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Drought response in an election year: Evidence from Brazil

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Keywords: Water conservation; policy instrument design; political budget cycles; drought.

JEL Codes: H12; Q25; Q28.

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Abstract

Research on the impact of water conservation instruments rarely considers the role of electoral politics. This paper evaluates the response of a major state-owned water utility to the drought that occurred in the city of São Paulo, Brazil during 2014. The response coincided with an election for state governor. A difference-in-difference research design produces no evidence that a reward-based instrument implemented before the election reduced household consumption. Evidence is found that a penalty-based instrument implemented after the election reduced consumption by 4 to 8%. The implications of insulating water utilities' drought response from the political-electoral cycle are discussed.

I. Introduction

In 2014 and 2015, a major drought in southern Brazil almost collapsed the main water system which feeds the largest city in South America, São Paulo. Among measures such as awareness campaigns and reducing pressure in the water system, the authorities put two instruments in place to incentivize water savings: a financial reward and a financial penalty.

In this paper, we use microdata from a panel of households to evaluate the causal effects of these policies on water consumption using a difference-in-difference (DD) approach. The effectiveness of both instruments is evaluated against an election period which took most of 2014. We will document that the financial penalty was much more effective than the financial reward and that the financial penalty was put in place right after a general election held in October 2014. To the best of our knowledge, this coincidence of hydrological, policy and political-electoral events is a rare natural experiment which sheds light on the intersection of two different strands of literature.

On one hand, there is a large literature investigating the effectiveness of different policy instruments in managing demand in a context of water scarcity. Usually economists recommend price-based instruments to reflect scarcity and to compensate for the higher cost of supplying water (Olmstead and Stavins 2009; Olmstead 2010). Pricing policies follow extensive empirical work showing that water consumption is negatively inelastic to price (Espey et al 1997; Dalhuisen et al 2003; Worthington and Hoffman 2008) and that price-based instruments are effective at reducing demand (Pint 1999; Kenney et al 2008). On the other hand, decision-makers may also employ financial rewards to induce conservation behavior. Empirical studies of the effectiveness of conservation rewards have found that they are not as effective at producing water savings as pricing (Kenny et al 2008; Renwick and Green 2000; Maggioni 2015) and that they involve efficiency risks, for example that users invoke the reward who would have undertaken the target behavior anyway (Joskow and Marron 1992).[1]

Another large strand of literature deals with the effects of the electoral cycle on elected officials' policy-making behavior. The relevant framework in this case is of political budget cycles, which was initiated by Nordhaus (1975) and given a modern formal structure by Rogoff (1990). One

[1]Recent experience in Australia, Canada and the United States shows that quite aside from incentive-based instruments, the most common policy response to drought is use restrictions (Dixon et al 1996; Grafton and Ward 2008; Mansur and Olmstead 2012). Use restrictions are generally effective at reducing demand (Maggioni 2015; Renwick and Green 2000) but cause unnecessary welfare loss compared to pricing, because their rigidity precludes heterogeneous responses within and across users (Brennan et al 2007; Olmstead and Stavins 2009).

of the most relevant contributions in this context is Klien (2014a), who documents how water tariff increases are much less pronounced in election years, due to the salience of water tariffs for the re-election prospects of incumbent politicians. For Latin America, Felgendreher and Lehmann (2016) also document political interference in water policies in Peru.

We believe our analysis contributes to recognizing the role that the electoral cycle can play in water management in general and more specifically, in the management of water crises in political jurisdictions where there is significant scope for elected officials to influence water management decisions. As mentioned above, there is a large literature prescribing what would be the optimal response in times of water crisis, and there is a large literature on how electoral cycles affect economic policy, but the present paper sheds light on how these events could bias decision-making towards policies that are technically sub-optimal from a public welfare perspective.

The next section uses political budget cycle theory to interpret the instrument effectiveness findings in light of the political-electoral context. This interpretation addresses an identified gap in water economics research around how political-electoral factors influence water conservation instrument design and in turn, effectiveness (Olmstead and Stavins 2009[2]; Oats and Portney 2003; Dinar 2000). Section III describes the rare co-occurrence of hydrological and political-electoral events that defined the São Paulo drought. Section IV describes water billing data for a representative sample of households of the municipality of São Paulo during the drought. Section V presents the tests of the effectiveness of the two instruments at managing household water demand. Section VI presents the results and section VII discusses the limitations and policy implications of the research.

II. Conservation instrument choice and election cycles

We believe that any evaluation of the policy response that emerged to the São Paulo water crisis based only on efficiency arguments would be incomplete. The electoral cycle effects on policymaking must be taken into account.

Political budget cycle theory holds that in the run up to an election, incumbent politicians and the individuals in the bureaucracy who they appoint, take policy decisions that are designed to win favor in the short term with voters (Nordhaus 1975; Rogoff 1990). Incumbent politicians do this because voters evaluate the incumbent's performance at least partly through the nature of the

[2] "In comparing price and nonprice approaches to urban water conservation, we have highlighted some important areas for future research …. These include …. theoretical and empirical investigations of political and legal constraints on pricing for the relative efficiency of market based water conservation approaches . . . and modeling the political economy of water conservation policy instrument choice" (Olmstead and Stavins 2009: 8).

bureaucracy's output during the incumbent's tenure. In the older framework of political budget cycles of Nordhaus (1975), the economy finds itself in a worse equilibrium than what would be socially optimal because consumers are backward looking. In the more recent version of this literature (Rogoff 1990), politicians behave immediately before an election to influence how voters form their expectations about the politicians' unobserved "ability". That means politicians distort policies in ways that increase their probability of winning.

As evidence of the existence of political budget cycles, proponents point to incumbents implementing policies immediately prior to elections that reduce taxes and increase deficits (Alesina 1989; Binzer and Durlouf 1989), raise government spending (Aidt et al 2011), and shift government spending towards more visible and "voter friendly" spending (Drazen and Eslava 2010; Tanzi and Davoodi 1997).

Relevant to the country context of our empirical analysis in the next sections, Brazil, is the idea that political budget cycles are stronger in jurisdictions where political and bureaucratic powers are weakly separated (Ennser-Jedenastik 2016). Cycles may be stronger in countries with weak or young democracies where voters are less inclined or able to dismiss pre-election fiscal giveaways as either ”budget antics" (Rogoff 1990) or bad policymaking (Ehrhart 2013). For example, Labonne (2016) finds that the employment rate across Philippines municipalities increased before elections, while Vadlamannati (2015) finds that the agency in India responsible for policing corruption increased the number of corruption cases it investigated before scheduled elections, but not before unscheduled elections. Political budget cycles have also been detected in the Brazilian context. Pailler (2018) found that deforestation rates in the Amazon increased by 8 to 10% in municipalities where an incumbent was running for re-election. More specifically with respect to water policies, Klien's (2014a) work indicates that changes in water prices in election years are smaller, due to the salience of water prices for voter choices. In related work, Klien (2014b) also finds that the effects of political interference are somewhat reduced when water supply services are carried out by an incorporated company that is separate from the state.

In non-election years, elected officials may be inclined to defer to the preferences of the bureaucracy, large water users, and non-governmental actors involved in the water management process, as long as these preferences are broadly in line with their own. Elected officials might defer to these actors as a strategy for maximizing the welfare of water user-voters over the long time horizon that exists before the next election. Participative, decentralized decision-making is slow but effective at maximizing welfare, so allowing these processes to proceed with minimal intervention makes sense for elected officials in non-election years.

In election years on the other hand, elected officials might be more inclined to exercise the power they hold in the process. They may have stronger incentives to influence bureaucratic output in order to signal their ability to manage their way out of a crisis with the least amount of welfare loss. This may cause the water management process, including instrument choice, design, and implementation timing, to produce policy outcomes that are not the ones that minimize welfare loss or the likelihood of deeper water shortage.

In terms of the instruments that specialists actually use to manage scarce water resources, some may be more likely to emerge in election years and in non-election years.

This can be illustrated by a hypothetical scarcity event. In non-election years, bureaucrats, water users and civil society elements are the dominant actors in the decision-making process. These actors might produce a policy response that many economic and natural scientists would judge to be "rational", such as a recommendation to increase the price of water. Economic research holds that water demand is negatively elastic to price (Ruijs et al 2008; Worthington and Hoffman 2008), and that prices must reflect water scarcity (Klaiber et al 2014). Another aspect defining this "rational" policy would be it being mandatory, as in Halich and Stephenson (2009), instead of relying on voluntary action.

There is also considerable evidence from the global policy experience of responding to drought that pricing can be an effective and efficient way of reducing demand (Barrett 2004; Grafton and Ward 2008; Kenney et al 2008; Mansur and Olmstead 2007). For example, Pint (2009) indicates that increases in block prices contributed to a 16% reduction in water usage in California. Thus, pricing might be the first best approach in this context from the economist's perspective.[3]

In election years, elected officials have a stronger motivation to participate in the governance process because they want to steer decisions in a direction that improves their chance of re-election. With strong government influence, the water governance process might be more inclined towards a response that signal their ability to deal with any water shortage in an effective way (that is, with the least cost to water consumers). These might include procuring new water supplies via the construction of new (visible) infrastructure, precisely because the (relatively high) costs of these options are born over very long time periods and the immediate impact on the water user-voter's welfare position is limited. As we argue below, another possibility would be to provide financial incentives for voluntary decreases in water consumption.

Pricing measures could be a less preferred alternative in an election year, since they would directly affect consumers' income, even though pricing would otherwise be the standard

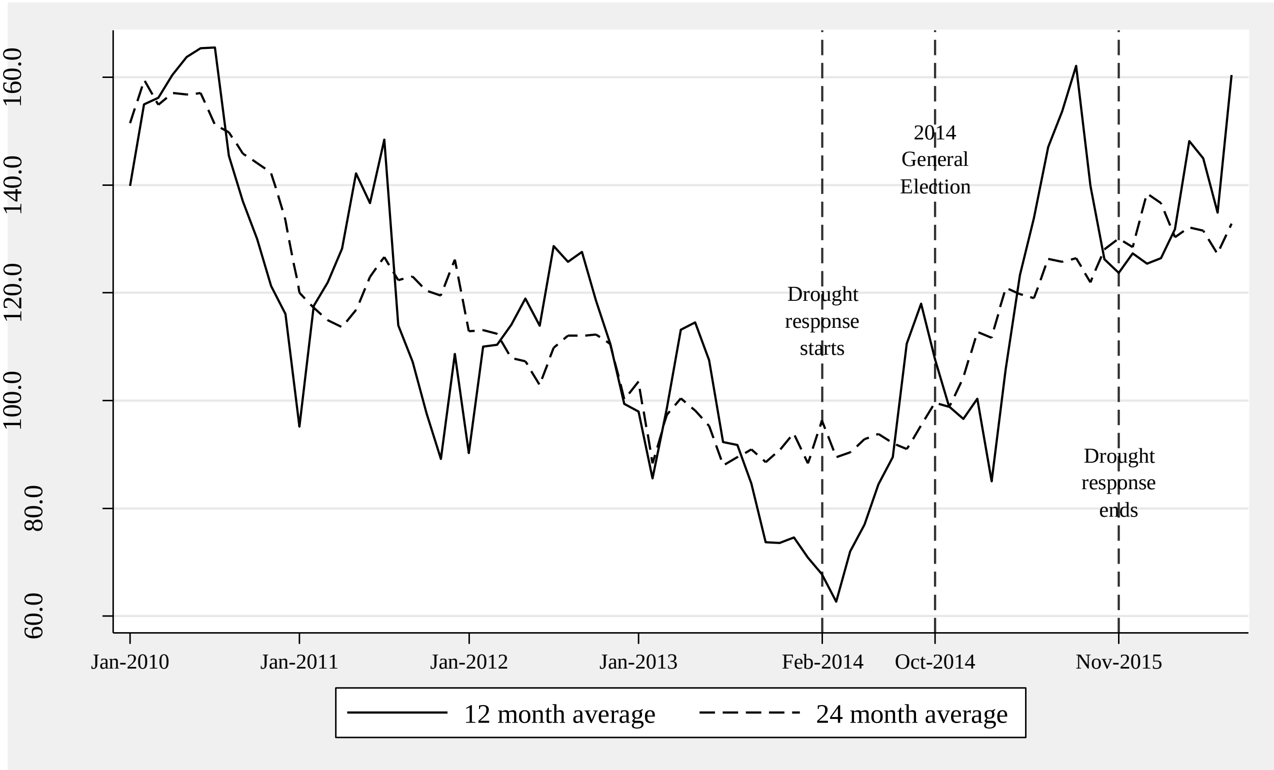
[3] This conclusion does not imply that the only available instrument for dealing with water scarcity would be price increases. Water consumption restrictions are a common and effective way of reducing demand during drought (Dixon et al 1998; Maggioni 2015), although they are probably less efficient than pricing (Olmstead and Stavins 2009). Procuring additional water resources through bulk water purchases or building additional water production or transportation infrastructure are other options.

microeconomic prescription to goods being scarce. This issue could be even more pressing considering the salience of water prices. As mentioned above, Klien (2014a) indicates that increases in water prices are smaller in election years and that water companies that are more insulated from political pressures (by being corporatized and so more distant from the state) could better withstand these pressures (Klien 2014b). Water rationing measures might also rank lower in the decision maker's choice set in election years.

III. Empirical context

The state of São Paulo, Brazil started experiencing historically low and irregular rainfall several years before 2014, and in the beginning of 2014 entered into the most severe drought that had occurred there in 80 years. Water production volumes for urban use were affected, including for the Metropolitan Region of São Paulo (MRSP). The figure below shows the rolling average rainfall level for the region to the north of the MRSP, where most of its raw water is sourced.

Figure 1: Rainfall level in the Cantareira system region



Beyond its hydrological characteristics, the drought was unusual because it occurred during an election year in Brazil. A general election was held in October 2014 for senators and representatives to all 26 Brazilian state congresses, for all state governors, for various representatives and senators to the national congress, and for president. General elections are scheduled in Brazil and occur every four years. In the state of São Paulo, in the governor's race there, the state government's response to the drought became an issue in the political debate.[4]

The main supplier of water and wastewater services in the state of São Paulo is the Basic Sanitation Company of the State of São Paulo (Companhia de Saneamento Básico do Estado de São Paulo [SABEP]). SABESP is one of the largest water utilities in the world by market capitalization. Its shares are traded on the NYSE. The state of São Paulo owns the majority of shares.

SABESP is operated as an integral part of the state of São Paulo's governmental structure. SABESP formulates policy decisions in conjunction with major state government bodies. SABESP's Form 20-F filing with the US Securities and Exchange Commission (2015) states the following risks to investors relating to the control of the company by the state of São Paulo:

> "As it owns the majority of our common shares, the State is able to determine our operating policies and strategy, control the election of a majority of the members of our board of directors and appoint our senior management . . . . Both through its control of our board of directors as well as by enacting State decrees, the State has in the past directed our company to engage in business activities and make expenditures that promoted political, economic or social goals, but that did not necessarily enhance our business and results of operations. The State may direct our company to act in this manner again in the future" (SABESP 2015: 12).

Economic regulation of SABESP is carried out by the São Paulo State Sanitation and Energy Regulatory Agency (Agência Reguladora de Água e Saneamento do Estado de São Paulo [ARSESP]). ARSESP oversees the supply of water and sanitation services to end-users in the state, either residential, commercial and industrial. ARSESP regulates SABESP on a cost-of-service basis with some aspects of incentive-based regulation. Every tariff cycle (usually five years), ARSESP computes operating costs, investments in network and reservoir expansion, and improvements, and an allowance for return on capital, and sets an initial tariff structure for the cycle. Prices are then adjusted during the cycle according to an inflation index, less a fixed amount, to account for productivity increases.

During the drought, SABESP created two economic instruments to induce household water conservation behavior in the MRSP. It launched the first in February 2014, eight months before the election. This was a financial incentive to reward household conservation effort, which came to be

[4] In the final minutes of the last televised candidate debate before election day, the incumbent governor stated that the drought was the most severe in the history of the state, that billions of Reals had been invested in the state's water and sanitation sector during his governorship, and that the "bonus" program had been implemented to promote water conservation "on the account of SABESP" (RBA 2014). The challenger candidates argued that the governor's administration had been negligent in planning and maintaining the state's water supply infrastructure, which caused the crisis, and that the governor's response to the crisis had been late and ineffective.

known as the "bonus".[5] The rules stated that a household would receive a discount of 30% on its monthly water bill in each month that it achieved a 20% reduction in water consumption relative the household's own average consumption in the 12 months preceding the drought (ARSESP 2014a).[6]

SABESP increased the spatial coverage of the bonus, in April 2014. The MRSP is fed by seven different water supply systems and the drought affected water production in all of them, but the worst affected was the Cantareira system. The Cantareira system is a massive network of reservoirs, tunnels, canals, and pumping stations that abstracts water from rivers in the interior of São Paulo state and transports it 80 km to the MRSP. About half of the households in the MRSP are served by the Cantareira system. In the original February 2014 version of the bonus, only households served by the Cantareira system were eligible. Under the revised version, all households served by all systems were eligible.

SABESP extended the bonus yet again in October 2014. On October 5, the incumbent governor of the state of São Paulo won his race, but in the national race for president, no single candidate received the 50% majority required for a win under Brazilian electoral law. This meant that the presidential race went to a run-off vote, which was scheduled for October 26. On October 22, SABESP extended the bonus to reward a broader range of conservation effort. Previously, a household that reduced its consumption by 20% or more in a month had its bill reduced by 30%. In the revision, a household that reduced its consumption by between 15 and 20% has its bill reduced by 20%, and a household that reduced its consumption by between 10 and 15% had its bill reduced by 15%. This increased the number of households that were eligible for the bonus. The incumbent president won the run-off vote on October 26, 2014.

Approximately two months after the election, in January 2015, SABESP implemented a second economic instrument. This was a financial penalty, which came to be known as the "contingency tariff". The rules stated that any household that increased its water consumption by between 1 and 20% in a month, relative to its own mean consumption during the 12 months preceding the drought, would have a 40% surcharge applied to its bill for that month. Similarly, any household increasing its consumption by more than 20% would face a 100% surcharge. In contrast to the bonus,

[5] Incentive Program for the Reduction of the Consumption of Water (Programa de Incentivo à Redução do Consumo de Água).

[6] This was the second time a "bonus" system was put in place. In March 2004 – another electoral year, but for mayoral elections – the bonus system was put in place. However, in that year the drought was less severe, and with the resumption of rainfall, water levels rose to a point that the bonus system was called off without any need for other measures.

which households could participate in voluntarily, the contingency tariff applied to all households mandatorily.[7]

By November 2015, rainfall levels in São Paulo state had returned to normal, the Cantareira system reservoirs were near full again, and the drought was deemed over. SABESP terminated both the bonus and the contingency tariff programs in November 2015.

IV. Data and descriptive analysis

This section evaluates whether the two programs were effective at inducing household water conservation behaviour, and if so to what extent.

Monthly water bill data were obtained for a sample of households located in São Paulo municipality, the most populous municipality in the MRSP. The data were obtained from the Foundation Institute for Economic Research (Fundação Instituto de Pesquisas Econômicas [FIPE]), located at the University of São Paulo. FIPE collected the data as part of its periodic Family Budgets Survey (Pesquisa de Orçamento Familiar [PoF]), which it uses to calculate an index of consumer prices. FIPE ordinarily uses the water bill data collected through the survey to rebalance the weights in the basket of goods in the index.

FIPE designed the sample to be representative of 95% of the earnings distribution of the households in São Paulo municipality. The sampling frame was the complete customer registry of AES Electropaulo, an electric power company with approximately 6 million customers in the MRSP. FIPE randomly selected households from the frame using household electricity consumption as a proxy for household income. FIPE then conducted a single interview with each sampled household. During the interview FIPE collected information about the social and demographic characteristics of the dwellers and the structural and material characteristics of the dwelling. The household's SABESP account information was also recorded and used to download the household's monthly water bill data from SABESP's online billing portal.

Other information gathered by FIPE included the household's street address; the water system serving the household; household income; whether the household owns a washing machine,

[7] The regulatory ruling that implemented the contingency tariff stated that the bonus had not been as effective as hoped at reigning in household water consumption. It stated that while the bonus had been paid to many households, "24 percent of users [had] increased their consumption and exceeded the pre-drought mean consumption level previously defined under the [bonus] Program, despite a public campaign calling for the rational use of water and the widespread water shortage" (ARSESP 2015a).

dishwasher, shower, and/or other appliances; whether the household owns or rents the dwelling it occupies; the number of rooms in the dwelling; and the number of occupants. In the data, these household characteristics are observed only in the cross section. They are relevant to predicting household water consumption, but most are made redundant in the estimation strategy by household fixed effects.

There were 352 households in the raw data obtained from FIPE, 12 of which we discarded for having incomplete or unusable water consumption data. This left a balanced panel of 340 households observed continuously over 40 months. The first month is February 2013, which is 12 months before SABESP launched the reward-based bonus program. The last month is May 2016, which is five months after the bonus and contingency tariff were terminated.

Information about the start and end months of the bonus and contingency tariff were taken from the regulatory rulings of ARSESP, which approved the programs (ARSESP 2014a; ARSESP 2015a).

A final variable captures whether the household is located on a part of SABESP's water distribution network that is fitted with pressure reduction valves (PRVs). During the drought, SABESP intensified a practice it had used since the 1990s of reducing water pressure in its MRSP distribution network at night to limit water loss from network leaks. Around the time of the drought, between 25 and 40% of the water SABESP treated for the MRSP was being lost to network leaks (Instituto Trata 2010). During the drought, SABESP extended the hours and degree of pressure reduction as a water conservation measure. However, it could only do this on the approximately 55% of the network that was fitted with PRVs (SABESP 2016). Information identifying these areas were obtained from a response by SABESP to a court ruling that obliged it to disclose which customers were being affected by pressure reductions (ARSESP 2015b). The variable captures whether the household is located an area that is *not* fitted with a PRV.

Table 1: Variables and descriptive statistics

Label	Units	Source	N	Mean	Min	Max	SD
$WATCONS_{it}$	water consumption, m^3	FIPE	13,600	14.01	0.00	126.00	9.41
$CANTSYS_i$	households served by Cantareira system	FIPE	13,600	0.46	0.00	1.00	0.50
$BONUSPER_t$	months bonus active, feb14 - mar14	ARSESP	13,600	0.05	0.00	1.00	0.22
$HICONS_{it}$	household water consumption > 10 m^3	ARSESP	13,600	0.59	0.00	1.00	0.49
$TARIFPER_t$	months contingency tariff active, jan15 - nov15	FIPE	13,600	0.28	0.00	1.00	0.45
$HHINCOME_i$	household income, R$	FIPE	13,600	4,519.51	510.00	37,014.00	3,904.78
$NOPRV_i$	household in area with no PRV	ARSESP	13,600	0.31	0.00	1.00	0.46

Note: Subscript *i* denotes household (*i* = 1 ... 340), *t* denotes month (*t* = 1 … 40). 1 Brazilian Real (R$) = US$ 0.27 at the time of writing (January 2019).

Figure 2: The municipalities of the MRSP and São Paulo municipality

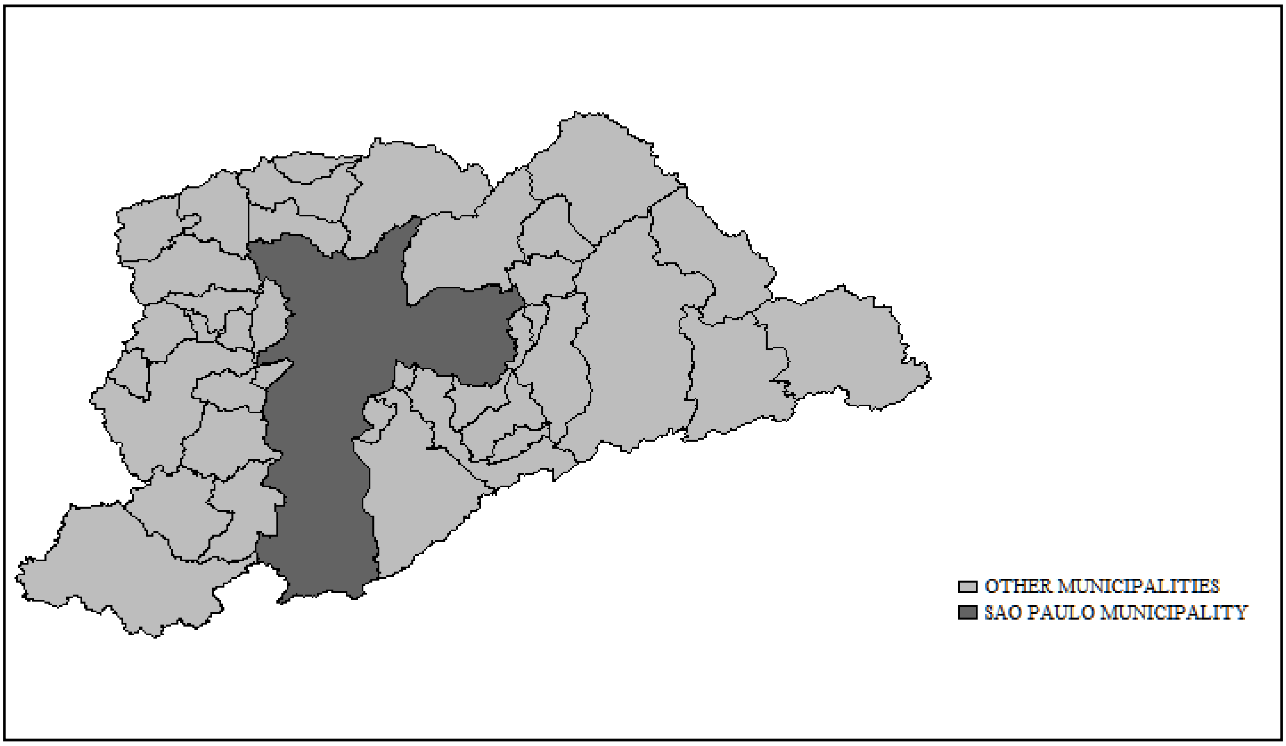


Figure 3: Sample households by water supply system and population density

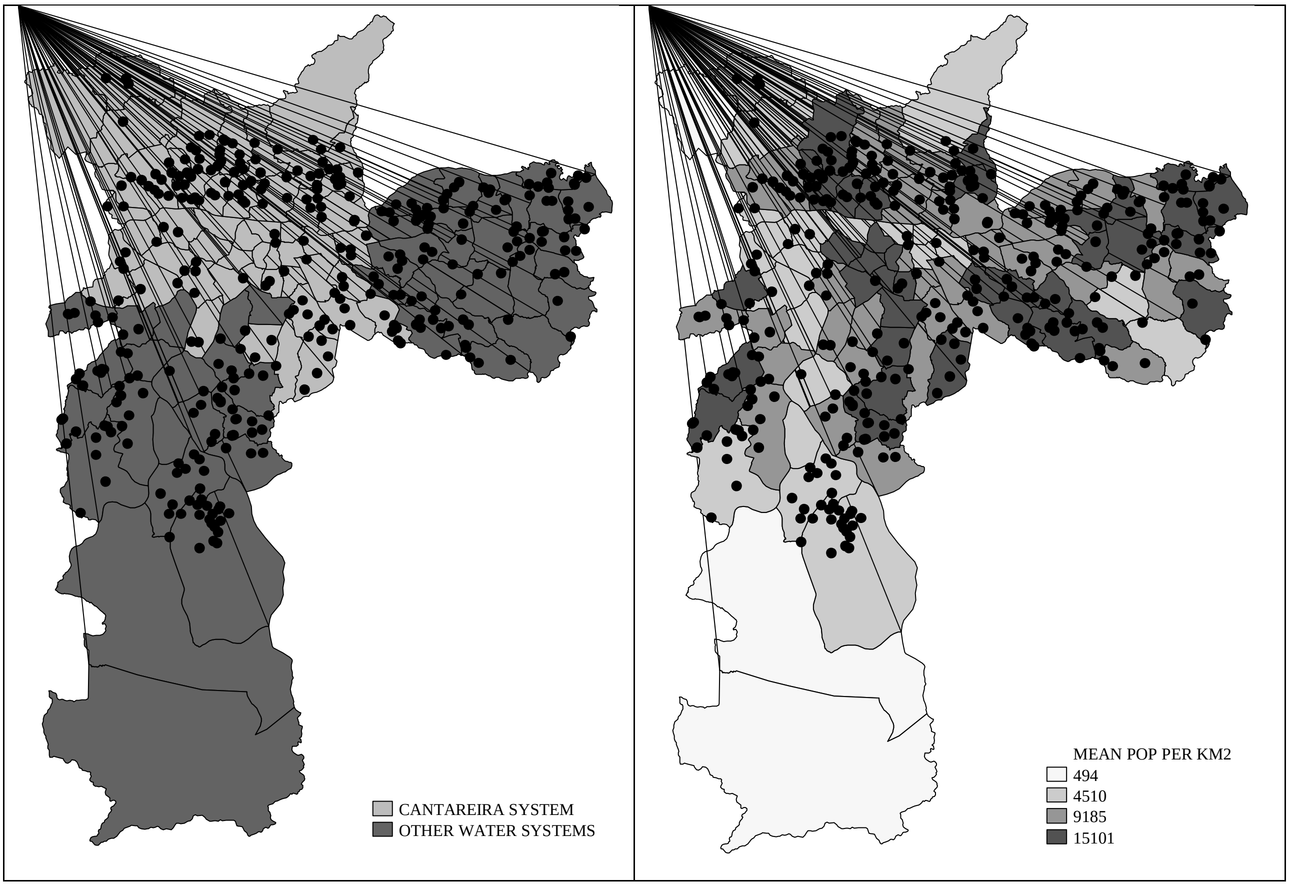


Figure 4: Household consumption by treatment and control groups

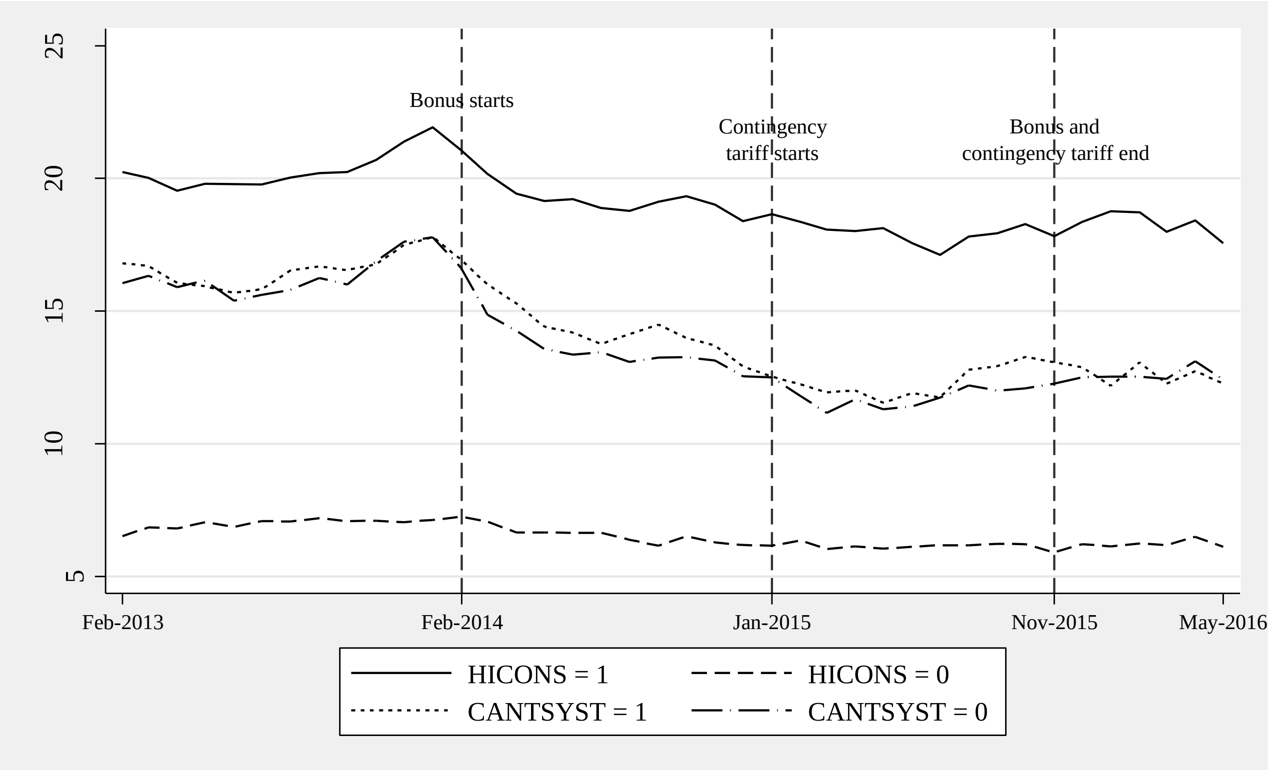


Table 2: Water bill as a proportion of household income, by income quintile

Income quintile	Monthly income, R$	Monthly income, US$	Mean monthly water bill, R$	Bill as % of monthly income
1	1,216	678	57	5.3
2	2,233	1,240	69	3.2
3	3,459	1,953	72	2.1
4	5,392	3,028	86	1.6
5	10,628	5,891	98	1.0

Note: calculation based on household income reported during 2011-2012 and mean exchange rate for 2012.

V. Model and estimation

In an ideal world the causal effect of the programs would be estimated by comparing the level of consumption by households exposed to a program, to the level of consumption by the exact same households in a situation where they were not exposed to the program. Since this is impossible to observe, it was necessary to estimate a counterfactual situation where comparable households were not exposed.

The single empirical model below sets up a difference-in-difference (DD) test for each program. In each test, a control group proxies for what consumption by the exposed households would have been during the treatment period. The response variable is $WATCONS_{it}$, which is household i's water consumption level measured in m3 in month t. The aim is to estimate the coefficients β_1 and β_4, the DD estimate for each program.

Equation 1: Basic specification

$$WATCONS_{it} = \alpha_i + \beta_1(CANTSYS_i \times BONUSPER_t) + \beta_2(CANTSYS_i) + \beta_3(BONUSPER_t) + \beta_4(HICONS_{it} \times TARIFPER_t) + \beta_5(HICONS_{it}) + \beta_6(TARIFPER_t) + \boldsymbol{\theta}_i + \boldsymbol{\lambda}_t + \varepsilon_{it}$$

The design of the bonus test exploits an irregularity in the program roll-out across space. As discussed previously, when SABESP initially implemented the bonus in February 2014, it limited eligibility to households served by the Cantareira system. Two months later in April 2014, SABESP extended the bonus to all households served by all systems.

This means that for the months of February and March 2014, the households that were served by the Cantareira system were exposed to the bonus, but the households that were served by the other systems were not exposed. Thus, the treated group is the households served by the Cantareira system ($CANTSYS_i$), the control group is the households served by all other system, and the treatment period is February and March 2014 ($BONUSPER_i$). Coefficient β_1 is interpreted as the average effect of the bonus program on exposed households during the bonus period.

The contingency tariff test exploits a different irregularity, namely an exemption of some households from the tariff under the program design. The regulatory ruling that established the contingency tariff stated that the tariff applied to all SABESP water users except " . . . a) those with water consumption in a month less than or equal to 10 m3 . . . ." (ARSESP 2015a).[8] This means that households that consumed up to 10 m3 of water in a month were exempt from the contingency tariff.

In the DD test, households with consumption in a month that exceeded 10 m3 are the treated group ($HICONS_{it}$) and households that consumed up to 10 m3 are the control group. The treatment period begins in January 2015 when the contingency tariff came into effect ($TARIFPER_t$) and ends 11 months later in November 2015 when the program was terminated. Coefficient β_4 is the average effect of the contingency tariff on household water consumption for exposed households.

[8] Hospitals, first aid stations, health clinics, police stations, prisons, and detention centers were also exempt.

The model is estimated by OLS with, variously, household and month fixed effects. Household fixed effects purge the data of all time-invariant, between-household variability that might affect the level of water consumption. This includes variability arising from whether or not the household sits in a PRV-fitted area of the city. In the estimations, household fixed effects are implemented by the demeaning method. They are denoted $\boldsymbol{\theta}$ above.

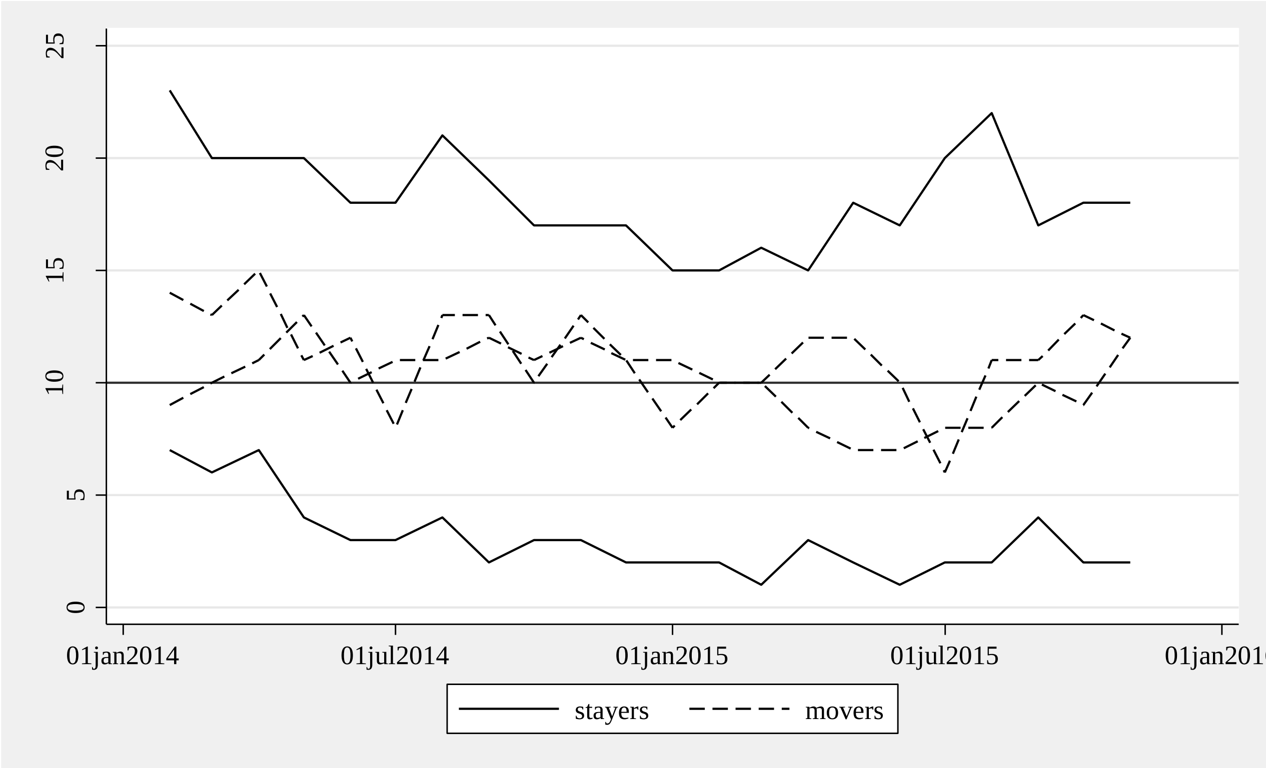
Month-specific fixed effects are denoted $\boldsymbol{\lambda}$. Each takes the same value for all households in month t. They account for influences on water consumption affecting all households in the month. An example of such an influence would be SABESP varying the intensity its nighttime pressure reduction measure over time.

The error term ε is assumed to capture residual stochastic variation that is uncorrelated with the independent variables. Following best practice for statistical inference in DD designs (Wing et al 2018), the errors are clustered on households so that they are robust to correlation within households across months.

An unusual issue with the contingency tariff test specifically is that household membership in the control and treatment groups is not completely stable over time. Recall that the treatment group was defined as households "whose monthly consumption" was greater than 10 m^3, and that the control group was defined as households whose consumption was less than or equal to 10 m^3 "in a month". The issue is that, because household consumption varies by month, and because there are multiple months in the pre-treatment and treatment periods, some households move in and out of the treatment group.

Lee and Kim (2014) discuss this issue as the "endogenous movers" problem. It occurs in a DD set up with more than two periods, and time-varying exposure by individuals to a treatment. Individuals that move between treatment and control groups ("movers") may do so because of the treatment itself, or for reasons that have nothing to do with the treatment. Our main concern is that the presence of mover households in the contingency tariff test could produce a DD test result that indicates that the contingency tariff caused households to reduce water consumption when in fact it did not, a false positive.

Figure 5: Mover and stayer households



Of the 340 households in the data, 201 are "movers" (either high-low, low-high, or both), 86 are "high stayers" and 53 are "low stayers". Lee and Kim (2014) recommend dealing with the problems presented by movers by estimating the model for stayers only. This ensures that the composition of the treatment and control groups is stable over time and that only those individuals that consistently qualify for the treatment, experience the treatment.

The second econometric issue the possibility of reverse causality. The contingency tariff is expected to reduce household water consumption because it penalized households for consuming beyond a household-specific threshold. However, SABESP customers also face an increasing block tariff (IBT) price schedule for the water they consume.[9] They faced the IBT before the drought and during the drought; the IBT did not change. The concern is that, if the unit price of water rises with consumption, then the level of consumption could possibly drive the unit price, as consumption moves upward through the blocks. This issue is well known in water economics studies estimating water demand price elasticities (Olmstead et al 2007; Worthington and Hoffman 2008).

[9] A household on SABESP's standard residential tariff paid a fixed monthly fee of approximately R$ 16.00 for any amount of water consumed up to 10 m3, then about R$ 2.50 for each additional m3 up to 20, R$ 6.50 for each additional m3 up to 50, and R$ 7.00 for each additional m3 over 50 (SABESP 2016).

We believe that reverse causality is of minimal concern here because no water price variable is used anywhere in the empirical set up. The tests test whether *exposure* by households to the programs affected household consumption. The effect of the *level* of exposure is not tested, nor is the effect of different water prices on consumption, nor is the effect of prices that are caused by exposure. Once a household is in the treatment group, greater consumption should not increase the household's likelihood of exposure to the program. Otherwise stated, the design of the treatment is exogenous to the actions of a single household (Besley and Case 2000). Under these conditions, OLS should be consistent.

VI. Results

No significant evidence was found that the bonus program caused households to reduce water consumption. Specification I in the table below estimates the model by OLS and includes month fixed effects. Specification II estimates the model by OLS and interacts each month fixed effect with the *NOPRV* variable, testing whether the pressure reduction measure masked the bonus effect. Specification III estimates the model with household fixed effects, which are implemented by the demeaning method. Household fixed effects account for all cross sectional variability in household water consumption including household location, dwelling size, household income, and outdoor water consumption (to the extent that these did not vary over time). Specification IV estimates the model with household fixed effects and adds the month fixed effects. The coefficients for all variables in the model are reported in the appendix (Table 6).

Table 3: Main results

	I	II	III	IV
$CANTSYS_i$ x $BONUSPER_t$	-0.09	-0.10	0.09	0.11
	[-0.19]	[-0.23]	[0.21]	[0.26]
$HICONS_i$ x $TARIFPER_t$	-0.93**	-0.95***	-1.82***	-1.46***
	[-3.31]	[-3.37]	[-6.92]	[-5.82]
Month FE	Yes	Yes	No	Yes
Month x pressure reduction FE	No	Yes	No	No
Obs.	13600	13600	13600	13600
Unique HHs	340	340	340	340
R2 overall, adjusted	0.45	0.46	0.27	0.31

Note: Dependent variable is monthly household water consumption in m3.
Errors are robust to correlation within households over time. T-scores in brackets.
Constant and coefficients on constituent variables omitted for presentation (full output in appendix).
(I) OLS estimation with month fixed effects.
(II) OLS with month fixed effects interacted with household exposure to pressure reduction measure.
(III) Household fixed effects estimation implemented by the demeaning method.
(IV) Household fixed effects estimation implemented by the demeaning method, and month fixed effects.

Various additional tests produced no evidence of a bonus effect (appendix, Table 7). The treatment period was lagged on the possibility that household participation was delayed because information about the bonus took time to arrive and/or because households took time to adjust their behavior (specifications I – III). The sample was restricted to households in the lowest income quintiles on the possibility that the incentive to participate was stronger in relative terms for these households (specifications IV and V). The sample was restricted to the two months before and after the introduction of the bonus on the possibility that asymmetrical window lengths were biasing the estimates (specification VI).

Consistent evidence was found by contrast for a contingency tariff effect. Specifications I and II above (Table 3) imply that households exposed to the contingency tariff reduced their consumption by a little less than 1.0 m3 on average during the treatment period, relative to unexposed households. Specifications III, which accounts for all time-invariant, between-household influences on household water consumption, estimates the effect at -1.8 m3. Specification IV, which further accounts for influences that might affect all households equally in each month, puts the effect at -1.5 m3.

Various robustness tests were run on the contingency tariff result. The table below estimates the same four specifications as above but with the mover households removed from the sample. Dropping the mover households produces considerably larger treatment effect estimates for the contingency tariff. These range from -1.8 m3 in specification III to -2.45 m3 in specification II. The appendix gives the full model output (Table 8).

Table 4: Sample restricted to "stayer" households

	I	II	III	IV
$CANTSYS_i$ x $BONUSPER_t$	0.74	0.85	0.57	0.60
	[1.04]	[1.23]	[0.88]	[0.93]
$HICONS_i$ x $TARIFPER_t$	-2.37***	-2.45***	-1.84***	-2.27***
	[-5.58]	[-5.25]	[-4.30]	[-5.36]
Month FE	Yes	Yes	No	Yes
Month x pressure reduction FE	No	Yes	No	No
Obs.	3526	3526	3526	3526
Unique HHs	139	139	139	139
R2 overall, adjusted	0.57	0.57	0.13	0.16

Note: Dependent variable is monthly household water consumption in m3.
Sample restricted to households with stable membership in contingency tariff treatment or control group ('stayers').
Errors are robust to correlation within households over time. T-scores in brackets.
Constant and coefficients on constituent variables omitted for presentation (full output in appendix).
(I) OLS estimation with month fixed effects.
(II) OLS with month fixed effects interacted with household exposure to pressure reduction measure.
(III) Household fixed effects estimation implemented by the demeaning method.
(IV) Household fixed effects estimation implemented by the demeaning method, and month fixed effects.

One concern was that the observed effect of the contingency tariff in these regressions is in fact attributable to the bonus, since the two programs overlapped in time, from January 2015 until both programs terminated in November 2015. This seems unlikely given that our bonus test is performed in period before the contingency tariff started, and no bonus effect was found at that time. Nor is there reason to believe that the bonus differently affected the households in the control and treatment groups in the contingency tariff test, either before or during the treatment period. The contingency DD design should account for any unobservable bonus influence.[10]

Another risk in our analysis is the potential imbalance in covariates between the treatment and control groups. To address this issue we used a propensity score to balance covariates between individuals who did and did not receive treatment. After constructing the propensity score, the next step was to use the propensity score to compare both groups.

We used a kernel weighting matching to employ the propensity score to select the control group. More specifically, a weighted composite of control group observations was used as a match for each treated household, with weights from a kernel function, and within a given bandwidth. This procedure maximized precision without decreasing the sample size, and does not make the bias worse, since it gives greater weight to better matches. The results are in line with those in Tables 3 and 4.

Another concern is whether, in the absence of all these interventions, the trends in consumption between the contingency tariff treatment group (households consuming more than 10 m3) and control group would continue in parallel. To examine this we ran a "placebo test" using the data for 2013 alone. We created two groups of about the same size and ran the same DD analysis supposing that the contingency tariff ran from July 2013 to November 2013. The treatment dummy for the contingency tariff in this period was not significant, which lends support to the parallel trends assumption.

The results point to an estimated contingency tariff effect of between -0.9 and -2.4 m^3 per household-month. To put these values into perspective, data from the 2010 Brazilian Census were used to weight the sample of households to match the population characteristics of São Paulo municipality. This made it possible to estimate the amount of water saved by the contingency tariff in the whole São Paulo municipality.

The census microdata comprise a sample of about 600,000 households (10% of the population of the municipality) and include data on household income and other household characteristics.

[10] We also tried restricting the sample to the households that, prior to the start of the contingency tariff, did *not* experience a consumption fall of 20% or more. The approach in doing so was that, if a household did not change its behaviour in line with the bonus before the contingency tariff, then it was unlikely to do so during the contingency tariff. The contingency tariff estimate remained within the reported range.

Weights were generated from a probit model of the combined samples (the FIPE sample used here plus the census sample) using the following variables: the number of bathrooms, the number of bedrooms, the number of living rooms, and the level of household income. Weights were generated to match the census sample of households.

In a second step, the weights were updated to match the characteristics of the whole municipality population. An iterative proportional fitting method[11] was used to recalibrate the combined sample weights to match the overall number of households in the municipality. The estimated effects were then used to produce an overall estimate of water saved by the contingency tariff while it was in place, as shown below.

[11] Described in detail in Kolenikov (2014).

Table 5 – Estimated water volume saved by contingency tariff based on treatment effect estimates

	Consumption in month prior to contingency tariff (m3)	Consumption change (m3)	Consumption change per month (m3)	Percentage change from pre-contingency tariff month
Sample	3,321	[-4,964] ; [-2,538]	[-292] ; [-149]	[-8.8] ; [-4.5]
Sample weighted for 10% of São Paulo municipality population	1,867,846	[-2,679,432] ; [1,369,754]	[-157,614] ; [-80,574]	[-8.4] ; [-4.3]
Sample weighted for entire São Paulo municipality population	32,770,427	[-47,009,282] ; [-24,031,650]	[-2,765,252] ; [-1,413,626]	[-8.4] ; [-4.3]

The overall water volume saved during the contingency tariff months is estimated at between 4.5 and 8.8% for the unweighted sample, and between 4.3 and 8.4% for the weighted sample. This is between 2.5 and 4.8% of the capacity of the Cantareira system.

These results can also be put into perspective by asking what the required price increase would be to cause a change in water consumption of this magnitude. Nauges and Whittington (2009) report a price elasticity of water demand for private connections in the range between -0.3 and -0.6. Considering the conservative end of these estimates, -0.3, the required price increase for attaining a reduction of water consumption of 4.5%, is 6.6%.

VII. Discussion and conclusion

We find no evidence that the financial reward-based bonus program caused households to reduce water consumption during the São Paulo drought. This does not mean that the program did not work, but that we were not able to find evidence that it worked. This conclusion is consistent with SABESP's own evaluation of the program's effectiveness (ARSESP 2015a), which was one factor that motivated it to implement the contingency tariff. A limitation to our bonus tests is that the two-month treatment period may be too short a window to observe a behavior response, due to the time it takes for information about such a program to disseminate across households and for households to adjust their routines.

The bonus finding presents a paradox, which is that despite finding no evidence that the program causally influenced consumption, the data show that many households did in fact experience a consumption decline of 20% or more, and that SABESP did in fact apply the 30% bill reduction to many households' bills. Our best explanation for this is that the consumption reduction was caused by SABESP's parallel measure of reducing pressure in the MRSP water network at night. *If this were true, it would mean that SABESP was reducing the supply of billable water to households while rewarding them for consumption reductions that the households did not themselves achieve.* The financial implications of these conflicting conservation measures are discussed below.

We find that the penalty-based contingency tariff reduced household water consumption by between 4 and 8% relative to unexposed households. This finding of the effectiveness of a penalty-based instrument is consistent with the findings of a wide body of water policy evaluation research (Pint 1999; Olmstead and Stavins 2009; Barrett 2004; Kenny et al 2008). In terms of policy design, the contingency tariff was not a straight price increase per se, but a shadow price increase on consumption that exceeded a household-specific historic mean. The authors are not aware of a policy like this that has been formally evaluated before, particularly as an urban drought management measure in a major middle-income country.

This research provided a concrete illustration of the "endogenous movers" problem in a DD research design in the water policy evaluation context. Estimating the model for only stayers roughly doubled the estimated contingency tariff treatment effect. An avenue for future research in DD designs is examining whether the treatment effect magnitude change is because individuals with "very high" values are now being compared with individuals with "very low" values, or because, within an otherwise compositionally similar sample, the elimination of real within-individual "switching" behavior is unmasking the true treatment effect.

A recent tariff filing by SABESP with the São Paulo state water and energy regulator shows the financial impact of the bonus and contingency tariff programs on SABESP's balance sheet. SABESP estimates that during the period 2014 to 2016 when both programs were in place, the bonus cost the company around US$ 694 million in bonus payments, while the contingency tariff added an extra US$ 373 million in revenue. During 2014 when only the bonus was in place, SABESP made around US$ 175 million in bonus payments. Whether or not a judgement on SABESP's response to the drought, the company's share price declined by about 30 percent during the worst 12 months of the drought (January 2014 to January 2015).

Paying households for consumption reductions that they did not themselves achieve does not make sense in a technical economic logic, but it may make sense in a political budget cycles logic. The conservation program that emerged immediately before the election compensated households for the welfare loss they experienced from their conservation effort, while the program that emerged in the post-election environment did not. This may be coincidence, or the result of policy experimentation and learning throughout the drought, but we feel it would be an incomplete interpretation of the findings to ignore the backdrop of heightened political accountability that preceded the election when the bonus was implemented. A topic for further research is understanding how high and low political accountability conditions influence conservation instrument design and scarcity response.

Global climate models predict that the risk of extreme weather events including drought will increase in the next decades (Touma et al 2015). Drought will be more disruptive in low- and middle-income countries where it threatens livelihoods and increases the incidence of social disruption and conflict. The São Paulo experience raises drought preparedness questions for water policymakers that extend beyond the technical, to the institutional. If water governance institutions weakly separate bureaucratic decision-making processes from political-electoral ones, then elected officials will be held accountable to the policies that arise, but they may produce ineffective policies in election years particularly. On the other hand, if bureaucratic decision-making is completely walled-off from political influence, the bureaucracy may produce more effective policy at all points in the election cycle, but at the expense of the accountability of elected officials to household-voters. The São Paulo experience suggests that greater autonomy for bureaucratic decision-making is preferable during periods when water for human consumption is scarce.

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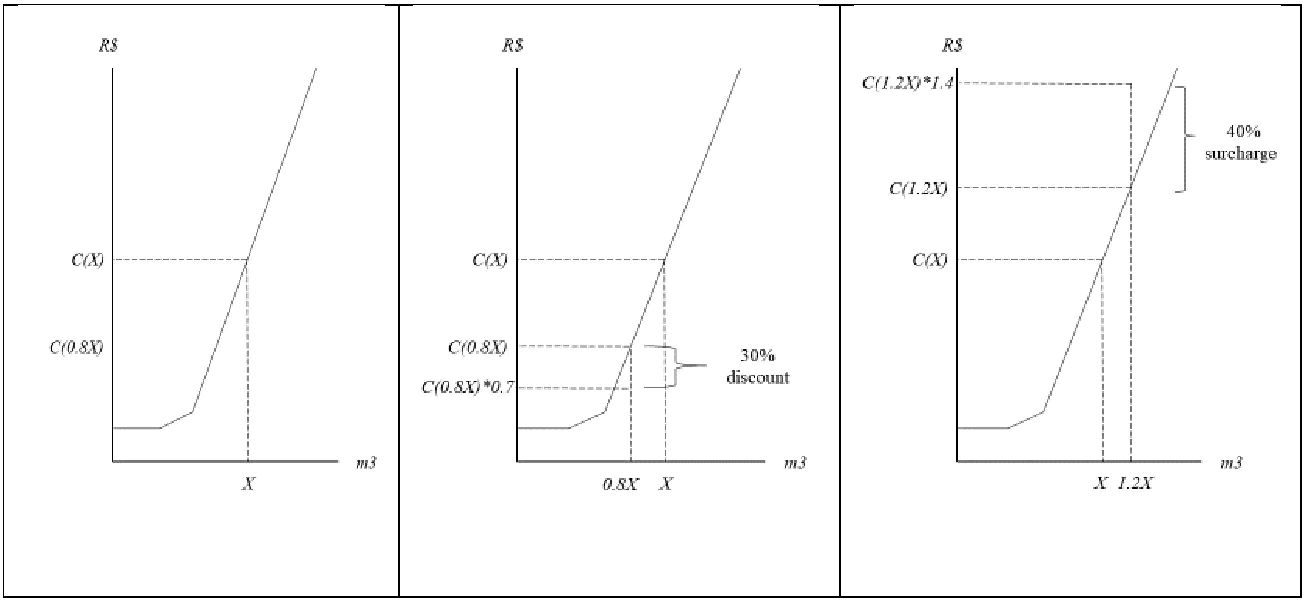
Appendix A: Illustration of bonus and contingency tariff

The figure below shows how SABESP applied the bonus and contingency tariff to a household's bill. SABESP customers face an increasing block tariff system. The kinks represent the marginal cost of consumption within each consumption block (only three blocks are shown for simplicity). The final bill is double the water charge to account for effluent services.

Figure 6: Bonus and contingency tariff in household water bill



At left, when a household's water consumption is ordinarily *X*, its bill for that consumption amount was *C(X)*. At middle, if a household reduced its consumption from *X* to *0.8X* before the bonus came into effect, it would have paid *C(0.8X)*. Under the bonus, the household received an additional discount that brought the final bill to *C(0.8X) * 0.7*. At right, if a household increased its consumption from *X* to *1.2X* before the contingency tariff, its bill would have increased from *C(X)* to *C(1.2X)*. Under the contingency tariff with the surcharge was applied, the total bill was *C(1.2X) * 1.4*.

Appendix B: Supplementary regressions

Table 6: Main results - full output

	I	II	III	IV
CANTSYS x BONUSPER	-0.09	-0.10	0.09	0.11
	[-0.19]	[-0.23]	[0.21]	[0.26]
CANTSYS	0.85	0.88	0.00	0.00
	[1.42]	[1.47]	[.]	[.]
BONUSPER	0.00	-0.16	1.02***	-0.64
	[0.00]	[-0.33]	[3.57]	[-1.68]
HICONS x TARIFPER	-0.93**	-0.95***	-1.82***	-1.46***
	[-3.31]	[-3.37]	[-6.92]	[-5.82]
HICONS	12.81***	12.79***	7.13***	6.37***
	[23.35]	[23.42]	[27.15]	[23.03]
TARIFPER	-1.25***	-1.39**	-0.78***	-1.96***
	[-3.47]	[-2.66]	[-7.88]	[-5.65]
NOPRV		-0.67		
		[-0.71]		
Month FE	Yes	Yes	No	Yes
Month x pressure reduction FE	No	Yes	No	No
Obs.	13600	13600	13600	13600
Unique HHs	340	340	340	340
R2 overall, adjusted	0.45	0.46	0.27	0.31

Note: Dependent variable is cubic meters of water consumption in each household-month.
Errors clustered on households, t-scores in brackets.
Constant omitted for presentation.
(I) OLS estimation, standard difference-in-difference set up, with month fixed effects.
(II) Same, where month dummies are interacted with household exposure to pressure reduction.
(III) Household FE estimation, demeaning method.
(IV) Same, with month fixed effects.

Table 7: Supplemental bonus tests

	I	II	III	IV	V	VI
CANTSYS x BONUSPER_L1	0.72*					
	[1.97]					
CANTSYS x BONUSPER_L2		0.41				
		[1.22]				
CANTSYS x BONUSPER_L3			0.07			
			[0.22]			
CANTSYS x BONUSPER				2.02	0.73	1.22*
				[1.45]	[0.87]	[1.97]
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Month x pressure reduction FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	13260	12920	12580	2720	5440	1360
Unique HHs	340	340	340	68	136	340
R2 overall, adjusted	0.31	0.31	0.31	0.30	0.34	0.037

Note: Dependent variable is cubic meters of water consumption in each household-month.
Errors clustered on households, t-scores in brackets.
Constant and coefficients on interacted variables omitted for presentation.
All specifications estimated with household FE, demeaning method.
(I) Bonus treatment period lagged by 1 month.
(II) Bonus treatment period lagged by 2 months.
(III) Bonus treatment period lagged by 3 months.
(IV) Sample restricted to lowest income households (1st quintile).
(V) Sample restricted to lowest income households (1st + 2nd quintiles).
(VI) Sample restricted to Dec 2013 - Mar 2014 to achieve symmetrical pre-treatment and treatment windows.

Table 8: 'Stayer' households only - full output

	I	II	III	IV
CANTSYS x BONUSPER	0.74	0.85	0.57	0.60
	[1.04]	[1.23]	[0.88]	[0.93]
CANTSYS	1.47	1.62	0.00	0.00
	[1.37]	[1.48]	[.]	[.]
BONUSPER	0.59	2.51*	1.94***	0.32
	[1.08]	[2.12]	[5.01]	[0.62]
HICONS x TARIFPER	-2.37***	-2.45***	-1.84***	-2.27***
	[-5.58]	[-5.25]	[-4.30]	[-5.36]
HICONS	19.62***	19.52***	5.97***	6.20***
	[19.96]	[19.32]	[10.32]	[9.58]
TARIFPER	-0.61	0.91	-0.74***	-0.79
	[-1.10]	[0.92]	[-3.88]	[-1.49]
NOPRV		1.13		
		[1.45]		
Month FE	Yes	Yes	No	Yes
Month x pressure reduction FE	No	Yes	No	No
Obs.	3526	3526	3526	3526
Unique HHs	139	139	139	139
R2 overall, adjusted	0.57	0.57	0.13	0.16

Note: Dependent variable is cubic meters of water consumption in each household-month.
Sample restricted to households whose membership in treatment or control group is stable ('stayers').
Errors clustered on households, t-scores in brackets.
Constant and coefficients on interacted variables omitted for presentation.
(I) OLS estimation, standard difference-in-difference set up, with month fixed effects.
(II) Same, where month dummies are interacted with household exposure to pressure reduction.
(III) Household FE estimation, demeaning method.
(IV) Same, with month fixed effects.