Government Transfers and Political Support[†]

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This paper estimates 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 pretreatment 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. (JEL D72, H23, H53, I38, O15, O17)

T his 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 Gerald H. Kramer (1971); William D. Nordhaus (1975); Ray C. Fair (1978); and Morris P. 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 reelection, 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 (Gary W. Cox and Matthew D. McCubbins 1986; Assar Lindbeck and Jörgen W. Weibull 1987; James A. Robinson and Thierry Verdier 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]

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 (Gregory B. Markus 1988; Steven D. Levitt and James M. Snyder, Jr. 1997; Jowei Chen 2008, 2010; Mikael Elinder, Henrik Jordahl, and Panu Poutvaara 2008), with Cristian Pop-Eleches and Grigore 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 important, 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 intertemporal 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 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

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[1] A related literature explores the implications of voters' political ideology on political parties' transfer choices. Matz Dahlberg and Eva 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 (Norbert R. Schady 2000; Anne Case 2001; Tina R. Green 2006b). There is also evidence of direct vote buying in Latin America, including Frederic C. Schaffer (2007) and Susan C. Stokes (2005).

response to a 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 pretreatment 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 18 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 11–13 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 in the online Appendix) 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; Douglas A. Hibbs, Jr. 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 (George J. 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 nonbeneficiaries. Since the simplest form of myopic pocketbook voting implies that beneficiaries and nonbeneficiaries 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, John Ferejohn 1986; Torsten Persson and Guido Tabellini 2002; Timothy Besley 2006). Under asymmetric information about politician characteristics, voters use policy outcomes as signals to infer politicians' competence (as in Kenneth Rogoff 1990) or their preferences for redistribution toward particular social groups (as in Allan Drazen and Marcela 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 toward "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 United States that voters are only partially able to extract a signal of incumbent ability based on local economic outcomes (Justin Wolfers 2007), and with their tendency to punish incumbents for natural disasters (Shawn A. Cole, Andrew Healy, and Eric D. Werker 2008), and even shark attacks (Christopher H. Achen and Larry M. 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 (Matthew Rabin 1993; James C. Cox, Daniel Friedman, and Steven Gjerstad 2007) that are empirically relevant in real-world labor market situations (Uri Gneezy and John A. List 2006; Sebastian Kube, Michel André Maréchal, and Clemens Puppe 2010; Stefano DellaVigna 2009) have recently been shown to play a role in voters' political decision making (Frederico Finan and Laura 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 United Nations Development Programme (UNDP) (2007) Human Development Index, with high life expectancy and schooling indicators (Table A1 in the online Appendix),[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 center-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.

A. *PANES Objectives and Components*

In a context of rapid recovery, the new FA government swiftly implemented 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 UYU$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 percent 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 electronic debit card, whose monthly value varied between UYU$300 and US$800 (UYU$15 and US$41) depending on the number of children and pregnant women in the household. Around 70 percent of PANES beneficiaries eventually received the food card.[5]

[3] Between 2001 and 2002, per capita income fell 8 percent, the poverty rate increased from 18.8 percent to 23.6 percent, unemployment reached its highest level in 20 years (at 17 percent), 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.

[5] Nearly 85 percent of applicant households had at least one child and/or a pregnant woman. However, this component of the program took some time to be implemented due to logistical difficulties. Additional but less common

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 percent of all Uruguayan households and 14 percent 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 (Rodrigo Arim, Verónica Amarante, and Vigorito 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]

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components included public works employment opportunities, education and training, and health care subsidies (further details on PANES are in online 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 percent of gross domestic product (GDP) and 1.95 percent 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 enrollment.

[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: an indicator for public employees in the household, an indicator for pensioners in the household, average years of education of individuals over age 18, the number of household members, the presence of children by age group (0–5 and 12–17), an indicator variable for whether a member of the household had private health insurance, 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 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. César Martinelli and Susan W. 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 UYU\$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 UYU\$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.
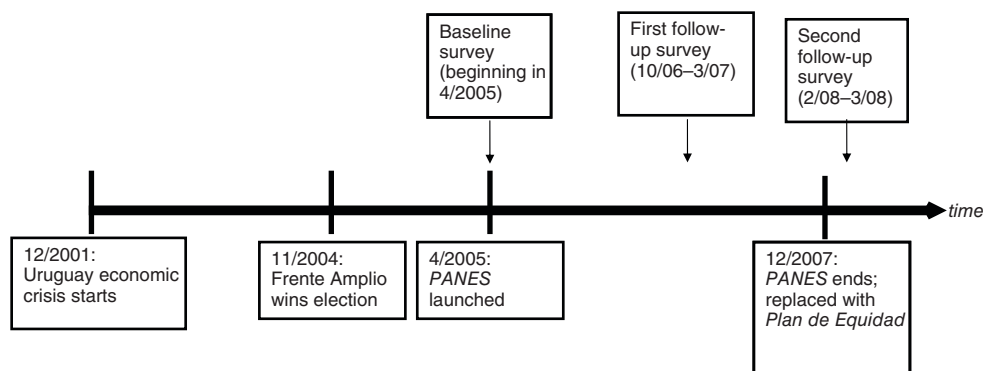
Figure 1. PANES Program and Data Collection Timeline

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.

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 18 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.

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,

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[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. We only use households whose score was computed based on the new formula.

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 2008 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 nonrecipients 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 2 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 nonresponse rate was relatively high, at 36 percent, replacement households with roughly the same score as the nonresponse households were subsequently interviewed. We discuss the implications of nonresponse below.

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 are 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 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

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[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.
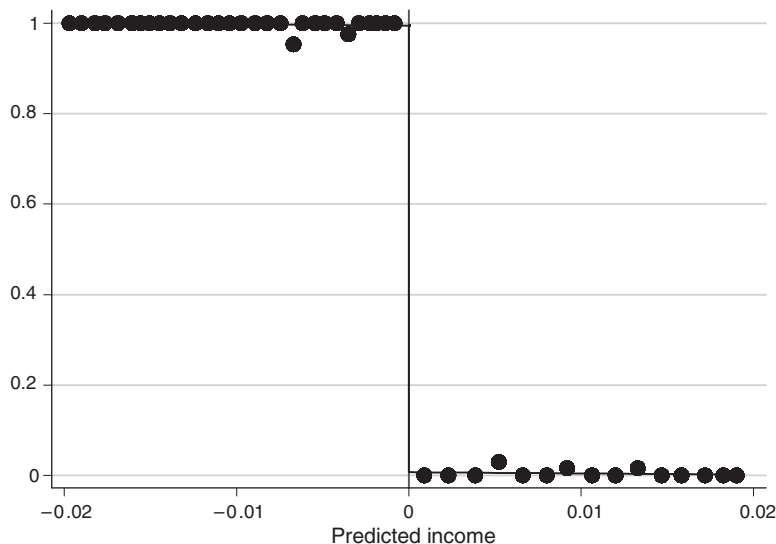
FIGURE 2. PANES PROGRAM ELIGIBILITY AND PARTICIPATION

Notes: The figure 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.

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 nonrandom 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.

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

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 pre-
vious government) as a function of the standardized score. The fitted plots are linear best fits
on each side of the eligibility threshold.

Source: First PANES follow-up survey (2007).

A. *Impact on Reported Support for the Government during the Program*

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 pre-
dicted 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 ineli-
gible group. As of 2007, PANES households were significantly more pro-govern-
ment: 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.

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[12] The questionnaire presents responses in the following order "*1: the same, 2: worse, 3: better.*" Online
Appendix Table A2 provides exact wording (translated) and codes for this question and the other main survey ques-
tions 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).

TABLE 1—PANES PROGRAM ELIGIBILITY, PARTICIPATION, AND POLITICAL SUPPORT FOR THE GOVERNMENT

| Dependent variable | Mean non-eligibles | Coefficient (standard error) | | | | | | Observations |
		(1)	(2)	(3)	(4)	(5)	(6)	
1. Ever received PANES, 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 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 1–3 include, in order, a polynomial in the standardized score of degree 0, 1, and 2, and these polynomials interacted with the eligibility indicator. Columns 4–6 additionally control for pretreatment 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 parentheses. Standard errors are almost identical (differing by roughly 1 percent) with the jackknife approach in Justin McCrary (2008b).

  ***Significant at the 1 percent level.
  **Significant at the 5 percent level.
  *Significant at the 10 percent level.

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 David S. Lee and David 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 $f_1(0) = f_2(0) = 0$:

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

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 1–3 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 percent.

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
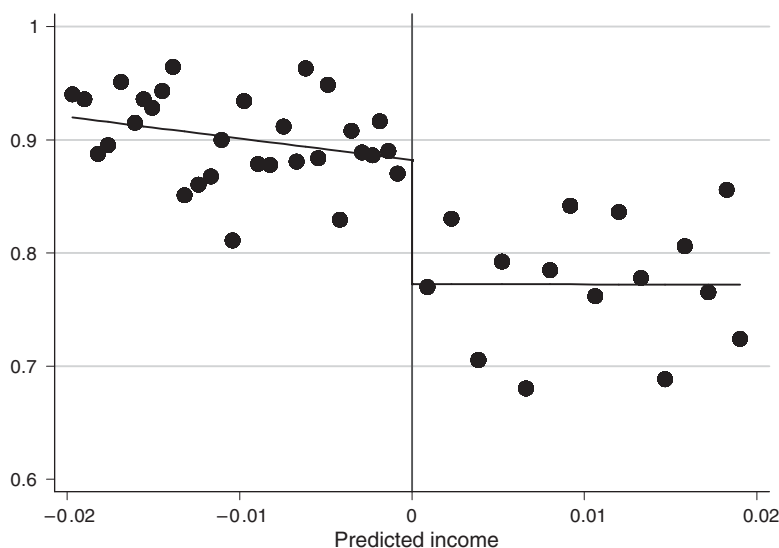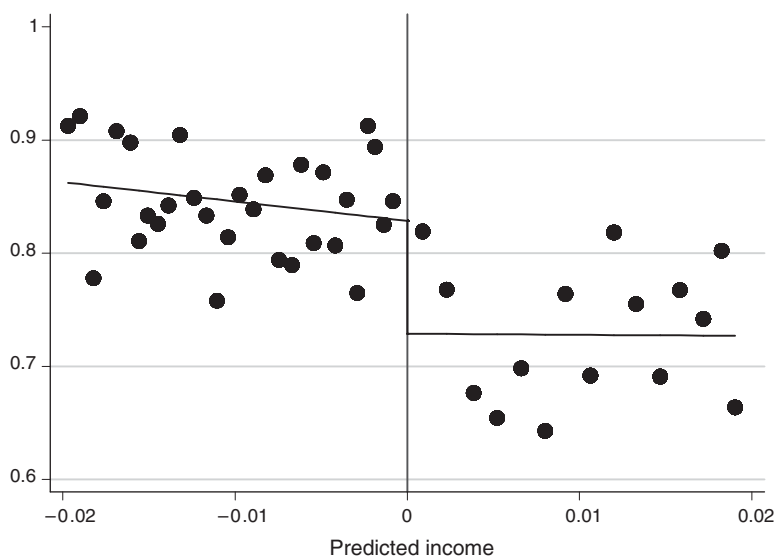
FIGURE 4. PANES PROGRAM ELIGIBILITY AND POLITICAL SUPPORT FOR THE GOVERNMENT,
2008 FOLLOW-UP SURVEY ROUND

Notes: The figure reports the average support for the current government (compared to the previous government) as a function of the standardized score. The fitted plots are linear best fits on each side of the eligibility threshold.

Source: The second PANES follow-up survey (2008).

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 and 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).

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 1–6.[13] This result is displayed graphically in Figure 4. The main implication is that the program

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[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 unstandardized 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 percent confidence and are remarkably similar to those in Table 1 (not shown).

generated persistent impacts on political support for the government, suggesting that past transfers also factor meaningfully into voters' decision making.

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 Adriana Camacho and Emily Conover 2007). Unfortunately, we lack data on baseline household political orientation, which prevents us from directly testing this alternative hypothesis; however, a variety of evidence makes it 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 nonrandom 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 Figure A1 in the online Appendix). 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 pretreatment 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

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[14] Households were able to apply to PANES for 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.

TABLE 2—PROGRAM ELIGIBILITY, BASELINE CHARACTERISTICS AND RESPONSE RATES IN 2005 (*Pre-Program*)

Dependent variable	Mean noneligibles	Coefficient (standard error)	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. Nonresponse/missing response on political support question (2007)	0.41	0.037 (0.044)	2,372
9. Nonresponse/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 pretreatment (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.

national election (row 10) was 92 percent for ineligible households and 93 percent for the eligible households (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 nonparametric distribution of the standardized score. If manipulation occurred so that some ineligible households were misassigned a low predicted income score, one would expect excess bunching of households just below the threshold (John DiNardo and Lee 2004; McCrary 2008a). Figure A2 in the online Appendix 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 nonresponse rates could be systematically related to program eligibility. This could be a concern even though nonrespondents were in practice replaced by households with a similar predicted income score if nonresponding

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[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 nontrivial fine.

[16] The point estimate of the log difference at the threshold in Figure 3 is just 0.041 (standard error 0.027).

households differ in their unobservables. In rows 8 and 9 of Table 2, we report the relationship between survey nonresponse (defined as either not responding to the survey at all, which is 36 percent 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 percent) and PANES eligibility among households in 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 nonresponse.

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 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 as 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" $\frac{1}{2} =$ "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 percent confidence).[17] Moreover, we matched up *Latinobarómetro* data to actual vote share at the Uruguayan *departamento* level, roughly equivalent to a US 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

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[17] *Latinobarómetro* is a survey conducted every year in 18 Latin American countries by the *Latinobarómetro* Corporation, a nonprofit 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 David Rothschild and Wolfers (2010), Donald Granberg and Sören Holmberg (1990), among many others.

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 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.

E. *Greater Support among Recipients—or Bitterness among Nonrecipients?*

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 use the predictions from this model to

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[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).

[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 zero and one. Note that there is no question on confidence in the President in the 2008 *Latinobarómetro*, unfortunately.
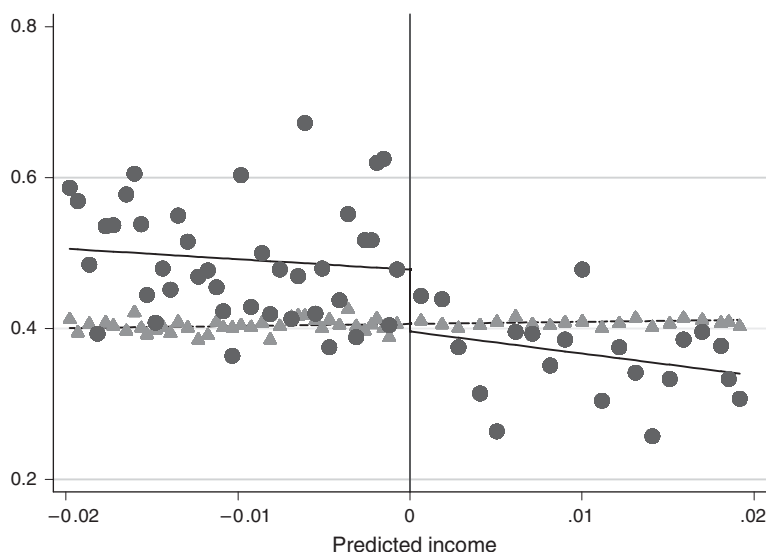
FIGURE 5. CONFIDENCE IN PRESIDENT: ACTUAL (*circles/solid line*) AND PREDICTED BASED ON *LATINOBARÓMETRO* (*triangles/dashed line*)

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.

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 percent 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.

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[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.

TABLE 3—PROGRAM ELIGIBILITY, INCOME, AND VIEW ON PANES IN 2007 (*During the program*)

Dependent variable:	Mean noneligibles	Coefficient (standard error)	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
PANES mistargeting:			
3. There are people who received PANES who should not have	0.91	−0.068** (0.030)	1,997
4. There are people who did not receive PANES 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.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

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.

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 sizable program impacts on log per capita household income (including transfer income) in 2007, on the order of 22 percent (in row 1).[22] In addition to the income transfer,

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[22] Note that self-reported per capita income grew by a remarkable 25 percent 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 (UYU\$452) among PANES beneficiaries are smaller than the transfer amount (UYU\$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 canceled, leading to no discernible program effect on work hours. This limited adult labor supply response is consistent with results from Mexico's *PROGRESA* program (Susan W. Parker and Emmanuel 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 percent), to pay utility bills (10 percent), and to repay debts or loans (10 percent). 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).

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 nonbeneficiaries (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 recipients express in 2008 is thus strong evidence against simple pocketbook voting.

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 nonbeneficiaries by early 2008. However, there is no effect of the program on household durable 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 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 percent.[27]

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[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 rescale this and all questions that follow from 0 to 1, as described in online 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 percent of PANES beneficiaries believed the program was permanent, while 57 percent 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.

[25] The assets measured include: heaters, stoves, microwaves, refrigerators, washing machines, dishwashers, TVs, VCRs, 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 UYU$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).

TABLE 4—PROGRAM ELIGIBILITY, INCOME, PARTICIPATION IN OTHER PROGRAM, AND
POLITICAL AND SOCIAL ATTITUDES IN 2008 (*Post-Program*)

Dependent variable	Mean noneligibles	Coefficient (standard error)	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 *Plan de Equidad*	0.34	−0.060 (0.041)	1,948
4. Received food card	0.04	0.141*** (0.032)	1,945
Positive opinion about:			
5. PANES	0.58	0.253*** (0.026)	1,916
6. *Plan de Equidad*	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.

  ***Significant at the 1 percent level.
  **Significant at the 5 percent level.
  *Significant at the 10 percent level.

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, standard error 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, standard error 0.044, not shown in table).

C. *Rational but Poorly Informed Voters*

In this subsection, we argue that a model of voter learning about politician preferences, which is formally presented in online 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 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.

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 subsection 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 reshuffling 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 nonbeneficiaries

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 nonbeneficiaries. 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 3–5). 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 self-serving beliefs among transfer recipients are 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 percent, relative to nonbeneficiaries (at 58 percent, 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, standard error 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.

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 nonbeneficiaries. 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, standard error 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, standard error 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, standard error 0.031, row 17).

F. *Behavioral and Psychological Explanations*

We argue above (and in online 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 nonstandard, 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, standard error 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 find large and robust effects—on the order of 11–13 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

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[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 percent, implying that left-leaning individuals are indeed less likely to believe that hard work pays off in life (not shown).

[29] Rafael Di Tella, Sebastian Galiani, and Ernesto Schargrodsky (2007) reach a similar conclusion about the long-run impacts of a land reform program in Argentina.

scant (as we argued in the introduction), the Uruguayan government's decisions still suggest that their objective function included factors beyond immediate reelection motives. In the case of the Uruguayan government in 2005 when PANES was launched, this may have included an ideological goal of extensive redistribution toward 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 its 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 a signal of government redistributive preferences. 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 the 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 toward 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 nonbeneficiaries, 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 nonbeneficiaries 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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