

ESTIMATING THE IMPACT OF PARTICIPATORY BUDGET ON OBSERVED OUTCOMES

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Resumo

Esse artigo testa se o Orçamento Participativo (OP) adotado por alguns municípios no Brasil teve algum impacto sobre a eficiência dos municípios ou sobre a distribuição de renda usando uma abordagem “quasi-experimental”. Os resultados são contraditórios mas apontam que o OP pode estar deteriorando a eficiência dos gastos em saúde dos municípios sem apresentar impactos positivos sobre a arrecadação. Um hipótese explicativa seria que o programa foi dominado por grupos de interesse que estariam captando mais renda do estado do que a coalizão anterior. Por outro lado, há alguma evidência de que houve um ganho de eficiência no fornecimento de educação pública o que confirmaria a hipótese alternativa que o OP quebrou uma coalizão perversa entre burocratas e a elite.

Palavras-chave: orçamento participativo, governância, experimento, democracia, empoderamento.

Abstract

This paper tests whether the Participatory Budget (OP) adopted by some municipalities in Brazil had any impact on the municipality efficiency or on income distribution using a quasi experimental approach. The results are mixed but they do show that OP municipalities may be deteriorating the quality of the health service delivery with no positive impact on the fiscal performance of the municipality. One explanatory hypothesis is that the program was dominated by groups of interest that are capturing more rent from the state than the former coalition. On the other hand there are some evidences of an improving in the efficiency of public education service what would confirm the alternative hypothesis that the OP broke the collusion between bureaucrats and the elite.

Key words: participation, governance, experiment, democracy, empowerment.

Estimating the Impact of Participatory Budget on Observed Outcomes*

1. Introduction

Starting in 1989 the new elected mayors in Brazil proposed an alternative for the budget proposal and approving process. Part of the budget would be decided in meetings open to all citizenship as happened in Porto Alegre one of the early adopters of the program. The change in the budget implementation following this scheme was generically referred as “Orçamento Participativo” (Participatory Budget - hereafter OP). In this paper it is assumed that the OP may represents an institutional change that represents a non-linear break in the public management. This paper argues that the OP is a change in the institutional infrastructure of governance.

Governance is defined very loosely in this paper but it is supposed to be mainly concerned with the “laws of the game” to use a game-theory jargon. In the broad conceptual framework of the “New Institutional Economics”, using Williamson (2000) survey, governance will be in the “third level scheme” of institutions. The first level is the most basic and is related to religion, social costumes and norms. It needs centuries to change. The second level is the institutional environment and is composed by the main laws such as the constitutions. It is expected to take decades to change. The third level sets the stage for playing the game. It includes the choice of appropriate schemes of governance for each type of transaction or organization. The fourth and lowest level includes routine economic interactions such as production, employment, etc.

The appropriate scheme of governance proposed in Williamson (1979) seminal paper is the one that minimizes transaction costs. Why would participatory democracy save in transaction costs compared to representative democracy? One possibility is that participatory democracy may be the less costly way to find out the citizens’ preferences. However, given Arrow paradox, it is impossible to prove that participatory democracy will necessarily reveal the citizens’ preferences because depending on the order of the votes or the hierarchy the election may have different outcomes. In this paper it is hypothesized that the OP may reduce the cost of revealing citizens’ preferences but that this is an empirical question to be tested.

So, the new institutional economics recognized a long time ago that inefficient forms of governance may retard economic growth and do not favor income distribution. Another branch of the literature deals more explicitly with the question why some societies are stuck with such inefficient institutional infrastructure of governance. Acemoglu, Ticchi and Vindigni (2006) show that, under certain circumstances, by choosing an inefficient state structure, the elite may be able to use patronage and capture the politicians. This enables them to reduce the amount of redistribution and public good provision even in democracy. Moreover, the inefficient state creates its own constituency and tends to persist over time. When the poor come to power in democracy, they will reform the structure of the state to make it more efficient so that higher taxes can be collected at lower cost and with lower rents for bureaucrats. Assuming that the participation in the OP process in Brazil is biased toward the poor, it is possible to expect, using this

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framework, both an increase in efficiency as in income distribution. Except if other groups have captured the participatory process as well¹.

The main attempt of this paper is testing whether the OP adopted by some municipalities in Brazil had any impact on the municipality efficiency or on distribution. In other words, it will test if the OP has reduced transaction cost of public management or the capture of the government by groups of interest. The idea is that people participating in the budget decision will be more willing to pay local taxes since (the argument goes) the citizens have more control over the allocation decision. It is also expected that there would be a general improvement in social conditions. There are two reasons for expecting better social outcomes in municipalities implementing OP. First, the OP opens space for the participation of the poor that may have less lobby power than the rich. Second its potential capacity to improve the quality of public spending. If citizens take part in the process of implementing municipality budget, government spending would be closer to their demands. That is, there might be “Pareto improvement” reallocations in the public budget that the local government would not find out unless “directly asking” the citizens.

The argument is reasonable but debatable. What is interesting about this discussion is that some of the claims made in the policy debate summarized above are testable using the panel built for this purpose and presented below. There is a rare opportunity since it is possible to pool together relevant data about the municipalities for 1991/1992, 1996 and 2000. Those are the last year of the tenures that took place between 1989 and 1992 (first generation of OP); 1993 and 1996; and 1997 and 2000 (and, of course, the previous year for the following tenure). Since just some municipalities adopted the program in the period analyzed it is possible to construct “control” groups to compare with the “treatment group” (municipalities adopting OP - hereafter OP municipalities).

If the “willing to pay” argument was correct we would expect that OP municipalities would have a better fiscal performance had they not adopted the program. In theory, we would expect a better performance in property taxes than in service taxes² since the focus of the program is the individual although small and medium enterprises might be well represented by their owners in the participatory process. Accordingly, we will attempt to test if there is any significant impact on the growth of total own revenue, property taxes or in service taxes.

The allocation argument is much more difficult to test and the problem is not data availability but difficulty in controlling for goals. For instance, we may have a municipality expanding “too much” in education and another one expending “too little”. Both adopted the OP and improved the allocation but the impact of the OP on education expenses is zero. The study attempts to check the impact on education and health expenses controlling for outcomes using years of schooling for education and infant mortality for health. It is expected that, if the allocation argument is correct, OP municipalities would have a lower change in education (health) expenses for a given

¹ As discussed elsewhere in this study, in Porto Alegre the participants in the OP process are biased toward the poor. It is not granted however that the rich will not capture the representative democracy as well. Many towns in the US have a budget process that may resemble very much the OP in Brazil but that are very much captured by the rich.

² The main sources of municipal (own) revenue in Brazil are the property taxes and a tax on services (a sale tax applied just to companies in the service industry).

change in years of schooling (infant mortality). It is also expected that OP municipalities would expend more in expenses actually controlled by the participatory budget process (investments) than on expenses not open for vote (operational costs specially wages). This possible allocation impact will be tested using either the variation in investment and in operational costs.

From the social point of view, the allocation argument should work the other way around, i.e. it is expected that OP municipalities would increase (reduce) years of schooling (infant mortality) for a given change in education (health) expenses. Notice that we are not worried about causalities in this sense. It does not matter for the purposes of this study if the (public) education system has (relatively) improved without changes in the expenses or if the expenses were (relatively) reduced not affecting the (public) education outcome. The program may have a budget improvement, a related social improvement or both. In other words, this paper is not testing how to improve the efficiency in education or health services but to assess possible (relative) impacts of the OP program on that efficiency.

Finally, the distributive hypothesis might be tested using data on poverty or a synthetic measure of income distribution such as the Gini index³. The problem is that such information is just available in demographic census years. So, the only way to test it is comparing two census years. Apart from technical issues there is a main difficulty in comparing indices that are 9 years apart⁴. The period comprises two tenures: one from 1993 to 1996 and another from 1997 to 2000. We could analyze just the municipalities that have implemented the OP on the two periods but this would give us just 17 municipalities making the statistical analysis not very robust. The alternative is testing if there was an effect on the municipalities that implemented the program in any tenure within the decade. The alternative would be testing if the OP had an impact on the number of illiterates or in the number of primary school drop outs what can be done for the two tenures separate⁵.

The OP in Porto Alegre has been considered as one of the main cases of success in governance at the sub national level. Accordingly, the program was considered one of the cases of best practice in the world by the 1996 meeting of the Habitat⁶. There is a consensus that the OP in Porto Alegre was responsible for a more progressive (spatial) expenditure distribution and increased citizenship. Notwithstanding, there are very few quantitative studies checking if those statement are correct. The few studies attempting to assess the impact includes Marchetti (2004), Baiocchi et al. (2006) and Zamboni (2007). To the best of our knowledge those are the only quantitative studies about the subject.

Marchetti (2004) was probably the first author attempting an empirical evaluation of the program. The author concludes that the OP in Porto Alegre has improved the (spatial) distribution of public goods. His research has some limitations though. The author

³ It is not so evident that the OP should affect the entire distribution. It is easier to accept that it would reduce poverty through a better (spatial) focus.

⁴ The last two Brazilian census were on 1991 and 2000.

⁵ The 1996 counting has also surveyed the education status of the individuals. Since there is a high correlation between poverty and illiteracy rate or primary school drop outs it is possible to indirectly test a reduction in poverty using those indices. The shortcoming is losing the municipalities that implemented the OP between 2001 and 2004 from the treatment group.

⁶ See <http://www.un.org/cyberschoolbus/habitat/dogood/dogood2.asp>

neglected a *contra factual* while studying OP since his analysis contemplated only the Porto Alegre case. The simultaneity with the new constitution is simply ignored compromising all results. Moreover, his attempt to show that OP induced the municipality to spend more in poorer regions is also debatable. In Porto Alegre there is a technical rule which privileges regions with higher population and lower average income. The result observed in the spatial distribution of municipal investment just shows that the rule works but does not guarantee that the OP helped implementing the rule. Therefore, it is not possible to separate the impact of the OP from the other events that were happening simultaneously.

Zamboni (2007) initial results suggest that municipalities that implemented the OP were less likely to have corruption than municipalities that did not implement the program. Using audit reports, Zamboni (2007) analyzes a set of Brazilian municipalities through a pair-matching analysis. He concludes that in municipalities where OP was implemented the likelihood of corruption was lower than in non OP municipalities. The author also suggests that the public participation in local public administration is more relevant to governance than the attendance to elections. These results confirm some of OP goals and suggest that the OP may act as an effective constrain to Executive during the budget implementation as well. Although Zamboni (2007) does have some methodological similitude with this study, the variable of interest in Zamboni (2007) is corruption while this study is mainly interested with fiscal performance, municipal resources allocation and welfare changes.

Although there are very few quantitative analysis of OP programs, there are an extensive literature on the possible consequences of OP implementation on democracy. Most of the studies are theoretical or case studies. The main conclusion from the studies is that OP drastically increases democratic participation. With direct consultation form, OP becomes a powerful tool to reduce problems of representative democracy. Its authors and supporters sustain that OP produces a “democratic radicalization” process. As Genro and Souza (1997) state, with no intermediaries between public needs and government action, OP programs would benefit the entire community.

Moreover, public participation would enforce a change in citizenship and consequently in public-private relationship. As the citizens get closer to public decision, they will better understand what is the government and the state role and will be able to improve their participation in public life. Citizens will become more engaged and concerned with the public sphere in a new way. This ‘new citizenship’ will be forged by public discussion and by the conflict resolution through public dialog.

Considering these points together, OP would guide the municipality to a higher development level. Silva and Carvalho (2006) state that this is one of the most relevant reasons given as a rationale for OP implementation. Higher level of democracy, more political engagement and better public expenditure. All together would induce the municipality to a much better situation had the municipality not implemented the program. It is interesting that so few quantitative analyses were undertaken since the claims are very much testable despite the technical difficulties.

Although OP claims to empower population in a country where the relationship between civil society and government is problematic, the lack of evaluation gives room to doubts about its capacity to reach that purpose. Critical studies are important because

they call the attention to an important fact that has not been systematically evaluated so far: how better is OP practices compared to budget practices introduced by 1988 Brazilian Constitution? Both were created at the same time and explanations that support the OP criticized a model that new Constitution was also attempting to change. The choice of direct democracy used in OP represents a high cost for the population. There is a clear trade-off that each individual faces comparing costs and benefits of taking part in meetings to decide the budget. The cost is relatively high and the benefits are uncertain and happen in the middle run: a new school, a new hospital or the pavement of streets take time to build and, thus, to perceive.

Fostering democracy, OP can be described as a new formal institution attempting to change patterns in the relationship between government and the civil society, depending on the previous history. Yet there is no formal model to OP in such approach; so far the literature focuses in describing its implementation and the main actors in the process. To the best of our knowledge, Baiocchi et al (2006) made the first attempt to formalize the circumstances that allow the raising of such practice. The report shows that municipalities ruled by PT (Labor Party) has (considerable) higher likelihood of introducing OP than a city ruled by any other party. There is also strong support for the effect of political and civil society factors (agents), as well as some support for the main constraining effect of social and demographic contexts, and clear support for the importance of municipal resource bases.

The main difference in this study and Baiocchi et al. (2006) is the unit of analysis and the methodology that may be clearer in the following sections. The unit of analysis adopted in this study is the municipality and the tenure. Baiocchi et al. (2006) uses a cross section computing the change in some variables between 1991 and 2000 as dependent, i.e. their unit of analysis is just the municipality. This study uses the difference in the outcome between each administration, i.e. 1992 to 1996; 1996 to 2000 and 2000 to 2004. The difference is not trivial since one cannot mix administrations when analyzing a public management issue. For instance, if the impact of the OP vanishes in four years, it would be not expected to find any impact in municipalities implementing the OP from 1993 to 1996 and then not keeping the program from 1997 to 2000. Also, there is a technical difference in the estimation technique. In this study the decision to implement (or not) the program is explicitly taken into account when computing the impact of OP in the variables of interest as will be discussed in the following section.

2. Econometric Strategy

The question that this report attempts to answer is essentially the same as the evaluation of any social program (e.g. a training program). A standard method for evaluating social programs uses the outcomes of non-participants to estimate what participants would have experienced had they not participated in the program. The difference between participants and non-participants outcomes would be the estimated gross impact of the program. The strategy proposed in this study is to use exactly this estimative as a measure of the impact of the OP on the selected variables, that is the gross impact of the OP will be measured by the difference between the outcomes of municipalities adopting OP (hereafter OP municipalities) *vis a vis* municipalities that did not adopt the policy (hereafter non-OP municipalities).

The main shortcoming of the analysis is the same as in the evaluation of social programs. The outcomes of non-OP municipalities may differ systematically from what the outcomes of OP municipalities would have been without adopting the OP, producing selection bias in estimated impacts. Following the literature on social program evaluation⁷ it will be assumed that each municipality has two possible outcomes: $\bar{Y}_t^{(1)}$ and $\bar{Y}_t^{(0)}$ in the OP and non-OP states, in period t , respectively⁸. The gross impact of adopting the OP could be measured by $\Delta_t = \bar{Y}_t^{(1)} - \bar{Y}_t^{(0)}$. The problem of evaluation arises because only $\bar{Y}_t^{(1)}$ or $\bar{Y}_t^{(0)}$ is observed for each municipality but never both. That is, it is impossible to know the outcome of a municipality had it not adopted the OP given that it adopted the program. The alternative is comparing the OP municipalities with non-OP municipalities conditional on observed variables.

To make this comparison the outcome of municipality i in period t (Y_{it}) is assumed to depend on a vector of (observed) municipalities characteristics (\mathbf{X}_{it}). The over performance associated with implementing a participatory budget process depend on a dummy variable (d_{it}) that equals 1 if the municipality implemented the OP in the previous period and 0 otherwise. To account for unobserved characteristics, a mean zero disturbance (U_{it}) is assumed. A linear version of this specification is:

$$\begin{aligned} Y_{it}^{(1)} &= \mathbf{X}_{it}\boldsymbol{\beta} + d_{it}\alpha + U_{it}, & t > k \\ Y_{it}^{(0)} &= \mathbf{X}_{it}\boldsymbol{\beta} + U_{it}, & t < k \end{aligned} \quad (1)$$

Where k is the period when the OP was actually implemented, $E(U_{it}) = 0$ and U_{it} is independently and identically distributed (i.i.d.) across municipalities. That is, the main goal of this study is estimating α . The decision of implementing a OP may be determined by the party in power, the (representative) voters or both. Whatever the specific content of the rule, it can be described in terms of an index function framework. Let IN_i be an index of benefits (to the appropriate decision maker) of implementing a OP. It is a function of observed (\mathbf{Z}_i) and unobserved (v_i) variables:

$$\begin{aligned} IN_i &= \mathbf{Z}_i\boldsymbol{\gamma} + V_i \\ d_i &= 1 & \text{iff} & \quad IN_i > 0 \\ d_i &= 0 & \text{otherwise} \end{aligned} \quad (2)$$

To estimate α a very complete set of information is need (that is rarely available) or additional assumptions have to be made regarding the (stochastic) dependence of the variables above. In many cases it is not possible to efficiently estimate α with cross-section data if there is no regressors in equations (1) and (2). Most cross-section estimators require a regressand in the decision rule (2) although it is possible to avoid most of the requirements with longitudinal data. The vector of municipal characteristics is not very problematic for this study since it is well established that 1. municipality

⁷ This goes back to Fisher (1935).

⁸ At this point I will be very generic, i.e. Y can be any variable of interest.

characteristics do play a role in the outcome and 2. there exists a large amount of municipality characteristics at least for Census years (1991 and 2000)⁹.

The decision rule is not so straightforward. In a training program it is standard to use a discrete choice approach where the decision to join the program would be taken if the cost to join the program was lower than the (expected) permanent income generated by joining the program. Given the evidence in Brazil, the decision is very much connected to the proportion of votes for PT in the first round. This fact was first emphasized by Baiocchi et al (2006) and was used to select the comparable pairs in their study. This paper uses this variable as one of the main determinants in the decision rule.

3. Data

For this study a comprehensive database was built that, to the best of our knowledge, has not been used before for analyzing institutional changes in Brazilian municipalities. The data span information from 1991/1992 to 2004 with intermediate information on 1996 and 2000. That is, the panel covers 12 year with four time periods¹⁰. As is well known if there is selection bias and time dependent measurement errors at least 3 time periods are needed to analyze the impact of an intervention. Besides the statistical advantages of such a panel, the periods are very appropriate for the study goals. Considering that the 1991 census data is a good proxy for 1992 (structural) variables, our periods are coincident with the mayor tenure (1993 to 1996; 1997 to 2000; and 2001 to 2004). So, the unit of analysis would be the municipality and its tenure. Given that the decision of having (or not) a participatory budget process is from the tenure this is a very coherent unit of analysis.

One of the main differences between this data set and most of the data sets used for analyzing outcomes of Brazilian municipalities is the collection of structural data for 1996. Most of the studies that needed structural variables compare 1991 with 2000 because they rely on census data. This study used data from the 1996 counting and data from administrative records (RAIS) to add data for 1996. The problem with this strategy is that there is not as much data for this year as there is for census years. In other words, there is a trade off between the number of variables available and the time periods analyzed. In particular there are no distributive variables for 1996 or 2004. So, the analysis on the distributive impact will take into account just the two census period.

The study compiled just fiscal and administrative records data for 2004. There is no structural source of data for this year. The idea is using lagged independent variables to avoid the problems related to measurement errors assuming that the measurement error is independently distributed across time. In other words all that is needed for 2004 is data for the dependent variable. So, it is not possible to actually use 4 time periods but just 3 of them in the specification used more often in the research. Either additional assumptions or instrumental variables will be used in analyses that use a sub-period of the panel.

⁹ As far as the dependent variable is not in Census it is possible to use all census variable as a control and do not lose the main attributes of the database built for this study, i.e. keeping the unit of analysis within the tenure period and controlling for fix effects. This issue will be discussed later in this paper.

¹⁰ Considering that the unit of analysis is the municipality the data is actually a panel and not a repeated cross section. However, it may be considered as a repeated cross-section if the unit of analysis was the individuals affected by the OP.

OP municipalities are in average more populated, richer, more productive, have higher formal wages, higher own revenues and more years of schooling, more infra-structure, etc. than non OP municipalities. So, there is a need for a controlled analysis. Regarding selection, as discussed above, one of the main differences in the two samples is the incidence of mayors from PT. From 1989 to 1992 more than half of the municipalities that have adopted the program were managed by a PT mayor. In this tenure PT had just 1% of the mayors in the country. PT managed cities continued to be prevalent in the municipalities adopting OP process in any period. In the 1993-1996 tenure and in the 1997-2000 period the share was similar to 1989-1992 (around 50%). The proportion diminishes to 30% just in the most recent period (2001-2004). Considering that the percentage of municipalities managed by PT have also increased considerably from the first to the last tenure under analysis (2% to 7%) the sample is more even in the last period than ever but it is still highly concentrated. Furthermore, in some of the municipalities implementing OP where PT was not elected, it had a large share on total votes or was in the winning coalition.

Analyzing the outcome variables also requires a careful analysis. As mentioned above, OP municipalities have much more resources than the average Brazilian municipality. So, a “naïve” straight comparison between OP municipalities and non OP municipalities averages will certainly leads to the conclusion that the outcome of OP municipalities was much higher than the non OP municipalities. On the other hand, since most of the OP municipalities started from a higher level in the outcomes they may growth at a slower (faster) pace if there is convergence (divergence) in the period.

So, the data shows that there are regressands both in the outcome, equation (1) as in the implementation decision, equation (2). So, using last section terminology, it means that it is necessary to estimate $E(Y_{it}|d_{it}=1, X_{it})$ and $E(Y_{it}|d_{it}=0, X_{it})$. The difference between the two estimates would be the ATE(\mathbf{X}), that is, the estimate of the average impact of the OP on the outcome conditional on \mathbf{X} . Assuming temporarily that there is no regressors in equation (2) and a linear behavior in equation (1), it is possible to estimate α regressing the outcome on 1, d and \mathbf{X} . The coefficient on d would be a good estimate for the ATE.

There is however a problem that usually arises in evaluating the impact of an intervention. To understand the problem, it will be assumed that \mathbf{X} has only one dummy variable x ¹¹. If all municipalities with $x=1$ participate in the program, it is not possible to estimate $E(Y_{it}|d_{it}=0, x=1)$, so it is not possible to estimate ATE(1). If some of the municipalities that have implemented the OP had $x=0$ and others not, it would be possible to estimate ATE(0) but the unconditional impact, i.e. $P(x=0)ATE(0) + P(x=1)ATE(1)$ cannot be calculated. This imply that the population of interest might be redefined.

A similar problem arises if the municipalities with $x=0$ never implemented the OP. Since it is not possible to calculate ATE(0) it is not possible to estimate the unconditional impact. The problem is a little different, though. In this case it is not possible to estimate the impact of the OP in municipalities that would never implement

¹¹ This follows Wooldridge (2002).

it anyway. In the first situation, it is not possible to estimate the impact on municipalities that always implement the OP. For instance, if all PT municipalities had implemented the OP it would not be possible to estimate the impact unconditional on the municipality being managed by a PT mayor. However, if none municipality managed by a PFL mayor had never implemented the program, it would be possible to estimate the program impact on all other municipalities including the municipalities managed by mayors from PT.

In practice there are both continuous as dummy variables determining the decision to implement the program. The strategy adopted was using a probit estimation for the likelihood of implementing the program. That is, it is estimated $p(\mathbf{Z}) \equiv P(d_{it}=1|\mathbf{Z})$ assuming a spherical error (the propensity score). The propensity score was also used for improving the estimation of the ATE both in cross section as in the panel. Since the normal distribution is implicitly assumed, it is not possible to have $p(\mathbf{Z})=0$ or $p(\mathbf{Z})=1$. The strategy is eliminating municipalities close enough to zero and 1 probability. It is not a considerable problem if part of the variables in \mathbf{X} are also in \mathbf{Z} as far as there is one variable that is not in both sets. As discussed in the next two sections, the probability of implementing the program is highly correlated with the number of votes for PT in the election but the performance is not correlated with this variable.

The redefinition of the sample is not very problematic in this analysis. Fortunately, there are enough municipalities managed by PT mayors that did not adopted the OP so it is possible to estimate the impact of the program on PT municipalities as well. Eliminating municipalities from PT would jeopardize the analysis. There are more cases with the second type of problem. For instance, although there are few municipalities from the PFL that indeed adopted the program, in 1992 no municipality managed by a PFL mayor have implemented the OP. Also, small municipalities and with low years of schooling have to be deleted from the sample. It is not possible to say for the municipalities that are very unlikely to implement such a program if it would pay off (or not) actually implementing it.

If all municipalities existent in 2000 existed already in 1991 and never split there would be 5,507 observation for each year, i.e. a total of 22,028 observations in the full sample. Actually, since the independent variables are lagged, there would be a maximum of 16,521 observations to be analyzed. However from 1991 to 1996 around a thousand municipalities were created in Brazil. Since the unit of analysis is the municipality and the tenure, it is not possible to analyze (in the full sample) municipalities created after 1991. But also municipalities that split in the period for creating a new one cannot be included in the sample. The sample is restricted to municipalities that were created before 1991 and had never split until 2004. Excluding municipalities that were created after 1991 or split in the period, there are approximately 3.5 thousand municipalities that can be analyzed.

Furthermore some variables are not available for the whole period. However, there is a large coincidence between municipalities that were excluded because it was not possible to estimate the unconditional and municipalities that did not have data. This coincidence is probably connected to the fact that smaller and poorer municipalities are less likely to report their data. Depending on the variables included the (pooled) sample used in the analysis varies from less than 4 thousand to more than 8 thousand observations but most

of the specifications have more than 7 thousand potential observations. A balanced panel would have around 6 thousand observations.

4. The decision to implement OP: an empirical approach

The decision to implement OP process by the municipality is clearly a political decision. In this paper it is assumed that this decision represents an institutional break. Since the decision was connected to the labor party (PT) taken the power, it may be argued as in Acemoglu et al (2006) that the poor was taken the power in an inefficient and captured (local) government. The institutional change was proposed to increase efficiency and distribution. The natural question is why would the PT need such a governance scheme since they had the power. In other words, why not implementing more efficiency and distribution directly?

One argument is that the inefficient government would provide more rent for the bureaucrats. There might be a hidden collusion between the rich and bureaucrats that would not allow the incoming party to change the status quo. The interrelation between technocrats and the elite in Brazil was studied in the seminal book by Bresser Pereira (1981) and point out in this direction. Acemoglu et al (2006) show that in order to generate enough political support, the coalition of the rich and bureaucrats may not only choose an inefficient organization of the state, but they may further expand the size of bureaucracy so as to gain additional votes. So, the OP would be necessary to break this coalition. This argument would explain why the process is very much biased towards PT administrations. The (very) recent spread to other parties may be a consequence of the success of the process and political competition pushing all parties to increase efficiency and distribution.

Another alternative would be that the OP was just an electoral strategy. The PT had to differentiate itself from other parties (and that may be the reason why Olivio Dutra defined the OP as “The PT way of public management”) and the OP was very appealing for this purpose. Also, the electoral scheme in Brazil is very much based on “political canvassers” (cabos eleitorais). Those are individuals that during elections are responsible for increasing the number of votes for the candidate he or she is supporting. The OP could generate a mass of political canvassers that would considerably increase the likelihood of (re)election.

In this section it will not be formally defined a model to explain the decision to implement (or not) the OP. Instead a very empirical strategy will be adopted. It will be estimated using a probit model which variables can significantly explain the likelihood that a municipality will adopt the OP. The goal of the empirical strategy is twofold. First it is important to understand the political decision to correctly analyze the impact of the OP on the variables of interest. Second it is important to check if the OP decision was correlated with any variable of interest. If this was the case, the study would have to deal with some estimation problems.

The variables expected to influence the municipality decision were briefly discussed above. First some fiscal variables may be connected to the decision. Municipalities with high tax in order to extract rent to the elite and bureaucrats would be more likely to implement such a scheme considering that Bresser Pereira (1981) argument formalized by Acemoglu et al (2006) is correct. This argument also suggests that it is a must to

estimate the influence of electoral variables on the political decision. Second, there might be a scale factor. Large cities may have resources to implement such a scheme that would not be possible for small cities. A similar argument can be made regarding rich cities *vis a vis* poor cities.

The first set of regression attempt to check for the fiscal variable and for the proxy for income if the logarithm is a better measure than the per capita measure. The analysis reveals that the per capita measure is better for income proxies while the logarithm is a better measure for fiscal proxies. We also test which fiscal variable (if any) explains the decision to implement the OP. In this case it is also important to check all variables that will be used as proxies for the outcome. It turns out that just own revenues are significant for explaining the probability of implementing the OP meaning that the analysis of public (local) expenditure performance will be easier to handle. Total own revenues, property taxes or service taxes are significant at 95%.

Using the service tax as a proxy for the influence on the fiscal side on the municipality decision whether to implement the OP it is appealing since it is expected that the property tax will be more affected by the program than the service tax. The problem is that service taxes are highly correlated with the income proxy (average wages in the formal sector). So, another proxy for income, the per capita gross “local” product (GLP) is tested along with service taxes. The GLP is less correlated with service taxes than the average formal wages but the results seem to be spurious as well. When one of the variables is not considered the significance of the other would increase consistently. It seems that the connection between the decision to implement the OP and fiscal revenues is more related to the scale argument than to the “state capture” one.

As stated before, the main variables influencing the decision to implement (or not) the OP is mainly political although the scale effect is very clear¹². The theoretical reason for the political influence is confirmed by the data. A PT mayor explains more than 1/3 of the decision to implement the OP. All other parties are not significant in defining if the OP will be implemented or not except the PFL that decreases the likelihood of implementation. Actually, from 1989 to 1992 and from 1993 to 1996 there was none municipality implementing OP from PFL. This result may confirm the hypothesis that the OP was part of the strategy to improve the efficiency and distribution given that the PFL is in theory the most right-wing party taken into account in the control variables. As a matter of fact considering that the most left-wing party is PT followed by PSDB, PMDB and PFL the signs and magnitudes of the party dummies are consistent with the statement that more left oriented parties are more likely to implement the OP.

Another political variable included in the regression was the political competitiveness. This variable is defined as the difference of the logarithm of the number of votes to the elected mayor and the second candidate in the first round. A small difference would mean that the competition is tight in the municipality. It was expected that this variable would be negative since more competition would encourage the mayor to adopt more efficient and distributive policies to increase the likelihood of re-election. The sign is the expected but the variable is not significant in most of the specifications.

¹² Biderman et al (2005) show that most of sophisticated public policies are very rarely implemented in small municipalities.

We also test alternative variables representing the political side of OP implementation decision. First, instead of using a dummy for PT two continuous variables were used: the votes for PT when the party elected the mayor and the votes for PT when its candidate was not elected. Both variables were significant but the sign of the later is negative. A possible interpretation for this result is that OP implementation was very much linked with PT. So, a politician attempting to differentiate him or herself from PT could not adopt a policy that is associated with PT. This result would reinforce the OP as an electoral strategy rather than a public management strategy. Then it was tested changing all party dummies by their interaction with the elected mayor votes. This specification do not change any of the results obtained when using the dummies. Then the political competition variable was decomposed in two: competition between PT elected mayors and other parties and the competition between non PT elected mayors and other parties. Just the competition between PT elected mayors and other parties was significant and positive meaning that more competition between PT and the second candidate would decrease the likelihood that OP would be implemented. Finally the political competition variable was decomposed further in 3: competition between non PT elected mayors with non PT second candidates; competition between non PT elected mayors and PT second candidates; and the competition between PT elected mayors and other parties second candidate already used in the previous specification. The later political competition variable was the only significant in both decompositions showing that the decomposition of the political competition do not add very much to the analysis.

Although per capita Gross Local Product or average formal wages seem to be a more direct measure of municipality aggregate income, a more indirect control for municipality income is tested namely years of schooling. As discussed before, years of schooling is also a proxy for (public) education outcome. So, it controls for the quality in education expenditure and is a measure of efficiency (controlling for expenditure in education). So, it is important to test the significance of years of schooling on the likelihood of OP implementation.

Including years of schooling as a proxy for income in the municipality makes the most significant fiscal variables (in the previous specifications) not significant at 95%. Also, years of schooling is the income proxy with more explanation power for the likelihood of implementing the OP. There are two possible explanations for this result. First the other proxies for income are usually measured with errors. The GLP is estimated based on other variables; average wages in formal employment does not consider the informal side of the economy (by definition) that represents in average 40% of total employment; the self declared income may be underestimated for personal concerns what is not so usual for years of schooling. The second possibility is that more educated citizens would be more willing to participate what is compatible with the political economy arguments discussed above.

Based on these specifications it is possible to choose a parsimonious specification where all variables are significant at 95% in most of the specifications. This specification is the natural candidate for the estimates of the impact of OP. In any specification municipalities that have implemented the OP in the previous year were more likely to implement it in the following year; larger and richer municipalities were more likely to implement the OP; municipalities where a mayor from PT was elected were more likely to implement the OP but an increase in the votes for PT when the mayor from PT was not elected reduce the likelihood of OP implementation; and it is

more likely to have OP in the 3 major regions in Brazil (Northeast, Southeast and South). The parsimonious specification kept the political variables that are not significant since they are not correlated with other variables.

We also run the parsimonious specification using the panel characteristic of the data, i.e. running a random effects probit model for the likelihood of OP implementation. The only change in significance is on the lagged OP implementation variable that was significant at 95% in every other specification not controlling for random effects, although the implementation in the previous period is still significant at 90%. This result is rather interesting since it shows that the significance of implementing OP in the previous year may be just proxying for idiosyncratic characteristics of the municipality.

It is interesting to notice that the PFL dummy is not significant for the likelihood of implementation from 2001 to 2004. Another related result is that the votes for PT when its candidate was not elected is also not significant in the last period of analysis. These results are showing that the OP program was moving from a “PT way of government” to a more general policy adopted by multiple parties. If the argument that the negative coefficient on the competition between non-PT and PT mayors (when the non-PT mayor is elected) is due to “party differentiation” is accepted, it seems that this differentiation is vanishing. However, the votes for PT when its candidate is elected is still very significant even from 2001 to 2004. So, it is still a policy very connected with PT but is not consigned to the party anymore.

5. Assessing the impact of the OP implementation

To analyze the impact of the OP controlling for observables 6 different estimators were generated. The first one is a naïve estimator just running an OLS regression including the main determinants of the performance variable but adding a dummy variable for OP implementation in the period. The second estimator is done in two steps. In the first step the probability of OP implementation is estimated using a parsimonious probit model and then adding the estimated probability to the OLS estimation. The third method uses the same two steps strategy except that the OP dummy is instrumented by the estimated probability and the other independent variables. Methods (4) to (6) is equivalent to methods (1) to (3), respectively, except that fixed effects are used instead of OLS.

The main result, i.e. the impact of the treatment on the treated, is presented on Table 1. The OP dummy is significant in some OLS estimations especially in method (3) where the implementation is instrumented. However, it is not significant in most results using fixed effects. So, results relying on OLS estimation like Baiocchi et al (2006) may be biased. Part of the endogeneity between OP implementation and performance is corrected by methods (2) and (3). The assumptions necessary to have the correct estimation of the impact of OP in method (3) are less restrictive than the assumptions needed for method (2). So, within the OLS estimators, method (3) is probably the most reliable. However, the fixed effect (hereafter FE) method is more reliable than any other OLS method since it is the only method that actually controls for the idiosyncratic characteristics of the municipalities.

Based on method (3) the evidence is showing that the OP had a positive impact on current revenues, on tax revenues and on property taxes but not on service sale taxes. This result is consistent with the expectations. However, this result does not survive the

FE estimation controlling or not for the implementation decision. Method (3) has also some results that, if accepted, would be bad news for the program. OP municipalities are expending more in operational expenses, education and health. Increasing the expenditures in operational items is not desirable since it would be expected that investment expenses would be increased. However, since the revenues were increasing (in general) it may be the case that all expenses increased as well.

Table 4: Impact of OP implementation on fiscal and social variables.

	(1)	(2)	(3)	(4)	(5)	(6)
Currente Revenue	0.092 (0.023)***	0.026 (0.029)	0.224 (0.043)***	0.015 (0.026)	0.020 (0.031)	-0.018 (0.06)
Tax Revenue	0.072 (0.053)	0.011 (0.066)	0.187 (0.092)**	-0.034 (0.063)	-0.039 (0.074)	-0.045 (0.136)
Property Tax	0.133 (0.083)	-0.023 (0.105)	0.319 (0.127)**	0.044 (0.076)	0.035 (0.086)	0.125 (0.151)
Service Tax	0.043 (0.087)	0.035 (0.105)	0.122 (0.146)	0.033 (0.114)	0.018 (0.127)	0.111 (0.231)
Operational Costs	0.130 (0.042)***	0.046 (0.046)	0.545 (0.103)***	0.012 (0.058)	-0.010 (0.062)	0.207 (0.185)
Investment	0.158 (0.071)**	0.031 (0.087)	0.354 (0.123)**	-0.122 (0.082)	-0.053 (0.094)	-0.214 (.178)
Education	0.126 (0.077)	-0.063 (0.093)	0.481 (0.121)***	0.010 (0.069)	0.007 (0.091)	-0.028 (0.157)
Health Expenditures	0.499 (0.102)***	0.185 (0.112)*	2.020 (0.254)***	0.364 (0.122)***	0.210 (0.126)*	2.095 (0.458)***
GLP	-0.106 (0.029)***	0.044 (0.036)	-0.365 (0.048)***	-0.023 (0.034)	0.024 (0.038)	-0.225 (0.079)***
Average Wages	0.028 (0.041)	0.028 (0.041)	-0.013 (0.071)	-0.051 (0.04)	0.003 (0.046)	-0.228 (0.085)***
Infant Mortality	-0.027 (0.021)	-0.027 (0.021)	-0.055 (0.036)	-0.029 (0.028)	-0.027 (0.031)	-0.054 (0.058)
Illiterates	0.000 (0.001)	0.000 (0.001)	-0.002 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.002 (0.002)
Drop-outs 1er school	-0.003 (0.001)**	-0.003 (0.001)**	-0.005 (0.002)***	-0.003 (0.001)*	-0.003 (0.001)*	-0.004 (0.003)
Years of Schooling	0.164 (0.052)***	0.164 (0.052)***	0.328 (0.084)***	0.145 (0.073)**	0.145 (0.073)**	0.104 (0.148)

*** - 1% sign.; ** - 5% sign.; * - 10% sign.

Source: Estimated by the author based on IBGE demographic census (2000).

A higher expenditure in either education or health controlling for the outcome is much more problematic. The positive impact is telling that an OP municipality spent more in education than a non OP municipality that had exactly the same performance in years of schooling. So, the efficiency of OP municipalities in education is lower than the non OP counterparts. The result is also observed in health expenditures. An OP municipality with the same change in infant mortality as a non OP municipality would expend more in health. For health the result is confirmed in most of the FE estimation what may be a very problematic result. That is, even considering that the OLS estimation should not be taken into account, the result for health expenditure survive the FE estimation and may be revealing some problems in the OP implementation.

Although the estimated parameters indicate a lower efficiency in health expenditure for OP municipalities *vis a vis* non OP municipalities, the result may be driven by the spillover problem in health services provision. As discussed before, the improvement in health public facilities may attract citizens from other municipalities to use those facilities. The health system may not work properly since the pressure from an increasing demand would require more resources allocated to health that would increase the demand further pressuring again the municipal health system. If OP municipalities originally increased the resources in health they may be facing this problem.

It is also possible that the OP did have a perverse effect on health services. For instance, the participation process may be inducing the municipality to over invest in health facilities instead of human resources connected to health. A reallocation of the resources from physical to human capital may increase the efficiency of the system. But the OP structure may favor physical capital over human capital since servant wages that represent a big part of the expenses are not subject to the participatory process. Of course the data collected and the methodology adopted for this paper does not allow such inference, i.e. it is not possible to assure if the OP municipalities are actually less efficient in Health or if they were serving a region larger than the municipality. Given those previous results, certainly, a more detailed study is needed to understand the impact of OP on public health services provision and maybe propose some specific policies to improve the process.

The result that is favorable to OP implementation is in the last row of Table 4. OP municipalities increased the years of schooling at a higher pace than non-OP municipalities. Since years of schooling is a good proxy for both income as poverty (more years of schooling being associated with less poverty) this is a very interesting result. OP municipalities were able to increase the welfare of the citizens faster than non OP municipalities. Since the way it is revealed is through formal education there might have been an increase in education expenditure efficiency that is not revealed in the education expenditure variable.

On the other hand, the results using method (6) are very unfavorable for the OP program. The problematic positive impact on health expenditure is significant using this method and the positive impact on years of schooling is not significant anymore. Besides, there is a significant and negative coefficient both on GLP as on average formal wages. This result means that OP municipalities production and income have grown at a lower pace than the non OP counterparts. These results are very worrisome since method (6) is the most robust method adopted in the estimation. It is revealing just costs related to OP and no benefits at least from the point of view of efficiency and aggregate outcomes.

6. Conclusions/implications

This paper attempted to assess the impact of implementing a participatory budget program on many fiscal and social variables. To perform this task a counterfactual type analysis was carried over. The impacted was assessed comparing the performance of municipalities that adopted the program (called OP municipalities) with municipalities that have not adopted the program (non OP municipalities) controlling for the main attributes of the municipalities (observed and idiosyncratic). Since some municipalities

implemented the program in different periods and the decision to implement the program is more connected to political (electoral) variables that are usually not related to the performance variable the program represents a unique opportunity to do a natural experiment analysis.

To the best of our knowledge this is the first attempt using this methodology and database. The main attribute of the database is that the panel is compatible with municipality mayor tenure. It is possible to collect data for 1992, 1996, 2000 and 2004 that corresponds to the last year of each tenure since 1992. Assuming that 1991 census data is a good proxy for 1992, it is possible to add information for 1992 and 2000. Since the analysis used lagged independent variables it is possible to analyze the municipalities implementing OP between 2001 and 2004 together with the municipalities implementing the program between 1993 and 1997. So, it is possible to explore the panel characteristic of the sample controlling for census variables. As is well known using census micro data it is possible to tabulate many indices at the municipal level that are impossible for any other period.

It is expected that the OP program will increase efficiency and distribution under some assumptions on the political equilibrium and the impact of the program on such equilibrium. This may be the case if the political equilibrium is “elite-bureaucrats coalition” type that may happen when the state is captured by the rich. To keep its rent capture it is necessary to adopt an inefficient state. To support such equilibrium in democracy the coalition with the bureaucracy should increase consistently the size of the bureaucracy in order to cope with the necessary votes (Bresser Pereira [1981], Acemoglu, Ticchi and Vindigni [2006]). Breaking those ties might be difficult especially considering the negotiation in the municipal legislative where most groups of interest are still represented. The OP moves part of the budget allocation decision both from the executive as from the legislative. If “participatory people” (that is, citizens that are willing to participate in the budget decision) is willing to change this situation, the OP would increase both efficiency in the public spending as distribution.

As it may be clear in the paragraph above, it is necessary to test two simultaneous assumptions that are not separable. It is assumed at the same time that the political equilibrium is “rich-bureaucrats coalition” type and that the OP would break this coalition. It is possible to test just one of the assumptions. In this report a non significant result was associated with a failure of the OP to break the coalition. However, if there was not such a coalition in the municipalities where the OP was implemented, the result would not be significant as well.

If there was no “coalition-type” problem in the municipal government it would be worth discussing why the program was implemented in the first place. There might be some other goals in the OP program that are not connected with efficiency or distribution. The non significance would be expected since those were not the goals of the program. In other words, the dependent variables were not actually representing the goals of the program. In this paper it was assumed that the goals of the program were toward allocation efficiency and/or income distribution through public goods allocation within the city. These might be the general objectives of the program but our variables may be beyond the OP capacity. Also the goals could be different such as improving the “empowerment” of citizens. One side of the empowerment goal was indirectly tested, though. The empowerment argument would imply that more residents (in average)

would be “happier” with the (local) government and will be more willing to pay taxes. So, OP municipalities would have larger fiscal revenue than non OP municipalities. However no evidence was found in this direction.

The likelihood of implementing the OP is very much connected to the PT performance in municipal elections. When the PT mayor is elected, more votes he or she gets, the higher the likelihood of OP implementation. However, if the PT mayor is not elected his or her votes reduce the likelihood of OP implementation. Such characteristic of the implementation decision suggests that the decision is very much identified with PT. A mayor in a municipality facing high competition from PT would differentiate him or herself not adopting the program. The result could also be interpreted differently. Municipalities with a strong PT would be the ones more likely to change the political equilibrium. So, if the winner party did not want to change the equilibrium, it could not adopt the OP. It is worth noticing, however that the proportion of mayors from PT reduced considerably in the last round of programs analyzed in this paper (i.e. from 2001 to 2004). This observation may be connected to the fact that the PT lost some unique identification characteristics that it used to have when it was smaller.

As most of sophisticated public policies, the OP is more concentrated in larger and richer municipalities. This is a problem since most of the performance variables selected for the analysis are also correlated with income or size. It is possible to control for this endogeneity problem however since PT votes usually do not influence the performance variables. It was also verified that the likelihood of implementation is probably not correlated with initial fiscal conditions. So, there is no selection bias problem from the fiscal point of view although there is selection bias in both population and income as for political party of the mayor.

One of the main contributions of this paper is working with a panel consistent with the mayor tenure. This characteristic allows the analysis to control for municipality idiosyncrasies using fixed or random effects estimates. Using this feature and estimating a random effect model to estimate the likelihood of OP implementation confirm the main findings except the dependence on the lagged implementation. All other specifications found that municipalities that have implemented the program in a tenure were more likely to implement it in the next tenure except the random effect specification. The implementation in the previous period may be more connected with idiosyncrasies of the municipality then with the previous experience.

The performance variables were selected to represent 3 groups of concerns. The first concern was with the empowerment hypothesis tested indirectly using fiscal revenues. The second concern was with public efficiency. To test for public efficiency expenditure data was selected and tested controlling for the service outcome. The third concern was with distribution. Distributive proxies were tested along with proxies for poverty. As expected most of the performance variables are connected with initial income of the municipality. The relationship with size is more complex. Some performance variables are not related with size. The more usual correlation favors smaller municipalities. Since OP municipalities are biased toward richer and larger cities, a simple comparison between averages could overestimate the difference because it did not control for income or underestimate it for not controlling for population.

In most of the specifications the performance variables were not correlated with PT votes as mentioned before. The political variable that did influence the performance more often was political competition measured as the difference between the logarithm of votes for the mayor candidate ranked first and second in the first round. Using a parsimonious version of the specification derived in this analysis 6 different methods were applied to estimate the impact of OP implementation on the variables of interest change in each mayor tenure. Four of the methods did control for the likelihood of OP implementation and 3 of the methods explored the panel characteristic of the data.

The program has not presented any significant impact in most of the performance variables in many specifications. For instance, in the methods estimated by fixed effects just 6 variables were impacted by the OP in 39 possibilities. The impacts are more pronounced in the methods that did not explore the panel characteristic of the data. In particular the procedure that instrumented for OP implementation using the estimated probability of implementation and all other attributes that define the performance as instrument, shows that OP was significant in 9 out of 13 variables analyzed. The non significance may be connected with many causes. There might have a measure problem in some variables that would be driving the results down. As a matter of fact, methods that used instrumental variables found more performance variables where the OP was significant. Also it may be the case that the impacts are not significant because some of the variables analyzed were very structural. So either the impact of the OP was not noticeable or it would require more time to show up.

There is also the problem in the measurement of the dummy variable. That is, we are mixing municipalities that may have different OP programs. Baiocchi et al. (2006) proposes a typology that separate binding from non-binding programs. Their case studies show a very different pattern in each type of program. However, for this heterogeneity to generate a insignificant result it may be the case that in a two-type space type I municipalities might perform worse so type II would perform better and the average would be insignificant. If this is the case it worth estimating the impact on each group since the type I (in this example) would be a bad policy while the type II would be a good one.

If the non significant results may not be telling a complete story of the program impact, results that are significant but in the opposite direction that would be a program goal might be a big concern for the policy maker. Some significant results are not very favorable for the program. Using the pooled regression with instrument variables, method (3), the revenue of OP municipalities was larger than the non OP municipalities for all revenue variables tested except for service sales tax what is compatible with the empowerment hypothesis. However, using this method, it has also a positive effect on operational expenses, reducing the amount available for investment; expenses on education and health, controlling for years of schooling; and on health expenses controlling for infant mortality.

An increase in education expenses controlling for years of schooling is a bad sign since it means that OP municipalities would expend more in education than non OP municipalities with the same outcome in education services measured by years of schooling. However this impact was significant just on method (3). However, the positive impact on health is significant at 90% for any method and at 99% in 4 methods. The OP seems to decrease the efficiency on health expenditure and not the opposite as it

was expected. As discussed before this might be connected to a spillover effect where OP municipalities furnish health services for the neighbor municipalities. It is possible to control for this effect using infant mortality over the number of infants actually treated in the city regardless of place of living.

On the other hand the program may have had a positive impact on education service outcome. In 5 methods the OP was significant and positive in the growth of years of schooling controlling for the municipality education expenditure change. Since education is very much connected with both income level and distribution this is a result favorable for the program. Also, the impact of OP implementation on primary school dropout rate is negative and significant at 99% in all non panel methods and significant at 90% in panel methods (4) and (5). So, the OP may be improving the delivery and distribution of education in the municipalities that have adopted the program. It may be argued that the way the state was captured was through the education system. The coalition between the elite and the bureaucracy would guarantee the inefficiency needed for capturing rent using the public education (non) delivery.

The only caveat with the above analysis is that the impact is not significant for both years of schooling as for primary school dropouts using method (6). Method (6) is the more robust method used in this paper. It controls for fixed effects, that is, adopt what is called a difference in difference approach and, at the same time, instrument for OP implementation to correct for problems in the dummy variable. Using this method the OP would have 3 significant impacts all toward inefficiency. The OP would increase expenditure in health without changing the infant mortality rate; OP municipalities' GLP would grow slower than in non OP municipalities; and the average salaries in the formal sector would grow at a lower pace in OP municipalities than in the non OP counterparts.

In brief, the results are mixed but they do show that there are many concerns with OP implementation. All concerns would need a more detailed study to fully understand it but it is possible that OP municipalities are deteriorating the quality of the health service delivery with no positive impact on the fiscal health of the municipality and may even be slowing up income and production growth. One explanatory hypothesis is that the program was captured by other(s) group(s) of interested that is (are) capturing more rent from the state than the former coalition. Another source of problem may reside on the proposed budget *vis a vis* the executed budget. The proposed budget may represent an improvement but the execution repeat the same standard experimented in the past. Finally, the lack of skill from the participants may end up proposing a less efficient budget than a panel of experts would produce.

In any case, it is not likely that any kind of OP would have negative impact. It seems that there might be a type of participation that it is actually deleterious. It is also possible that some programs might be good. However, in general, the program is probably neutral. In any case, the decision whether to implement the OP or not might be from the people in the municipality. The researcher might find some practices that are more efficient than others given that the municipality decided to implement such a scheme. It is probably impossible to say if OP is essentially bad or good. It is difficult to say that dictatorship is bad from the economic point of view, let alone participatory democracy versus representative democracy. Maybe a public vote together with mayor

election deciding if the municipality should have a participatory budget or not would be the edge of general policy suggestions for the OP implementation decision.

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