NBER WORKING PAPER SERIES

GOVERNMENT TRANSFERS AND POLITICAL SUPPORT

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Working Paper 14702
http://www.nber.org/papers/w14702

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
February 2009

We are grateful to Uruguay's Minister for Social Development, Marina Arismendi and her staff, in particular Marianela Bertoni and Lauro Meléndez at the Monitoring and Evaluation Unit, for making this research possible; to Gabriel Burdín, Adriana Vernengo and James Zuberi for excellent research assistance; and to Alberto Alesina, Verónica Amarante, Gary Becker, Michael Boskin, David Card, Stephen Coate, Raj Chetty, Ernesto Dal Bo, Stefano DellaVigna, Caroline Hoxby, Brian Knight, Botond Koszegi, Justin McCrary, Susan Parker, Rohini Pande, Matt Rabin, Gerard Roland, and seminar participants at Columbia University, LSE, U.C. Berkeley ARE, the NBER Political Economy and Public Finance groups, the Universidad de la República (Uruguay), USC, the 2008 CEPR European Summer Symposium in Labor Economics, the CEPR Public Policy group meeting, Stanford, the University of Chicago, RAND, Michigan, Center for Global Development, Universitá Bocconi, Paris School of Economics, Colegio de Mexico, ITAM and Washington University for comments. Marco Manacorda gratefully acknowledges hospitality from the British Embassy in Montevideo and the Government of Uruguay. Some of the data analyzed in this article were collected by Latinobarómetro Corporation. The Latinobarómetro Corporation is solely responsible for the data distribution and it is not responsible for the views expressed by the users of the data. The authors appreciate the assistance in providing these data. The views expressed in this paper are the authors' own and do not necessarily reflect those of the Government of Uruguay, the Latinobarómetro Corporation, or the National Bureau of Economic Research. All errors remain our own.

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NBER Working Paper No. 14702
February 2009, Revised November 2009
JEL No. D72,H53,O12,O23

ABSTRACT

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 14 percentage points more likely to favor the current government relative to the previous government. Political support effects persist after the program ends. A calibration exercise indicates that these persistent impacts are consistent with a model of rational but poorly informed voters learning about politicians' redistributive preferences.

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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 political preferences. We exploit the quasi-random assignment to the program based on a sharp discontinuity in an underlying eligibility score in order to identify the effect of receiving a government transfer on support for the incumbent government, and to ultimately advance our theoretical 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, and an extensive literature documents how voters reward incumbents for desirable outcomes of government policies. 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), Stigler (1973), Fair (1978) and Fiorina (1981), many scholars have studied voters' responsiveness to macroeconomic conditions and policies. A robust empirical finding in many countries is that economic conditions around election time have predictive power for the incumbent's re-election success. This observation underpins the theory of political business cycles, namely that incumbents seeking re-election can strategically manipulate the economy via expansionary fiscal policies to win votes, a claim that squares well with the observation that pre-election periods are indeed characterized by higher government spending and public goods provision (see Hibbs, 2006 for a review). 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 conditions, and in particular targeted government transfers, on the evolution of voter preferences. While it is conventional political wisdom that targeted government programs sway votes, and thus could be used strategically by incumbents seeking re-election, there is still little convincing evidence on the magnitude of these effects. Just as importantly, little is known about the mechanisms that underpin the exchange of votes for transfers between voters and politicians. The secrecy of the ballot in modern democracies makes "vote-swaying" through targeted government transfers impossible to enforce due to intertemporal commitment problems.

Inherent empirical difficulties have limited progress in identifying the impact of targeted transfers on political preferences. Both observational studies (Markus, 1988), and the few studies that seriously attempt to tackle causality issues (Levitt and Snyder, 1997, Chen, 2008, Elinder et al., 2008) suggest that personal economic conditions affect voting behavior, although some of this evidence is contested (Green, 2006a).[1] 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 (Lindbek and Weibull, 1987, Cox and McCubbins, 1984, Verdier and Snyder, 2002), one might find a spurious positive correlation between transfer receipt and political support that does not constitute a causal effect of the former on the latter, as parties make tactical decisions about 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.[2]

While the empirical claim that voters respond retrospectively to macroeconomic conditions and government transfers is largely accepted, despite the econometric concerns described above, considerable disagreement also exists on the interpretation of these patterns, including the true causal magnitudes, and their implications for theories of voter preference formation. An early model of voter decision-making based on naïve "adaptive retrospection" fits the data and can in principle generate a political business cycle (Nordhaus, 1975), but it suffers from logical weaknesses: if voters respond adaptively, strategic governments can systematically "fool" voters by adopting expansionary policies on the eve of re-election. Yet aware of politicians' distorted incentives, rational voters should dismiss such policies as inconsequential to their welfare, eliminating politicians' strategic incentive to engage in expansionary policies.

[1] Levitt and Snyder (1997) study the effect of spending at the district level on voting behavior in U.S. House of Representatives elections. They instrument spending in each district with spending in neighboring districts within the same state, and find a positive effect of non-transfer federal spending on the incumbent's vote share, but surprisingly no effect of transfer spending. A possible concern with their IV strategy is a violation of the exclusion restriction, for instance, if spending on roads or military bases in nearby districts directly affects voters' choices. Sole-Olle and Sorribas-Navarro (2008) use a similar approach and estimate positive impacts of government spending on incumbent support in Spain. Chen (2008a, 2008b) estimates the impact of transfers on voting in the U.S. Chen uses aggregated voting data and relies on the quasi-random path of hurricanes to predict federal transfers. Green (2006a) uses the discontinuity in assignment to *Progresa* across Mexican communities to estimate voting impacts, and finds a slightly larger incumbent vote share in treated communities but this pattern is also present before the program, suggesting endogenous political selection of beneficiaries rather than a causal impact.
[2] 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).

Springing from the 1980s rational expectation revolution, several scholars have shown how political business cycles can arise even in the presence of "rational" voting. In this view, past economic performance matters to rational voters to the extent they help predict future outcomes. While some versions simply rely of wage stickiness and output surprises to produce booms and busts around election times (e.g. Alesina et al., 1993), micro-founded theories of rational business cycle embedded in models of asymmetric information and politician career concerns (like Ferejohn, 1986, Rogoff, 1990, Persson and Tabellini, 2002, chapter 4, Besley, chapter 3) suggest in particular that incumbents use expansionary policies to signal their quality or competence to voters. In a world with asymmetric information about incumbent competence, good macroeconomic performance serves a useful signaling role, and, as in the literature on CEO compensation (Bertrand and Mullainathan, 2001), elections provide high powered incentives for politicians with career concerns to exert greater effort. In such models, retrospective voting is fully consistent with rational forward-looking voter decision-making.

A related but distinct rationale also relying on asymmetric information between politicians and voters has been invoked to explain why targeted transfers might affect rational voters' choices: government transfers could act as a signal to imperfectly informed citizens about incumbents' preferences for different socio-demographic groups. In this spirit, Drazen and Eslava (2006) develop a formal model where it is rational for voters targeted by government transfers to have stronger political preferences for the incumbent, as they presumably learn that their population subgroup is also more likely to be favored by that party again in the future.[3]

The assertion that voters are fully rational, in the sense that they gather and process all relevant information to maximize expected future outcomes when making their political choices, is arguably a blunt view of reality, especially in light of the growing body of empirical evidence from psychology and economics demonstrating more complex individual motivations (DellaVigna, 2009). Some empirical evidence in political economy also increasingly leans against the textbook models of perfectly rational retrospective voters discussed above. Alesina et al. (1993) reject the rational retrospection model using data on U.S. presidential elections in the context of a structural model, and Wolfers (2009) shows that U.S. voters are only partially able to extract a signal of incumbent ability based on local economic outcomes. That voters are

[3] A growing empirical literature shows that politician identity strongly affects policy preferences, most notably Pande (2003) and Chattopadhyay and Duflo (2004).

unable to perfectly assess government policies and politician competence is similarly attested to by their tendency to punish incumbents for natural disasters (Cole et al., 2008) and even shark attacks (Achen and Bartels, 2004). The issues of reciprocity, fairness and gratitude (Rabin, 1993, Cox et al., 2007) that have been shown to be empirically relevant in real-world labor market situations (Gneezy and List, 2006, Kube et al., 2007) could plausibly also play a role in the evolution of voters' political decision making

To contribute to the understanding of voter decision-making, in this paper, we measure the extent of voters' responsiveness to targeted public transfers by exploiting the quasi-random assignment of a large anti-poverty program in Uruguay, together with individual micro-data on expressed support for the incumbent government. In March 2005, against the backdrop of an economic crisis, a center-left coalition took power in Uruguay for the first time and swiftly launched a large anti-poverty program, called *PANES*. The main components of *PANES* were a cash transfer and a food card (basically a debit card pre-loaded with a certain sum of money that could be spent only on food). Unlike some recent Latin American anti-poverty programs (notably *Progresa/Oportunidades* in Mexico), *PANES* was conceived as a temporary program from its inception. Household eligibility for the program was determined by a predicted income score based on a large number of pre-treatment covariates. Only households with scores below a predetermined threshold were eligible for *PANES*. 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, and a second similar follow-up survey took place the following year. Because assignment to the program near the threshold was nearly "as good as random", we are able to circumvent the problems of reverse causality, endogenous political selection, and omitted variables highlighted above to more reliably estimate the impact of transfers on political preferences.

To preview our main findings, program beneficiaries are much more likely to support the incumbent than non-beneficiaries, by 11 to 14 percentage points, and these effects are statistically significant at high levels of confidence and pass numerous regression discontinuity validity checks. Uruguay has highly developed democratic political institutions for a middle-income country, suggesting that some of the political findings could also be relevant for wealthier countries. In a novel twist that helps us understand voter motivations and overcome certain misreporting concerns, we also use post-program survey data to show that the positive impacts on support for the incumbent persist even in the year after the program ended.

We next adapt a formal model of rational but poorly informed voters learning about politicians' redistributive preferences to assess whether this framework can rationalize the main empirical patterns, and in particular account for the persistent impacts on political support into the post-program period. An empirical exercise indicates that the estimated impacts on political support for the government due to the transfer program, and, importantly, the persistence of these effects into the year after the program ends, can in fact be reconciled with this model. Though we cannot definitively disentangle this rational learning model from behavioral explanations relying on reciprocity, and both plausibly play a role in driving our findings, this exercise indicates that a standard political economy framework can in fact go a long way towards rationalizing voter decision-making.

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 presents a formal model of voter learning about politician redistributive preferences and fits it to the program data, providing insights into the channels behind the increase in political support. The final section concludes.

I. THE *PANES* PROGRAM IN URUGUAY

Uruguay is a small Latin American country, home to 3.3 million individuals, half of whom live in the capital of Montevideo. The country experienced rapid economic growth in the first decades of the twentieth century, and was among the first countries in the region to complete the demographic transition, implement universal primary education, and establish a generous old age pension system. Although economic growth stagnated in the second half of the century, Uruguay is still among the most developed Latin American countries according to the UNDP Human Development Index, with strong life expectancy and schooling indicators (Supplementary Appendix Table A1). Currently, PPP-adjusted annual per capita income is just below US$10,000. According to *The Economist* Intelligence Unit, the country's political system has low levels of corruption and free and fair elections.[4]

[4] *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).

After years of economic stagnation, the country experienced a severe economic crisis in 2002. 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. The crisis laid bare the weakness of the existing social safety net, which was largely focused on transfers to the elderly population.[5] Yet constrained in part by a severe fiscal adjustment, the ruling center-right *Colorado* party government (which had been in power since 1999 in coalition with the *National* party) focused on expanding existing programs rather than adopting new measures, with the exception of a small emergency food plan.

The left-wing *Frente Amplio* (*FA*) coalition took power in March 2005 after winning the October 2004 elections, capitalizing on widespread dissatisfaction with the economy and the previous government's management of the crisis. The *FA* campaigned on a platform of extensive pro-poor redistribution and structural economic reforms. The new *FA* government swiftly created the Ministry for Social Development (*Ministerio de Desarrollo Social*, *MIDES*) and implemented the National Social Emergency Plan (*Plan de Atención Nacional a la Emergencia Social*), or *PANES*, which we study.

I.a *PANES* objectives and components

The *PANES* program was designed to be temporary, running from April 2005 to December 2007, and had two main aims: first, providing direct assistance to households that had experienced a rapid deterioration in living standards since the onset of the 2001-2002 crisis; and second, and in light of rising poverty during the 1980s and 1990s, strengthening the human and social capital of the poor, to enable them to eventually climb out of poverty on their own. *PANES* is the most ambitious and generous anti-poverty program in the country's history, and was heavily publicized by the government and the mass media.

The target population consisted of the poorest households in the country, namely the bottom quintile of households falling below the national poverty line. *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

[5] In 2002, total expenditure on elderly pensions represented 65% of all government social expenditures, 96% of cash transfers and almost 13% of GDP. This is reflected in marked differences in poverty incidence by age: while nearly half of children under age five lived in poverty that year, the rate for those 65 and older was only 8% (UNDP, 2008).

exchange rate), independent of household size. This is a very large transfer for the target population, amounting to more than 50% of average pre-program household self-reported income among program applicants.[6] Households with children or pregnant women were also entitled to a food card (*tarjeta alimentaria*), an in-kind transfer that operated through an 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 also received the food card.[7] 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).[8]

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

Participant enrollment occurred in stages. All low income households were first publicly invited to apply via mass media outlets, and the government also made a large outreach effort, sending enumerators to poor communities with the intent of boosting applications. After an initial enrollment phase, where applicants self-reported their income and household size, the 188,671 applicant households were then visited by *MIDES* personnel and administered a detailed baseline survey, providing information on household characteristics, housing, income, work, and schooling, characteristics that were used to determine program eligibility. Of the 188,671 applicant households, 102,353 households eventually became program beneficiaries, nearly 10% of all Uruguayan households.

To determine program assignment, the government used a predicted income score that depended only on household socioeconomic characteristics collected in the baseline survey, not directly on self-reported reported income itself. 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. The

[6] 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.
[7] 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. This explains why by the beginning of 2007 only around 70% of beneficiary households report having received a food card.
[8] 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.

underlying 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.[9] Only households with predicted income scores below a predetermined threshold were assigned to program treatment.

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 wide array of socioeconomic characteristics in the income score, as opposed to self-reported income, the government hoped to minimize strategic misreporting.[10]

This discontinuous rule for program assignment was suggested to Ministry officials by the authors of this paper with the explicit goal of carrying out the prospective evaluation of *PANES*. Government officials proved remarkably receptive to the proposal and were 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*). Similarly, neither the enumerators nor households were ever informed about the exact variables that entered into the score, the weights attached to them, or the program eligibility threshold, easing concerns about manipulation of the score and also making the program assignment process somewhat opaque to both enumerators and applicants.[11] Moreover, the score was developed in August-

[9] 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 were also allowed to vary slightly across the country's five main administrative regions to entitle similar numbers of poor households in each area of 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).

[10] Martinelli and Parker (2009) discuss the risks of under- and over-reporting of assets in the context of a similar anti-poverty program eligibility score in Mexico.

[11] 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

September 2005, months after the baseline survey was collected from households in our sample, the timing making it impossible for enumerators or households to know exactly how to manipulate surveys for a formula that did not yet exist. The eligibility score components and weights were eventually made public on the *MIDES* website after the program ended.[12] The program was fully rolled out within a year of its launch in April 2005. 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.[13]

The *PANES* program was designed to be temporary ending in December 2007 (see Figure 1). In January 2008, *PANES* was replaced by a new transfer program, the *Plan de Equidad* (*PE*). *PE* was part of a broader tax and social program reform, and also included a generous cash transfer for poor households with children, with an average *PE* monthly transfer of UY$1,300 (US$67 at the January 2008 real exchange rate), nearly the *PANES* level. 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 the new *PE* program received a written formal communication from *MIDES* about their inclusion in the new program.

PANES beneficiaries and non-beneficiaries in our sample, near the original *PANES* eligibility threshold, were equally likely to receive *PE* transfers in 2008, as discussed further

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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. There was an additional participation condition that in practice disqualified around 10% of applicants: 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 for. Household income for eligibility purposes was computed as the maximum of self-reported income and earnings reported in official social security records. Hence, the predicted income score was not even computed for households with income exceeding that threshold. All participating households were informed of this rule before applying. Beneficiary households whose social security earnings later exceeded the UY$1,300 threshold eventually lost eligibility.

[12] 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. Despite non-enforcement of the conditionalities, most beneficiaries were aware of their existence: 56% of beneficiary households report knowing of at least one program conditionality.

[13] Although payment did not begin until the second half of 2005, beneficiaries were paid arrears back to enrolment.

below. The *PE* program also involved expansion of food card coverage to all *PE* households, whether earlier in *PANES* or not. However, the implementation of this additional component was delayed for new household until mid-2008, and thus in early 2008 when our second follow-up survey was carried out the former *PANES* households (among *PE* beneficiaries) were still receiving their food card while non-*PANES* households were not.

I.c. Follow-up surveys in 2007 and 2008

Figure 1 presents a timeline of the *PANES* program and data collection. 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 the Economics and Sociology Departments at the University of the Republic. The University departments were also in charge of data collection. To exploit the discontinuity design, the original survey sample contained data on approximately 3,000 households, including both eligible and ineligible applicants, in the neighborhood of the program eligibility threshold score (households with a predicted probability of being below the target income level within two percentage points of the cutoff eligibility score). There was a desire to over-represent eligible households, leading the sample to be split between eligible and ineligible households in a 2:1 ratio.[14] 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. 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 around 1,100 households.[15] Overall, the analysis sample contains complete data on the remaining 1,942 households.

In addition to information on housing, household composition, durables, work, income and schooling, the survey collected information on health, economic expectations, knowledge of

[14] This main sample was supplemented with data on 500 eligible households farther away from the eligibility threshold, although we do not use these data in the discontinuity analysis in this paper.
[15] These excluded households were used in an earlier version of this paper, and the main political support impacts and statistical significance levels remain unchanged (not shown).

political rights, participation in social groups, opinions about the *PANES* program, and political attitudes, including support for the government, our key outcome variable.

A second follow-up household survey round was collected in February and March 2008, three months after *PANES* had ended and had been replaced by the *PE* (Figure 1). It was similar to the first follow-up survey, with the addition of several questions on respondents' social and political attitudes, for instance, a question on respondents' national pride as a Uruguayan. Attrition across follow-up rounds is a minor concern, as 92% of households from the first follow-up round were successfully re-surveyed in the second follow-up, and was balanced across *PANES* and non-*PANES* households.

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 figure is based on program administrative records. The score was normalized so that all figures are centered on zero, the eligibility threshold, and such that predicted income increases moving to the right on the horizontal axis.[16] 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 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.[17]

[16] 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.

[17] 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)..

Although the program included a variety of components, we do not attempt to disentangle what roles these different elements played in shaping outcomes since there was potentially non-random selection into some of them. We concentrate 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. RESULTS

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). A concern is manipulation of program assignment by either officials or enumerators, due to strategic responses or a correlation between survey non-response and political views. We also begin to highlight the channels through which *PANES* affects attitudes by investigating post-program income and participation in other programs (Table 3).

II.a. Impacts on political support for the government

We use the following question from the follow-up survey to measure support for the incumbent government: "*In relation to the previous government, do you believe that the current government is worse (0), the same (1/2), better (1)?*".[18]

Figure 3 presents support for the government as a function of the normalized predicted income score. The discontinuity at zero provides an estimate of the proportion of individuals who support the current government in the *PANES* eligible group versus the ineligible. *PANES* households are significantly more pro-government: among eligible households support for the current government is around 91%, compared to 77% for ineligible households (still a high level of support, as might be expected since the left-wing coalition is widely supported by the poor). The estimated discontinuity implies that program eligibility is associated with a 14 percentage

[18] 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 also 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).

point increase in support for the government over the opposition coalition. This provides evidence that households' political views are responsive to government transfers.

The outcome measure we use merits further discussion. One concern is that the question refers to preferences for the incumbent relative to the previous government, not to the current opposition coalition, hence 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 survey voting intentions, although these were not collected in either the baseline or follow-up surveys, since *MIDES* feared this would appear inappropriate.

To circumvent these difficulties, we examine several other useful measures of political attitudes contained in our surveys, including a question in the 2008 follow-up on "confidence in the President" (coded *0*="Little" *1/2*="Some confidence", *1*="A lot"). The estimated discontinuity in this variable at the threshold is similar to the impact on *FA* support, at 9 percentage points, and is highly statistically significant (section III.c below contains further discussion). We included this "confidence in the President" question in the survey in part because a question with the exact same wording is included in nationally representative *Latinobarómetro* surveys, allowing us to evaluate how it correlates with voting intentions and actual votes.[19] The Uruguayan electoral system is presidential (with proportional representation in Congress), so confidence in the President is a compelling measure of voting intentions. Indeed, in the *Latinobarómetro* survey (2005-2007), the correlation between confidence in the President and stated *FA* voting intentions is very strong, at 0.50 (statistically significant at 99% confidence). 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 for the *FA*, expressed support and actual votes are likely to be closely related.

[19] *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.

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 Card and Lee (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) $$y_i=\beta_0 + \beta_1\, 1(N_i<0) + f_1(N_i) + 1(N_i<0)\, f_2(N_i) + u_i$$

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 $f_1(.)$ and $f_2(.)$ be linear, though in the regressions we also allow for quadratic functions.

The top panel of Table 1 reports first-stage regression discontinuity (RD) estimates of equation (1) with an indicator for ever being a *PANES* beneficiary household as the dependent variable. Columns 1 to 3 present specifications with different parameterizations of the functions $f_1(.)$ and $f_2(.)$: no polynomial, a first order polynomial (as in Figure 2), and a quadratic polynomial, respectively. The first stage is strong and estimates vary minimally, between 0.98 and 0.99 across specifications, including those that also control for a variety of baseline household controls (columns 4-6).

The second panel of Table 1 reports reduced form intention-to-treat (ITT) estimates, where the dependent variable is political support for the government in the first follow-up survey in 2007. All estimates are of similar magnitude and statistically significant, suggesting an increase of 11 to 14 percentage points in support for the government among those eligible for *PANES*. We strongly reject the hypothesis that government transfer income does not affect support for the government.[20]

The third panel 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

[20] This effect is mainly driven by a shift among beneficiaries 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).

and 12 percentage points.[21] This result is displayed graphically in Figure 4. The program we study thus had persistent impacts on political support for the government, suggesting that past transfers also factor meaningfully into voters' decision-making.[22]

II.b 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. Evidence on manipulation of a program eligibility score in a recent Colombian health insurance program (Conover and Camacho, 2007) suggests that this is far from a remote possibility. Unfortunately, we lack data on baseline household political orientation, which prevents us from directly testing this alternative hypothesis; however, a variety of evidence makes it 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. An alternative possibility is that the variables recorded in the baseline survey, and that determined the predicted income score for *PANES* eligibility, were manipulated by either government officials or enumerators, or that households with closer *FA* ties somehow learned the formula and were thus able to answer the questionnaire strategically to gain eligibility. However, this is essentially impossible since the formula was not yet developed until months after the baseline surveys had been collected in 2005. The predicted income score formula was also developed by outside researchers and never publicly disclosed or shared with

[21] 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).

[22] In further analysis not presented here, we explored the possibility of heterogeneous treatment among different population subgroups. While older individuals and those living in Montevideo are marginally less responsive to the transfer in some specifications, these effects are generally not statistically significant (results not shown). In an earlier version of this paper (Manacorda et al, 2009) we also found significantly larger *PANES* impacts among households with lower baseline self-reported income, and for households predicted to be politically moderate (fitted using *Latinobarómetro* data), using the 2007 survey data. However, these results are not robust to using the second 2008 follow-up, and we do not emphasize them here.

the *MIDES* officials implementing *PANES* until after household assignment had been carried out (although the formula was of course presented to and approved by the Minister of *MIDES*).

As a first check for non-random assignment around the eligibility threshold, we estimate equation 1 for multiple pre-treatment covariates in Table 2 (and present the results graphically in supplementary appendix Figure A1). If score manipulation systematically occurred, we might find these characteristics varying discontinuously at the eligibility threshold, to the extent that they are correlated with households' political orientation. Focusing on our preferred specification with the linear fits (as in Table 1, column 2), we fail to find evidence of a discontinuity at the threshold for any household covariate including: average household members' age and education (among those over 18), income, and for the gender, age and years of education of the survey respondent, as well as in survey non-response. Consistent with this validity check, the results in Table 1 are almost unchanged when household controls are included (columns 4-6). Similarly, there is no evidence of a difference in voter turnout in the previous national election at the eligibility threshold: self-reported turnout in the previous national election was 93% for ineligible households and 94% for the eligible (and the difference is not statistically significant), in line with the consistently high turnout in Uruguay, where voting is mandatory.[23]

As an additional check for manipulation around the eligibility threshold, we present the non-parametric distribution of the standardized score. If manipulation occurred so that some ineligible households were assigned a low predicted income score, one would expect excess bunching of households 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 of households just below the eligibility threshold being overrepresented relative to those just ineligible.[24] Manipulation of the eligibility score does not appear responsible for the effects in Table 1.

Another concern is that non-response rates for the survey overall, or on the political questions, were systematically related to program eligibility. This could be a concern even

[23] Voting is mandatory in Uruguay and citizens who fail to vote (other than for justified reasons, i.e., hospitalization) 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.

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

though non-respondents were in practice replaced by households with a similar predicted income score, if unobservables differ for non-responding households. In Table 2 we report the relationship between survey non-response and *PANES* eligibility among households in the original sample as well as the probability of being a replacement household in the final analysis sample, and neither coefficient estimate is statistically significant at conventional confidence levels. We also do not find a significant relationship between survey non-response and baseline log household income, or with income interacted with *PANES* eligibility (not shown), further evidence that differential attrition as a function of program eligibility is not a leading concern.

A final concern is that *PANES* households might have expressed higher support for the government in the follow-up survey for fear of losing their benefits. This is unlikely given the degree of institutional transparency and relative lack of corruption in Uruguay, but precautions were taken to address this concern during data collection, nonetheless. 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*.[25] 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.* A final piece of evidence against strategic responses to the political questions is the fact that stated support for the government among *PANES* households remains higher in 2008 even after *PANES* had ended.

II.c. 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. The growing field evidence on the power of negative reciprocity in driving labor effort and other economically relevant behaviors (surveyed in

[25] 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).

DellaVigna, 2009) suggests this is potentially important. 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.[26] If the *Latinobarómetro* sample contains relatively few *PANES* applicants (as they were a 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 derive counterfactual support among households in our 2008 sample.[27] It is reassuring that the correlation between the predictions from this model and actual responses in our follow-up survey data is positive and large (at 0.49), and statistically significant at 99% confidence.

Figure 5 reports predicted confidence in the President for various levels of the normalized income score, as well as presenting the actual level of confidence in the President in the 2008 follow-up survey. Unsurprisingly, predicted support for the President is smooth around the *PANES* eligibility discontinuity, since we are not controlling for a discontinuous function of the eligibility score and 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

[26] 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 to vary between 0 and 1. Note that there is no question on confidence in the President in the 2008 *Latinobarómetro*, unfortunately.

[27] 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

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 PERSISTENT POLITICAL SUPPORT

III.a Impacts on income and participation in other programs

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

A leading explanation for the 2007 effects (when *PANES* was still ongoing) is that households were insufficiently informed about the program's temporary nature and thus anticipated permanent living standard gains from the program. This explanation seems unlikely since we find in 2007 survey data that only 3% of *PANES* beneficiaries believed the program was permanent, while 57% knew it was temporary (and the remaining respondents were unsure).

More importantly, the effect of *PANES* on support for the government is persistent to 2008, lasting even after the program had ended. The 2008 effect might be explained by the *PANES* program leading to persistent living standards gains among former beneficiaries, if some respondents interpreted the question about support for the government in terms of whether they were personally better off under the current government than the previous one. However, the notion that there were persistent gains in living standards is not borne out in the data. We first report impacts on log per capita household income in the two follow-up surveys (Table 3, rows 1 and 2), in our preferred specification with linear polynomial controls. Per capita income is significantly higher (by 18%) for *PANES* beneficiaries in 2007, but drops to near zero and is not statistically significant in 2008 after the program had ended. Thus lasting income gains alone cannot explain the persistent *FA* support gains in 2008. 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.[28]

[28] 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

The *PE* program introduced in early 2008 enrolled equal proportions of households on both sides of the *PANES* eligibility score threshold (Table 3 and Figure 6), at roughly 55% of households, and this seems to rule out another possible channel for persistent 2008 *FA* support gains among former *PANES* beneficiaries. While enrollment in the main *PE* program was equal in 2008, however, former *PANES* beneficiaries were more likely to receive a food card in early 2008 (point estimate 0.145, s.e. 0.032), although this constituted only a relatively small monetary amount relative to the main *PANES* and *PE* cash transfers. Interestingly, even if the *PANES* beneficiaries who continued receiving a food card in 2008 are excluded from the analysis, the effect on government support in 2008 remains statistically significant (estimate 0.079, s.e. 0.020).

The experiences of *PANES* beneficiaries and non-beneficiaries had a major impact on their perceptions of both the *PANES* and *PE* programs (Table 3). *PANES* beneficiaries have a far more positive opinion about the *PANES* program, with support increasing from 58% among non-beneficiaries up to 84% among beneficiaries, and this effect is highly significant. Similarly, while households on both sides of the eligibility threshold have serious doubts about the fairness of *PANES* targeting – with large majorities believing that some beneficiaries should not have received the transfers, that some non-beneficiaries should have, and that the program should have spread around transfers to additional households – these critical views are significantly less pronounced among *PANES* beneficiaries, clear evidence of self-serving beliefs. Former *PANES* beneficiaries also appear more knowledgeable about and supportive of the new *PE* transfers.

The main takeaway message from this subsection is that a simple model where contemporaneous transfers and income are the sole factors determining voters' political preferences cannot explain our finding that support for the *FA* remains higher among former *PANES* beneficiaries into the post-program period. Similarly, continued income gains (from government transfers or other sources) for former *PANES* beneficiaries are unlikely to explain

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 response is consistent with results from Mexico's *Progresa* program (Parker and Skoufias, 2000). We also find only 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. However, there is no evidence of impacts on durables ownership, home characteristics or self-reported health (see Amarante et al., 2008). 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%).

their persistent increase in *FA* support in 2008, since these are minimal or nonexistent, and thus we need to explore other channels to understand the evolution of political preferences.

III.b Voter learning about politician preferences: a model and calibration

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.[29]

The assumption that voters also 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 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

[29] 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.

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:

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

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:

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

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:

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

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 (Table 4)[30], 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 former *PANES* beneficiaries also continue to receive a food card valued at US$19.50 per month.

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_{i0} to 50% of the actual *PANES* transfer, or US$44.75, although results

[30] 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.

are not sensitive to 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.

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-beneficiates of 0.639 (Table 4), 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 Table 4). 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 Table 4, 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.

This analysis implies that, under plausible assumptions, a model of rational but poorly informed voters learning about politician redistributive preferences can rationalize the broad patterns in the Uruguay data, and in particular the persistence of *FA* support gains into the post-*PANES* period. However, these findings do not necessarily imply that models of reciprocity from psychology and economics are not also at work in this or other related contexts. In particular, the learning model we present cannot be conclusively distinguished from a gift-exchange model between voters and politicians, with decaying effects as time elapses since the transfer (as Gneezy and List, 2006 find empirically in a labor market setting). Disentangling these two models remains an important objective for future research.

III.c Impacts on political attitudes and social perceptions

The results presented in section II provide clear evidence of voters' responsiveness to the program, with targeted transfers securing additional supporters for the government both during the life of the program (2007) and shortly after its end (2008). The previous subsection assesses whether a model of voter learning about politician preferences can rationalize the persistence of *FA* support gains. In this sub-section, we present evidence on an array of additional political and social attitudes, to gauge whether they are consistent with the implications of the model and also to describe broader *PANES* impacts on respondents.

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 an assistance program, some beneficiary households might have also experienced an improvement in their self-esteem and psychic well-being. To investigate these issues further, we consider an alternative, subjective measure of household satisfaction, using the following question from the follow-up survey: "*on a scale 1 to 5, where 1 is very bad and 5 very good, how would you qualify the current situation of your household?*" (We re-scale this and all questions that follow from 0 to 1, as described in Supplementary Appendix Table A2.) Related questions ask about the country's current situation and expectations one year into the future.

A first key difference between former *PANES* beneficiaries and non-beneficiaries in 2008 is the increased optimism about their household's own current and future situation (point estimates 0.059, s.e. 0.019 and 0.045, s.e. 0.016, respectively in Table 5). This increased optimism about the future is consistent with the previous sub-section's model, to the extent that these individuals may be expecting greater government transfers in the future. Beneficiaries' report of greater subjective satisfaction with their own household situation is also broadly consistent with self-interested "pocketbook voting". However, the conclusion that self-interest alone is driving increased *FA* support is tempered by the observation that, relative to non-beneficiaries, former *PANES* beneficiaries also have a more positive perception of the country's situation, both currently and for the future (gains of 0.059 and 0.055, respectively). They are also significantly less likely to have perceived increased social differences in the past year (-0.109, s.e. 0.041). This suggests that voters overweigh their own personal experience in making sense of economic and social conditions, a possibility that has recently found widespread support in experimental economics (see Simonsohn et al., 2008 for an example). It is plausible that voters' support for a particular party is driven both by their own individual situation and economic conditions in society at large; misperceptions about the latter (for instance, the sanguine views about social inequalities among former beneficiaries) might thus also help partially explain the persistent differences in expressed *FA* support. This leaves the basic intuition of the model developed in the previous subsection intact, but suggests an even richer set of explanations for the persistent *FA* support gains among former *PANES* beneficiaries in 2008.

Changes in attitudes are manifested in higher support not only for the incumbent government (Table 1), the President, and *PANES* itself (in Table 3), but also confidence in the Ministry of Social Development (0.192, s.e. 0.038, Table 5), which designed, and administered *PANES*. However, we find no significant increases in support for other institutions not directly related to *PANES* (for instance, Parliament, local councils) or even the social security administration that disbursed the transfers, indicating that former *PANES* beneficiaries are not simply casting a more optimistic eye on political institutions and organizations across the board, although point estimates tend to be positive, suggesting small positive spillovers.

There are a number of other provocative impacts of *PANES* receipt, including a statistically significant increase in national pride at being Uruguayan (coefficient estimate 0.046, s.e. 0.024, from an already high level of patriotic feeling, Table 5), and significantly more self-expressed interest in politics in general (0.067, s.e. 0.031). However, there is 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" (0.019, s.e. 0.026). Similar questions in other social surveys are generally thought to capture more conservative views.[31] This suggests that a change in left-right ideology is not the key driver behind the increase political support for the *FA*.

IV. SUMMARY AND DISCUSSION

We find that beneficiaries of a large government anti-poverty program in Uruguay are significantly more likely to support the current government than non-beneficiaries. We use individual level data on political support and a credible regression discontinuity research design to estimate these effects, constituting a novel approach in this branch of the empirical political economy literature. We find large and robust effects on the order of 11 to 14 percentage points, and these effects last into the post-program period. These results indicate that government economic policies can have large and potentially persistent impacts on beneficiaries' political and social attitudes. DiTella et al. (2007) reach a similar conclusion in their study of the long-run impacts of a land reform program in Argentina.

[31] 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).

It is difficult to extrapolate these results to the case where a right-wing party would have implemented a similar transfer policy, or if the policy had been implemented in a period of economic contraction, rather than the largely favorable macroeconomic environment that Uruguay experienced from 2005 to 2008, in which even non-beneficiaries might have given the government some credit for their income gains. Another important validity issue is how likely these results are to generalize to other countries. While Uruguay is a middle income country, it has well-developed democratic institutions and a long tradition of strong political parties, suggesting that the findings of this paper are relevant not only for Latin America but also possibly for wealthier countries with similarly capable political institutions.

Finally, we estimate a local treatment effect in this paper at the program eligibility threshold (among poor households), and thus extrapolating treatment effects to other populations, such as middle or wealthy class voters, requires stronger assumptions. We cannot rule out the possibility that the *Frente Amplio* government in Uruguay even lost some votes among better-off voters who had to pay for the policy though higher taxes, offsetting the political support gains we estimate among the poor; our dataset and research design do not allow us to measure any such effects. Thus while it would be interesting to compute a cost per extra vote due to *PANES*, we are reluctant to do so due these potentially lost votes among richer households, as well as the imperfect correspondence between stated support in a survey and actual votes.

A further caveat relates to the possibility that the *PANES* program caused an erosion in support among those households who just barely failed to qualify. While the presence of "sore losers" would not change our basic punch line – that differences in transfers across groups translate into different degrees of political support – and we do present suggestive evidence that leans against the "bitterness" interpretation, a government seeking re-election is obviously not indifferent about whether transfers alienate more voters than they gain.

We present a formal model of rational but poorly informed voters who use anti-poverty transfers as signals of government redistributive preferences towards them, and find that it can rationalize the broad patterns in our data, in particular the persistent post-program political support gains. In a similar framework, Drazen and Eslava (2006) show that heterogeneous politician preferences across different socio-demographic groups can generate a political economy equilibrium where parties favor certain groups and these groups in turn reward the incumbent with their votes, even if voters are fully aware of politicians' incentives to target transfers strategically to maximize their vote share.

Although our results are consistent with this model of voter learning about politician preferences, we cannot conclusively rule out the possibility that theories of reciprocity rooted in psychology and economics are also playing a role in driving the persistent political support gains for the party that implemented this large and unprecedented transfer program in Uruguay. In particular, the persistent but slightly diminished effects we estimate in the year after the *PANES* program had ended are also consistent with a model of voter "gratitude" that decays gradually over time. Pinpointing the role, if any, that gratitude and reciprocity play in generating persistent political support gains among transfer recipients is an important area for future research.

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Figure 1: *PANES* program and data collection timeline

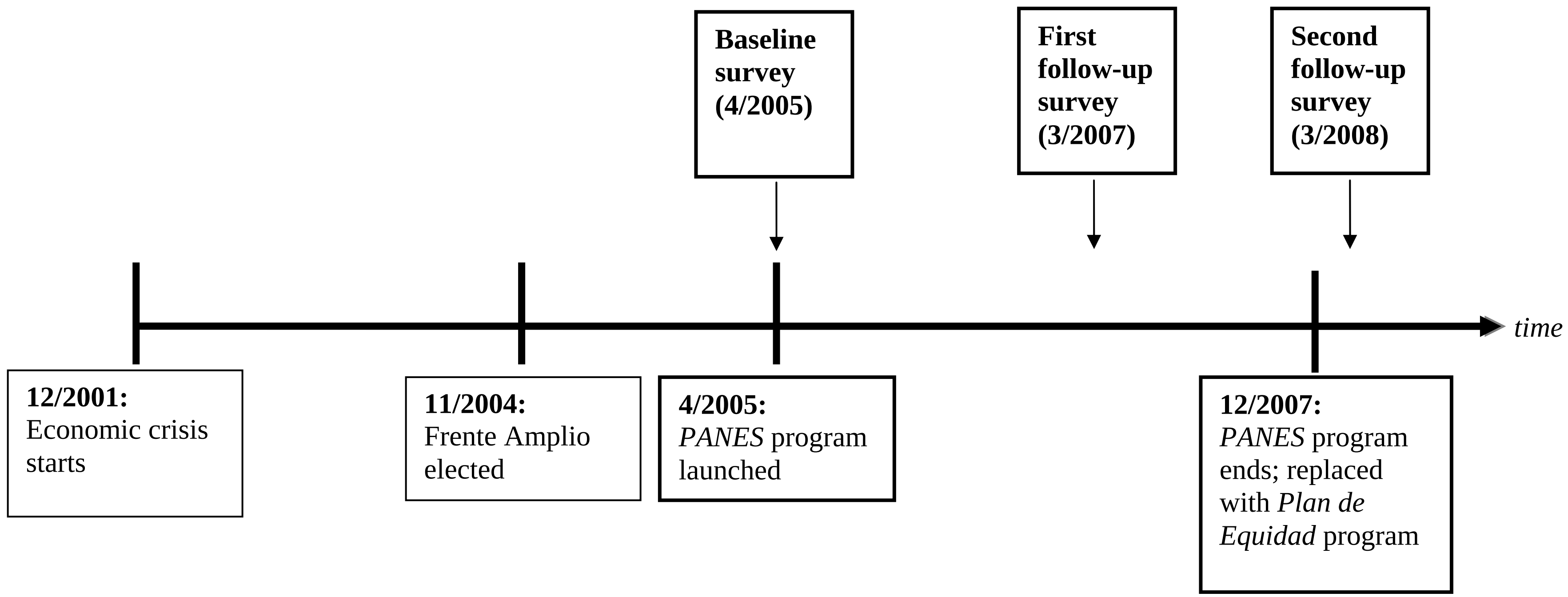
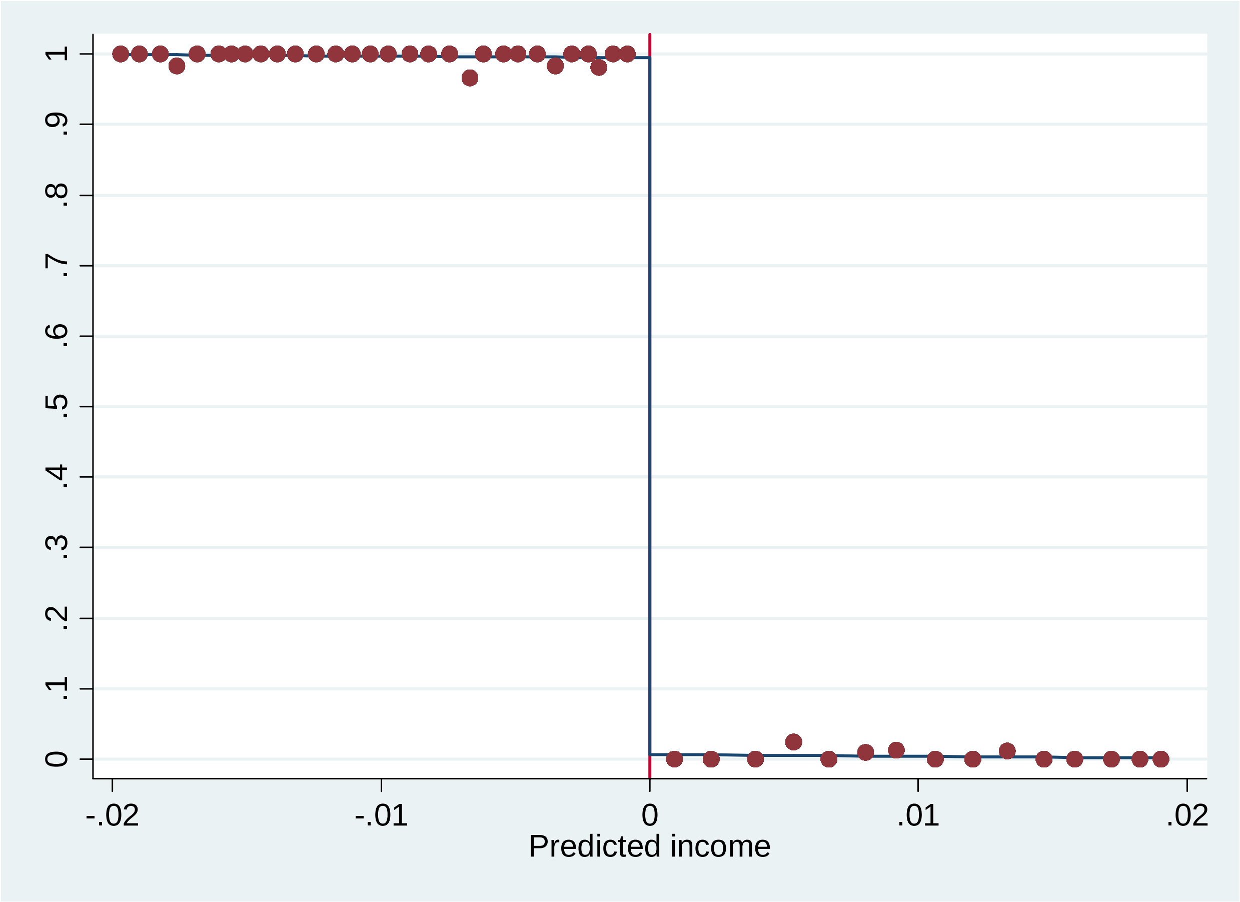
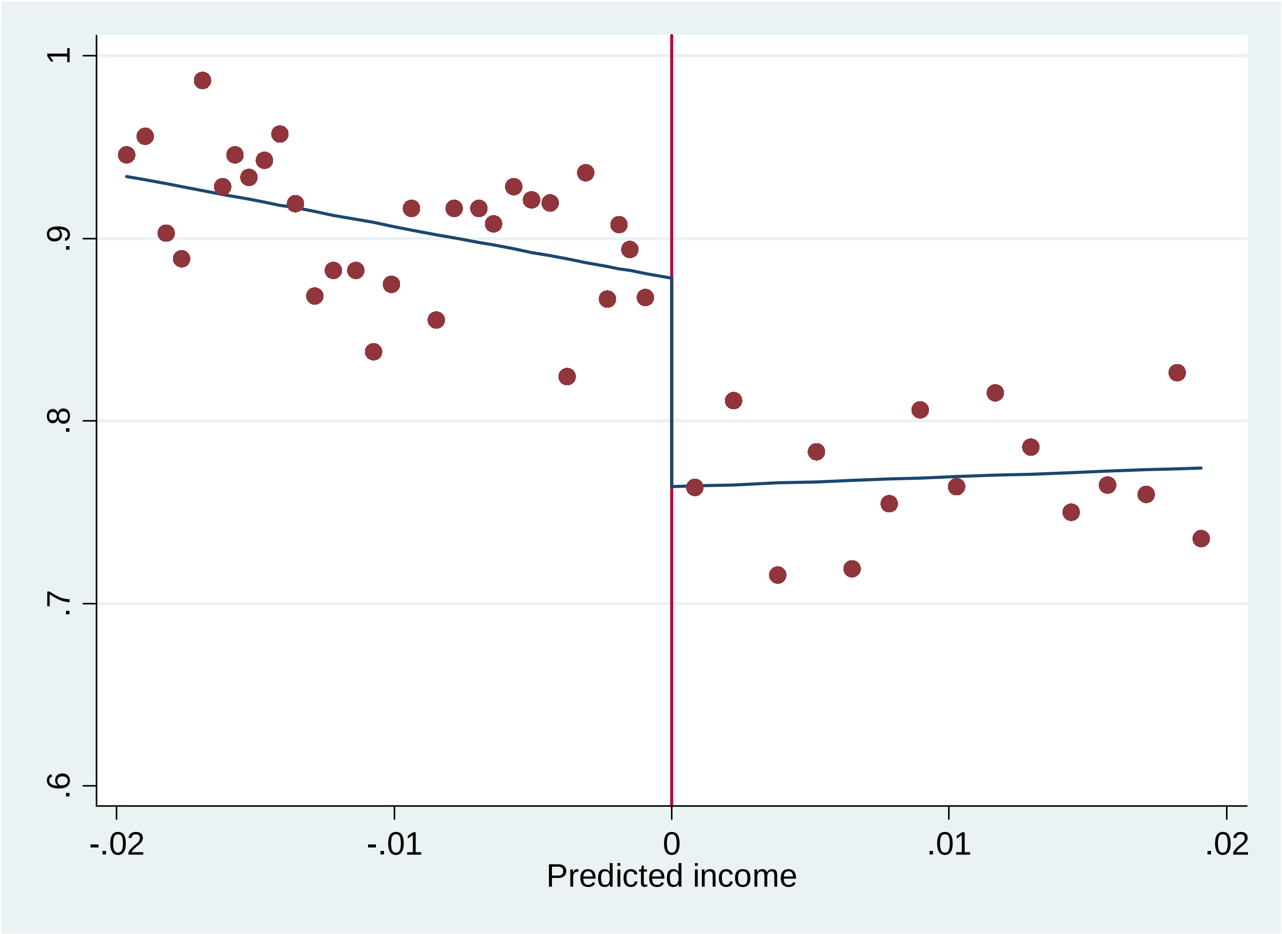


Figure 2: *PANES* program eligibility and participation



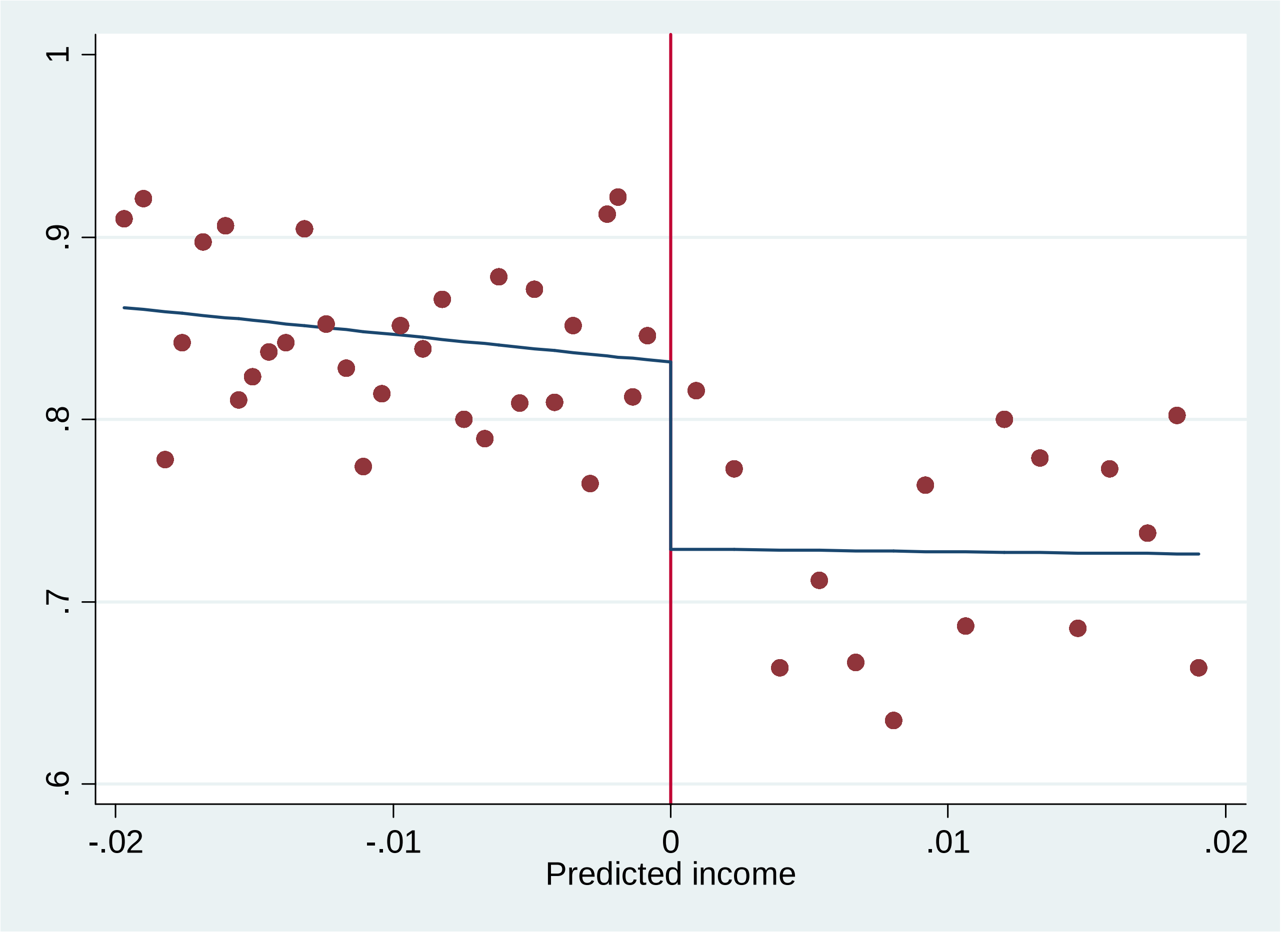
Notes. The picture reports the proportion of households ever enrolled in *PANES* as a function of the standardized score (based on administrative data). The fitted plots are linear best fits on each side of the eligibility threshold.

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



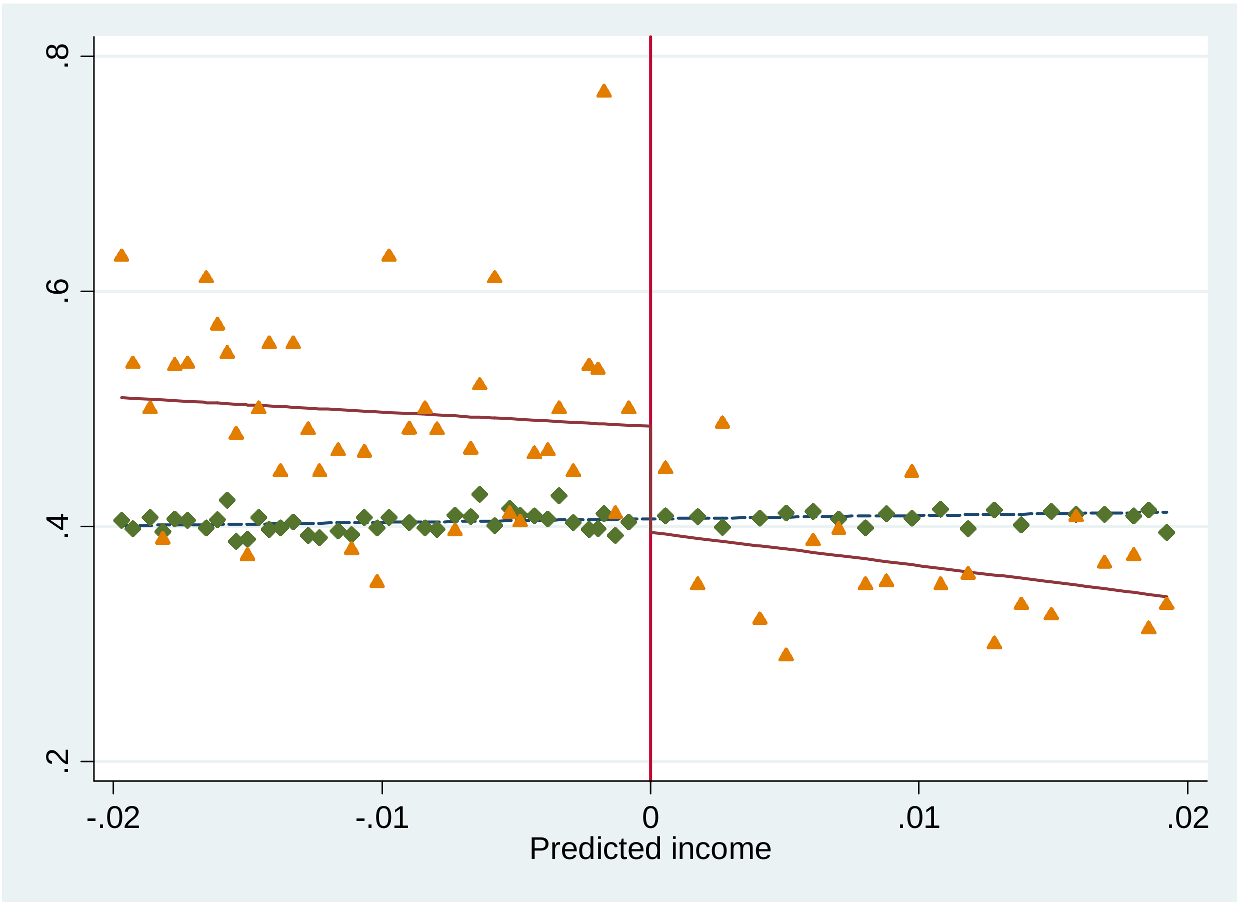
Notes. The figure reports the average support for the current government (compared to the previous government) as a function of the standardized score. Source: first *PANES* follow-up survey (2007). The fitted plots are linear best fits on each side of the eligibility threshold.

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



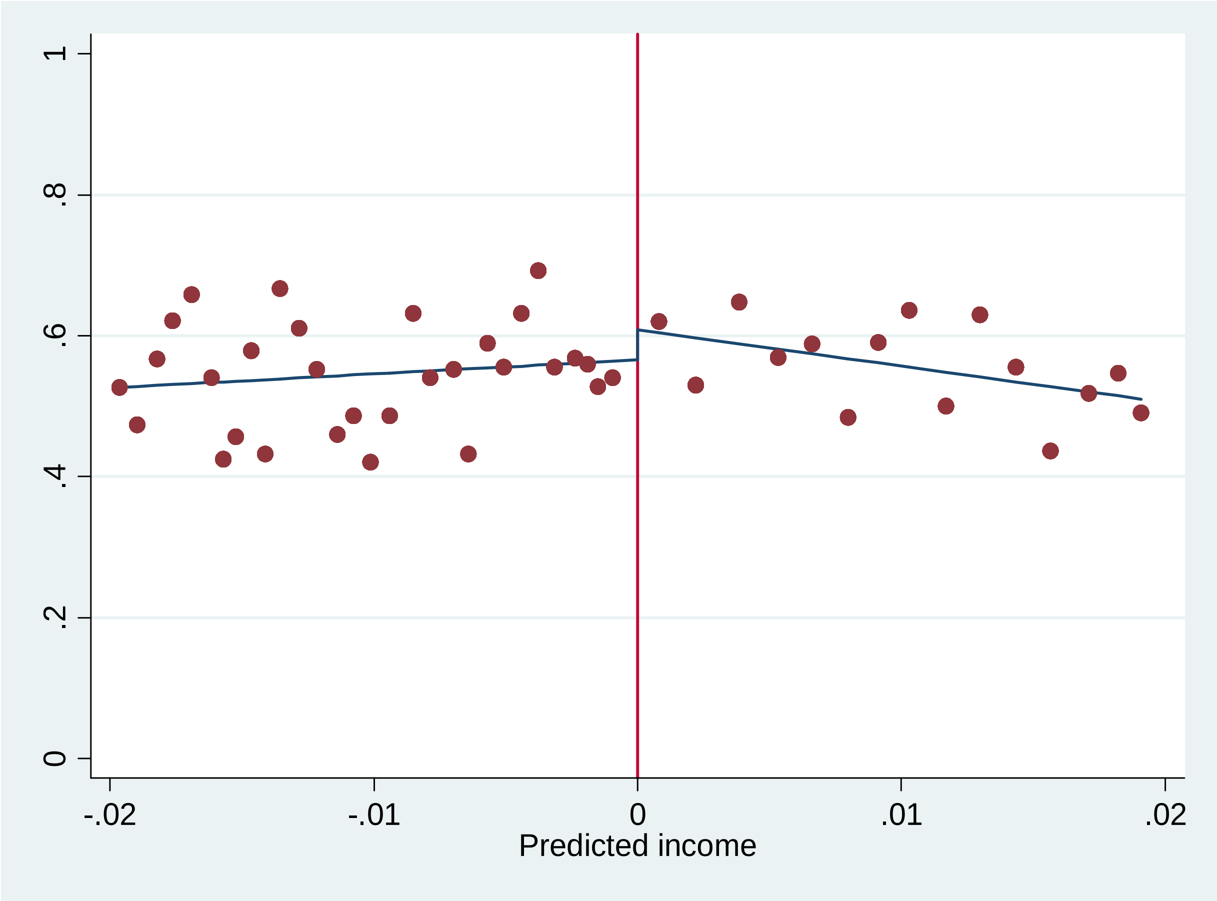
Notes. The figure reports the average support for the current government (compared to the previous government) as a function of the standardized score. Source: the second *PANES* follow-up survey (2008). The fitted plots are linear best fits on each side of the eligibility threshold.

Figure 5: Confidence in President:
Actual (triangles / solid line) and predicted based on *Latinobarómetro* (diamonds / dashed line)



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

Figure 6: *PANES* program eligibility and participation in the later *Plan de Equidad* Program



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

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

		(1)	(2)	(3)	(4)	(5)	(6)
Panel A:	Mean non-eligibles	Dep. var: Ever received *PANES*, 2005-2007 (administrative data)					
Program eligibility	0.002	0.997***	0.993***	0.996***	0.997***	0.993***	0.997***
		(0.002)	(0.004)	(0.005)	(0.002)	(0.004)	(0.005)
Panel B:		Dep. var: Government support , 2007					
Program eligibility	0.770	0.137***	0.118***	0.138***	0.135***	0.112***	0.136***
		(0.014)	(0.028)	(0.043)	(0.015)	(0.029)	(0.045)
Panel C:		Dep. var: Government support, 2008 (post-program)					
Program eligibility	0.729	0.116***	0.095***	0.092**	0.117***	0.091***	0.081*
		(0.015)	(0.030)	(0.043)	(0.016)	(0.032)	(0.045)
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 (Panel A) and political support in 2007 and 2008 (Panels B and C, 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 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. Number of observations in Panes A and C: 1,938; in panel A: 1,826. Standard errors clustered by score 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, pre-treatment characteristics and response rates

Dependent variable:	Mean non-eligibles	Coefficient (s.e.)	Observations
Log per-capita income (2005)	6.33	-0.061 (0.063)	1,876
Household average years of education 16+ (2005)	4.04	0.091 (0.200)	1,887
Household size (2005)	3.05	-0.323 (0.237)	1,938
Household average age (2005)	31.69	-1.599 (2.126)	1,938
Respondent is female (2005)	0.699	-0.016 (0.056)	1,937
Respondent years of education (2005)	6.45	0.294 (0.321)	1,916
Respondent age (2005)	44.10	-1.811 (1.583)	1,938
Survey non-response rate (2008)	0.384	0.071 (0.045)	2,367
Replacement household (2008)	0.349	-0.069 (0.045)	1,938
Voted in 2004 elections	0.924	0.014 (0.024)	1,911
Linear score controls		Yes	

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

Table 3: Program eligibility, income and participation in other programs

Dependent variable:	Mean non-eligibles	Coefficient (s.e.)	Observations
Log household per-capita income (2007)	6.869	0.176** (0.072)	1,889
Log household per-capita income (2008)	7.125	-0.071 (0.070)	1,893
Positive opinion about *PANES* (2008)	0.583	0.256*** (0.026)	1,906
PANES targeting and design (2007)			
There are people who received *PANES* who should not have	0.917	-0.072** (0.031)	1,843
There are people who did not receive *PANES* who should have	0.981	-0.055*** (0.016)	1,878
Beneficiaries should have received less so that more people could benefit	0.876	-0.105*** (0.033)	1,878
Received *Plan de Equidad* (2008), administrative data	0.555	-0.022 (0.048)	1,938
Received food card (2008)	0.039	0.145*** (0.032)	1,935
Heard about *Plan de Equidad* (2008)	0.626	0.154*** (0.042)	1,916
Positive opinion about *Plan de Equidad* (2008)	0.704	0.056** (0.024)	1,251
Relative to *PANES*, *Plan de Equidad* has improved the situation of:			
The respondent	0.653	-0.046 (0.045)	1,179
The country	0.636	0.097** (0.045)	1,186
Linear score controls		Yes	

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

Table 4: Voter Learning Model Calibration Results

Panel A:	FA support (actual)	Expected transfer (US$) $1/\sigma_0^2 = 0.5$	$1/\sigma_0^2 = 0.1$	$1/\sigma_0^2 = 0.9$
PANES beneficiaries, in 2007 (t=1)	0.843	61.6	67.7	58.0
PANES non-beneficiaries, in 2007 (t=1)	0.639	14.9	4.1	21.2
Former PANES beneficiaries, in 2008 (t=2)	0.746	56.0	58.1	54.4
Former PANES non-beneficiaries, in 2008 (t=2)	0.574	23.7	19.7	26.6

Panel B:	Parameter estimates $1/\sigma_0^2 = 0.5$	$1/\sigma_0^2 = 0.1$	$1/\sigma_0^2 = 0.9$
Parameter estimates for:			
a_1	0.216	0.500	-0.067
a_2	-0.265	-0.045	-0.503
b	0.024	0.017	0.030

Panel C:	**FA support (actual)**	Predicted FA support (model) $1/\sigma_0^2 = 0.5$	$1/\sigma_0^2 = 0.1$	$1/\sigma_0^2 = 0.9$
FA support in 2008 (t=2), former PANES beneficiaries:	**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 section III.b. 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 $1/\sigma_0^2$. See text for further details.

Table 5: Program eligibility and political and social attitudes (2008)

Dependent variable:	Mean non-eligibles	Coefficient (s.e.)	Observations
Assessment of the respondent's current household's situation (2008)	0.448	0.059*** (0.019)	1,933
Expectation of the respondent's household's situation next year (2008)	0.646	0.045*** (0.016)	1,813
Assessment of the country's current situation (2008)	0.496	0.059*** (0.022)	1,912
Expectation of the country's situation next year (2008)	0.597	0.055*** (0.018)	1,788
Relative to last year, are social differences greater? (2008)	0.515	-0.109*** (0.041)	1,721
Respondent confidence (2008) in the:			
Minister of Social Development	0.391	0.192*** (0.038)	1,722
President	0.371	0.089** (0.040)	1,844
Political parties	0.123	0.033 (0.028)	1,794
Social Security administration	0.472	0.025 (0.036)	1,803
Local councils	0.303	0.027 (0.036)	1,763
Parliament	0.21	0.016 (0.038)	1,363
Interest in politics (2008)	0.201	0.067** (0.031)	1,929
National pride (2008)	0.788	0.046* (0.024)	1,890
Hard work pays off in life (2008)	0.349	0.019 (0.026)	1,900
Linear score controls		Yes	

Notes. The table reports results from regressions of various outcomes and survey responses on the program eligibility indicator. The specification is equivalent to 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

	UNDP *Human Development Report 2007*				*The Economist* Intelligence Unit democracy index				
	Human development index	GDP per capita (PPP)	Life expectancy	Gross school enrolment rate	Democracy	Rank	Electoral process	Functioning of govt.	Political culture
Uruguay	**0.852**	**9,962**	**75.9**	**88.9**	**Full**	**27**	**10.00**	**8.21**	**6.88**
USA	0.951	41,890	77.9	93.3	Full	17	8.75	7.86	8.75
Argentina	0.869	14,280	74.8	89.7	Flawed	54	8.75	5.00	5.63
Brazil	0.800	8,402	71.7	87.5	Flawed	42	9.58	7.86	5.63
Chile	0.867	12,027	78.3	82.9	Flawed	30	9.58	8.93	6.25
Colombia	0.791	7,304	72.3	75.1	Flawed	67	9.17	4.36	4.38
Mexico	0.829	10,751	75.6	75.6	Flawed	53	8.75	6.07	5.00
Venezuela	0.792	6,632	73.2	75.5	Hybrid	93	7.00	3.64	5.00

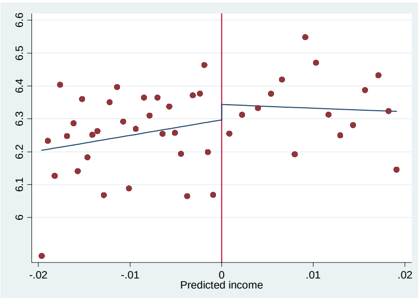
Source: UNDP (2007) and The Economist Intelligence Unit (2007).

Appendix Table A2: Description of categorical attitude variables

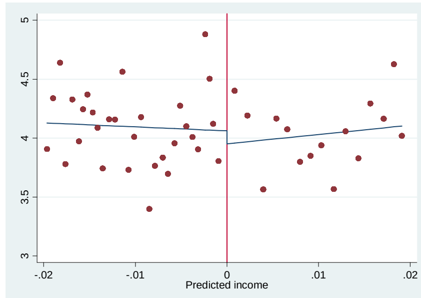
Variable	Range of values	Question wording (translated from Spanish by the authors)
Supports current government	1 to 3	Compared to previous government is the current government: 1: worse, 2: same, 3: better?
Positive opinion about *PANES*	1 to 5	At a general level how do you feel with respect to *PANES*: 1: very bad, 2: bad, 3: neither good nor bad, 4: good, 5: very good?
Positive opinion about *PE*	1 to 6	At a general level what did you think of the *PANES*/*PE*: 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?
Interest in politics	1 to 4	How interested are you in politics: 1: not at all, 2: not very, 3: somewhat, 4: very?
National pride	1 to 4	How proud are you of being Uruguayan: 1: not at all, 2: little, 3: somewhat, 4: very?
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?
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?
Relative to *PANES*, situation of person/household/country with *PE*	1 to 3	Relative to *PANES*, do you believe that with the *PE* the situation (of person/household/country) is: 1: worse, 2: the same, 3: better?
Hard work pays off in life		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 characteristics

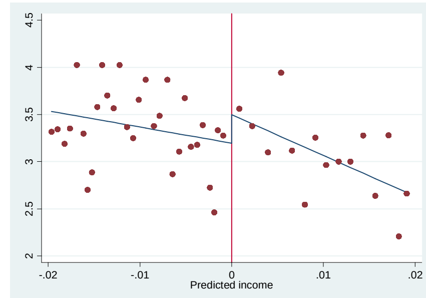
Panel A: Log per capita income



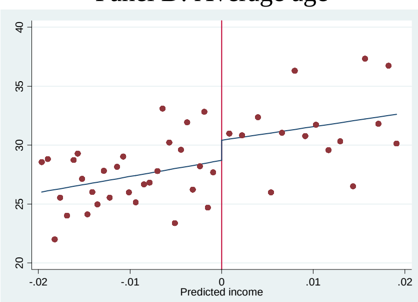
Panel B: Average years of education



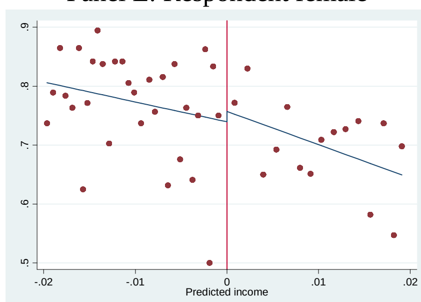
Panel C: Household size



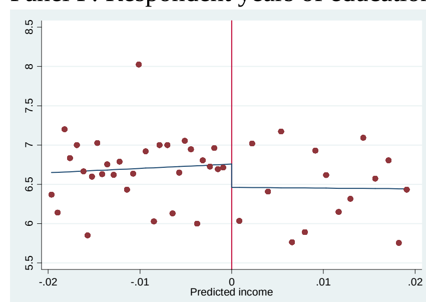
Panel D: Average age



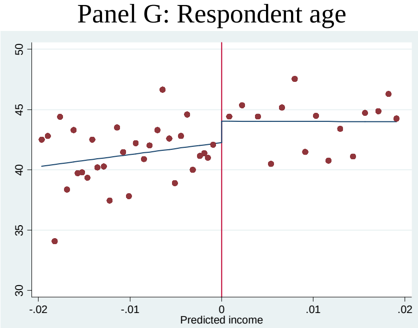
Panel E: Respondent female



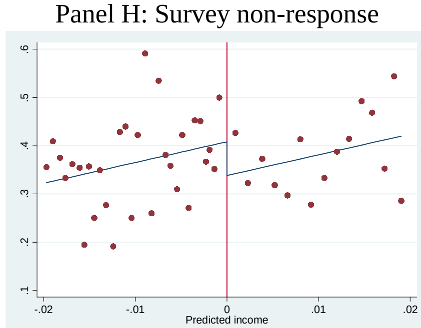
Panel F: Respondent years of education



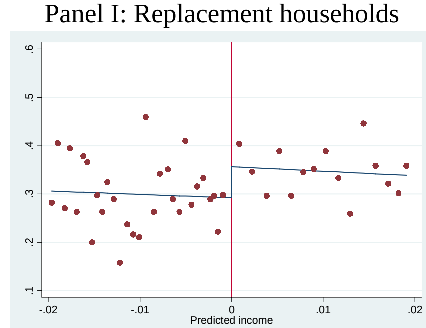
Panel G: Respondent age



Panel H: Survey non-response

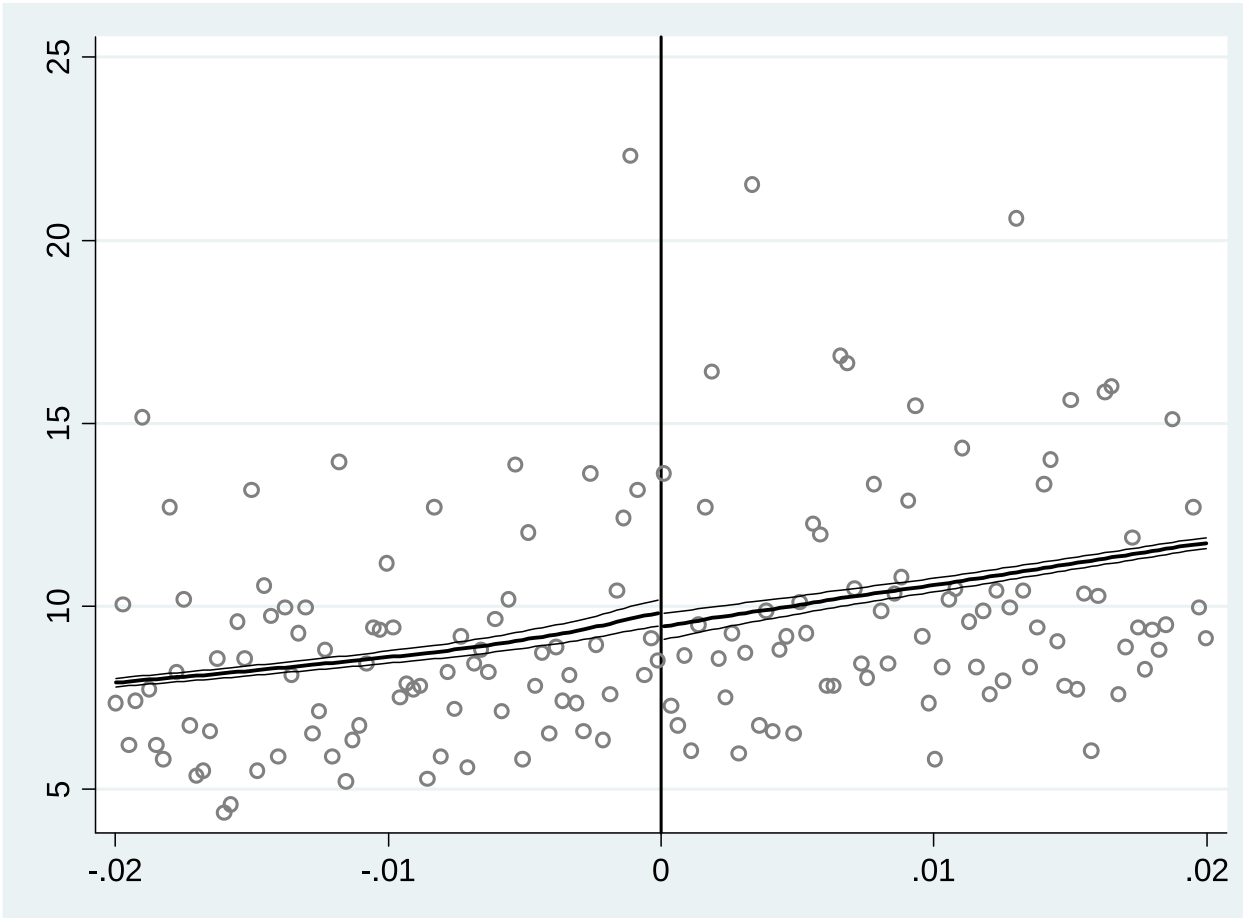


Panel I: Replacement households



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

.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