individuals vote sincerely, and also sincerely express their voting intentions on our surveys, convenient assumptions in political economy empirical work. Voter i supports the *FA* when $V_{FA,it} > V_{OP,it}$, or equivalently $a_t + bE_t[g_{i,t+1}] > \varepsilon_{it}$, where $a_t = \pi_{FA,t} - \pi_{OP,t}$. The logit solution, where the probability of supporting the *FA* government (opposition) is $P_{FA,it}$ ($P_{OP,it}$), is:

$$(C3) \quad \ln(P_{FA,it} / P_{OP,it}) = a_t + bE_t[g_{i,t+1}]$$

The empirical calibration is straightforward. We consider three time periods, where $t=0$ corresponds to the pre-*PANES* period, $t=1$ corresponds to the 2007 survey round (when *PANES* was still ongoing) and $t=2$ is the 2008 follow-up (when *PANES* had already ended). Households are assumed to receive *i.i.d.* signals about future government transfers in periods $t=1$ and $t=2$. These differ across *PANES* and *PE* program beneficiaries, with the average household transfer at US\$89.50 for *PANES* beneficiaries in $t=1$ and zero for non-beneficiaries (Appendix Table C1), and the average *PE* transfer is at US\$67.00 in $t=2$ for both the former *PANES* beneficiaries and non-beneficiaries now enrolled in *PE* (and zero for those not in *PE*), although some former *PANES* beneficiaries also continue to receive a food card valued at US\$19.50 per month. For simplicity, we assume that all beneficiary households receive the usual cash transfer (US\$70.00) plus a food card corresponding to having two children, of value US\$19.50 per month, although results are essentially unchanged with different assumptions.

The quasi-experimental variation in the *PANES* transfer allows us to identify the parameter b by comparing *FA* support between *PANES* beneficiaries and non-beneficiaries in 2007. The model laid out above implies that this difference in *FA* support is driven by differences in the transfers they expect to receive in the future, which is in turn determined by their past transfer experiences. Further assumptions are needed to pin down these expectations about future transfers and calibrate the model, most importantly on voters' prior beliefs at $t=0$ about the transfer they would receive from the *FA* and on the precision of this prior. We assume that both *PANES* beneficiaries and non-beneficiaries held a common prior on the government's redistributive preferences towards them, which is reasonable given the similarity of their

observed characteristics and the quasi-random assignment of the program near the threshold, and we set this transfer level γ_0 to 50% of the actual *PANES* transfer, or US\$44.75, although results do not depend on this assumption. We also assume that the prior precision is $1/\sigma_0^2 = 0.5$ as our leading case, and discuss robustness to a wider range of precisions, from 0.1 to 0.9. Model parameters are identified based on differences between program beneficiaries and non-beneficiaries, and thus to the extent that the prior on transfer levels is the same across the two groups, its precise level is essentially irrelevant for the purposes of the calibration.

To apply the binary logit framework, we group together voters who are indifferent between the *FA* and opposition with opposition supporters (although the results are similar if the indifferent are shifted to the *FA* camp, not shown). This yields a level of predicted support for the *FA* among beneficiaries in 2007 of 0.843 and among non-beneficiaries of 0.639 (Appendix Table C1), i.e. a difference in the log odds ratio of 1.101. Calibrating the model in the case of $1/\sigma_0^2 = 0.5$, we find that at $t=1$, *PANES* households expect a transfer of US\$61.58 in the next period while non-*PANES* households expect a transfer of US\$14.91, so a difference of US\$46.67. This yields a parameter estimate of $b = 0.024$ ($=1.101/46.67$, see Appendix Table C1). At time $t=2$, we use this estimate of b and the actual level of support among non-*PANES* beneficiaries in 2008 (57.4%) to pin down a_2 , and thus to predict *FA* support in 2008 among former *PANES* beneficiaries. As shown in the bottom row of Appendix Table C1, predicted *FA* support is 74.4% using a prior precision of $1/\sigma_0^2 = 0.5$, and ranges from 72.5% (with precision $1/\sigma_0^2 = 0.1$) to 75.7% ($1/\sigma_0^2 = 0.9$). These are very similar to the actual *FA* support of 74.6% reported among former *PANES* beneficiaries in the 2008 survey.

Appendix Table C1: Voter Learning Model Calibration Results

		FA support			
		----- Expected transfer (US\$) -----			
		(actual)	$1/\sigma_0^2 = 0.5$	$1/\sigma_0^2 = 0.1$	$1/\sigma_0^2 = 0.9$
Panel A:					
	<i>PANES</i> beneficiaries, in 2007 (t=1)	0.843	61.6	67.7	58.0
	<i>PANES</i> non-beneficiaries, in 2007 (t=1)	0.639	14.9	4.1	21.2
	Former <i>PANES</i> beneficiaries, in 2008 (t=2)	0.746	56.0	58.1	54.4
	Former <i>PANES</i> non-beneficiaries, in 2008 (t=2)	0.574	23.7	19.7	26.6
			----- Parameter estimates -----		
Panel B:			$1/\sigma_0^2 = 0.5$	$1/\sigma_0^2 = 0.1$	$1/\sigma_0^2 = 0.9$
Parameter estimates for:					
	B		0.024	0.017	0.030
	a_1		0.216	0.500	-0.067
	a_2		-0.265	-0.045	-0.503
			----- Predicted FA support (model) -----		
Panel C:		FA support	$1/\sigma_0^2 = 0.5$	$1/\sigma_0^2 = 0.1$	$1/\sigma_0^2 = 0.9$
	FA support in 2008 (t=2), former <i>PANES</i> beneficiaries:	(actual) 0.746	0.744	0.725	0.757

Notes. The table reports actual and predicted support for the *Frente Amplio* based on the model of Bayesian learning presented in Appendix C. The calibration exercise assumes monthly transfer amounts for *PANES* households of US\$89.50 in 2007, and US\$86.50 in 2008 among *PE* recipients (and zero for non-*PE* recipients). For *PANES* non-beneficiaries, these values are US\$0 in 2007 and US\$67.00 in 2008 among *PE* recipients (and zero for non-*PE* recipients). The latter is less than \$86.50 since they had not yet received the food card. The prior belief on future transfers at time $t=0$ is assumed to be half the transfer actually received by *PANES* households at $t=1$. The precision of the prior is denoted by $1/\sigma_0^2$.