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ESSAYS IN POLITICAL ECONOMY

Renata Rizzi

Orientador: Prof. Dr. Fábio Kanczuk

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Prof. Dr. João Grandino Rodas
Reitor da Universidade de São Paulo

Prof. Dr. Reinaldo Guerreiro
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Profa. Dra. Elizabeth Maria Mercier Querido Farina
Chefe do Departamento de Economia

Prof. Dr. Pedro Garcia Duarte
Coordenador do Programa de Pós-Graduação em Economia

RENATA RIZZI

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RESUMO

Esta tese se divide em três partes. A primeira parte avalia a instituição do voto compulsório, proporcionando novas estimativas para os efeitos da obrigação de votar sobre os indivíduos. A estratégia de identificação se baseia no sistema dual em vigor no Brasil - voluntário e compulsório - sendo a exposição determinada pela data de nascimento. Usando as metodologias de RD e VI, e dados de uma pesquisa coletada especificamente para este estudo, concluímos que esta legislação leva a um aumento significante na participação política através do voto. Este aumento é acompanhado por uma elevação considerável na probabilidade de os cidadãos expressarem preferência por um partido político, mas não no seu nível de conhecimento sobre política. Além disto, concluímos que a primeira experiência de voto afeta permanentemente as preferências dos indivíduos. A segunda parte da tese analisa empiricamente episódios de calote da dívida soberana. Alguns dos aspectos fundamentais da literatura teórica sobre o assunto, incluindo a previsão de que quase todos os calotes deveriam ocorrer em "Períodos Ruins", não são confirmados pelos dados: mais de 38% dos calotes ocorrem em "Períodos Bons", sob a definição do filtro HP. Exploramos as características de cada tipo de calote e apresentamos evidência econométrica de que calotes na dívida externa em períodos bons em geral podem ser explicados por três componentes: (i) mudanças no ambiente político, (ii) aumentos nas taxas de juros internacionais e (iii) instâncias em que o filtro HP classifica um período como bom ainda que a real situação econômica seja bastante negativa. Por fim, apresentamos alguns resultados que sugerem que a duração do episódio de calote não depende substancialmente do tipo de calote em questão, mas sim do ambiente em que o calote ocorre. Tal resultado abre caminho para novas pesquisas sobre o acesso a mercados internacionais de crédito após calotes. A terceira parte da tese trata da questão de contribuições de campanha em troca de favores políticos (esquema conhecido como "*pay-to-play*"). Eu proponho um jogo simples para modelar os incentivos de partidos políticos e firmas de setores intensos em receitas públicas, e testo as implicações deste modelo usando dados de doações de campanhas e contratos públicos do Brasil. Os dados confirmam a hipótese de *pay-to-play*.

ABSTRACT

This thesis is divided into three parts. The first one evaluates the institution of compulsory vote, providing new estimates for the effects of the obligation to vote on individuals. The identification strategy relies on the Brazilian dual voting system - voluntary and compulsory - the exposure being determined by the date of birth. Using RD and IV approaches and data from a self-collected survey, we find that the compulsory legislation leads to a significant increase in voter turnout. These changes are followed by a sizable increase in the probability that individuals will express preference for a political party, but not by an increase in political knowledge among the population. Moreover, we find that the first compulsory voting experience permanently affects individuals' preferences. The second part of the thesis empirically analyses episodes of sovereign debt default. Some of the salient features of the theoretical literature on sovereign debt, including its prediction that almost all defaults should arise in "Bad Times", are at odds with the data: over 38% of defaults actually occur in "Good Times", as measured by an HP filter. We explore the specific characteristics of each type of default and present econometric evidence that failures to repay foreign debt in good times can, usually, be rationalized by three components: (i) changes in the political environment, (ii) hikes in global interest rates and (iii) instances in which good HP times actually take place under quite poor economic conditions. We also present some suggestive indications that the duration of the episodes does not vary substantially with the type of default that precedes them, but with the environment in which they occur, drawing some important implications for the understanding of economies' post-default market access. The third part of the thesis looks at the issue of campaign contributions in exchange for political favors (the so called "pay-to-play" scheme). I proposes a simple game to model the incentives of political parties and firms from public-revenue-intensive sectors, and test the implications of this model using data on campaign contributions and public contracts from Brazil. The data confirms the pay-to-play hypothesis.

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1 THE CONSEQUENCES OF BEING FORCED TO VOTE: EVIDENCE FROM BRAZIL'S DUAL VOTING SYSTEM[1]

1.1 Introduction

Most countries of the world follow a democratic system of government, in which elections are decided by the population (a strategy that seems consensual). However, from an individual's point of view, the chance of any singular vote to be pivotal is zero. The question of why people vote[2] is as intriguing as how and whether the voting process transforms citizens. Does voting make individuals more politically involved and informed citizens? If so, is this effect permanent or relevant?

The answers to these questions are less clear in the context of a forced democracy, imposed by compulsory voting legislation. This system is in place in 14.5% of the world's countries[3]; however, while it ensures more representativeness of the voting population, it is unclear whether and how citizens react to the compulsory voting legislation, especially those who otherwise would abstain from participating in the political process. A possible downside of the compulsory legislation is that these extra votes could negatively affect election results.

This paper aims to determine how exposure to the controversial legislation of compulsory voting affects citizens and elicits input into the discussion of forced voting. In order to overcome unobservable correlations between voting and preferences, we explore the dual voting system of Brazilian legislation, in which individuals between 16 and 18 years old are entitled to vote but are not required to, while those older than 18 are legally required to vote[4]. This legislation provides an exogenous shift in an individual's likelihood to vote, which is used to identify the causal effects of voting. The data come from a self-collected survey conducted during the week following the 2010 Brazilian Presidential Election. It consists of a

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[1] With Fernanda Leite Lopez de León. We are very grateful to Ernesto Birner and many other teachers at Anglo Vestibulares, Escola Estadual Professor Ascendino Reis, Escola Estadual Rui Bloem, Escola Estadual Professor Leopoldo Santana and Universidade de São Paulo for their support and help in the application of our survey. We thank CPP-Insper for the financial support.
[2] See Coate and Conlin (2004), Feddersen (2004).
[3] http://aceproject.org/about-en.
[4] The voting enforcement mechanism is explained in Section 1.2.

comprehensive set of demographic and political preference questions and a political quiz to evaluate respondents' levels of political information.

In the first part of this paper, we estimate the immediate effects of voting---that is, of being exposed to compulsory legislation---in a regression discontinuity fashion by comparing the political behavior among age groups around the threshold that determines exposure to different electoral institutions. First, we estimate the impact of the compulsory legislation on turnout. These estimates, which are new and interesting on their own, contribute to the vast political science literature[5]. We then estimate the impact of the legal requirement to vote on political preferences and the causal link between voting turnout and information. It is well documented that voters are better informed than non-voters and are citizens who are more likely to vote when provided with information (LASSEN, 2005, BATTAGLINI; MORTON; PALFREY, 2008 and BANERJEE *et al*., 2010). We examine the reserve causal effect, that is, whether people acquire information in preparation to vote once forced to vote.

The theoretical expectations are controversial. Rational choice theories built on a pivotal voter framework conclude in favor of a "rational ignorance hypothesis," stating that any single vote would make a difference and the cost of acquiring information would always exceed the benefit of voting (MARTINELLI, 2006). However, other models that depart from this framework find that acquiring political information can be consistent with optimal voting behavior (FEDDERSEN; SANDRONI, 2006; DEGAN, 2006). On the whole, whether people acquire costly information and get involved in the political process when exposed to compulsory elections is an empirical question, and experimental evidence is mixed in this regard[6].

While some literature has studied the association between forced voting and political behavior, these studies are based on cross-country comparisons[7] or field and lab-experiments[8]. This is the first paper that studies the relationships between compulsory voting,

[5] See Lijphart (1997) for a literature review.
[6] In a field experiment, Loewen, Milner and Hicks (2008) provided monetary incentives to a randomly assigned group upon the condition that they vote. They found that this group did not become more informed than the control group, whose members were not offered any incentive to vote. Seebauer and Grosser (2006) studied this relationship in the laboratory with a voting game using costly private information. They found that participants were significantly more likely to acquire costly information when they were forced to vote than when they had the opportunity to abstain.
[7] See Engelen and Hooghe (2007) and Czesnik (2007).
[8] See Loewen, Milner and Hicks (2008) and Seebauer and Grosser (2006).

turnout, information acquisition and ideology in a large-scale election under real-world incentives faced by individuals. Comparisons are made among similar individuals who face choices regarding the same politicians and presumably differ only in terms of their date of birth and consequently their voting requirements. Hence, we are able to identify the impact of the requirement to vote using a clear quasi-experimental design through a regression discontinuity (RD) approach. As a complement to this analysis, we conduct instrumental variable regressions (IV) using exposure to the compulsory system as an instrument for voting turnout in order to identify local average treatment effects of the requirement to vote.

More generally, this paper contributes to a large body of literature on the impact of voting turnout on individuals' citizenship[9]. Using US data, previous studies find that voting eligibility polarizes individuals (MEREDITH, 2009)[10]. It is plausible to assert that the opportunity to vote affects those who are willing to participate in elections. The estimated effects of democracy reported in this paper are more compelling and unanticipated, as they are based on compulsory voting. This is the first paper that estimates the causal effect of compulsory voting on political preferences and information acquisition of those who abstain from the political process (i.e., non-voters). Finally, our data also provide an opportunity to investigate the heterogeneous effects of voting among individuals from different socio-economic backgrounds. Perhaps the most relevant argument in favor of compulsory elections is distributive, as voters are more likely to come from higher socio-economic statuses than non-voters are (BENABOU, 2000). We are able to test whether and how these different groups react to the compulsory voting legislation.

We find that the requirement to vote increases turnout by 18 to 27 percentage points (p.p.) among the population, representing an increase of at least 50% in the size of the voting population. Results from RD and IV regressions and RD graphical inspection suggest a modest, if any, impact of compulsory voting on information acquisition. These are driven mostly by individuals whose mother has no college education. They increase their consumption of information through the internet and improve their comprehension about the

[9] Some of those find a positive relationship between voting and subsequently turnout (DENNY; DOYLE, 2009; GERBER; GREEN; SHACHAR, 2003; PLUTZER, 2002) or establish a causal effect of voting effect on subsequent political preferences (KAPLAN; MUKAN, 2011; ELINDER, 2011; MEREDITH, 2009; MULLAINATHAN; WASHINGTON, 2009). These studies are based on evidence from voluntary voting.

[10] Like this paper, he uses an RD approach to determine a voting effect. He compares party registration in California among those who, in the previous election, were "almost eligible to vote" with those that were "just eligible to vote."

political spectrum. On the other hand, we find that individuals become significantly more likely to express preference for a political party and to become more ideologically polarized among those with an educated family background.

In the second part of the paper, we test whether this voting effect is permanent and whether it varies with more voting experience by examining how the number of experienced compulsory elections correlates with individual political preferences. To separate the effect of voting experience from aging, we conduct regressions controlling for year-of-birth fixed effects, quarter-of-birth polynomials and alternative specifications using year-of-birth polynomials as control variables. We find that, after exposure to one compulsory election, citizens become approximately 15% more likely to align with a political party. This effect lasts for at least three election cycles (or six years). Further voting experience has no additional impact on individuals' political preferences. They point to the important role of voting, even when imposed, in increasing individuals' involvement with politics. These results are new and relevant to a full comprehension of how democracy affects individuals and the consequences of compulsory voting[11].

This paper proceeds in four sections. In Section 1.2, we explain the Brazilian electoral institutions and describe the data. In Section 1.3, we present the results relating to the effect of being forced to vote on information and ideology. Next, we estimate the frequency and permanent effects of voting on political preferences. We conclude in Section 1.4.

1.2 Data

1.2.1 Some Background on the Brazilian Election System

Mandatory voting was introduced in Brazil in 1932, when the country's first Electoral Code was created following the Revolution of 1930[12]. In 1964, a coup d'état initiated a period of 21

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[11] There is extensive literature related to this topic as summarized by Lijphart (1997). He informally discusses the distributive advantages related to the increase in turnout. Other studies discuss welfare implications related to this possible change in election outcomes using a theoretical framework (KRASA; POLBORN, 2005, KRISHNA; MORGAN, 2011).

[12] One of the principles of the Revolution was the moralization of the electoral system. One of the first acts of the provisional government was the creation of a commission to reform the electoral legislation. Advances in the electoral legislation were subsequently included in the Constitution of 1934; in 1937, however, a new

years of military rule in the country, during which the regime controlled the electoral process according to its interests through a series of institutional acts, constitutional amendments, laws and decrees. Direct elections for president, governors and mayors of strategic municipalities were suspended, and existing political parties were again extinguished. A new transition to democracy began in 1985, when a constitutional amendment re-established direct elections in the country, reinstating the right to vote (rather than the obligation) for those older than 18 and extending it to illiterates.

In 1988, the current Brazilian Constitution was promulgated, adopting compulsory voting (henceforth, CS) for literate individuals between 18 and 69 years old and voluntary voting (henceforth, VS) for citizens who are illiterate, over 70 years old, or between 16 and 18 years old (TSE)[13]. Democratic elections are currently held every second year in Brazil. All voters must register; when individuals who are required to vote fail to do so and fail to provide justification to the electoral authority, they must pay a small fine[14].

Stronger sanctions are applied to those who fail to justify their absence for three consecutive elections; they are not allowed to issue or renew their passports and national identity cards and also become ineligible for public education, public jobs, cash transfer programs and credit by financial institutions maintained by the government. The legal requirement refers to showing up at the polls; all voters have the option of casting an invalid vote (this option is available on the ballot).

One can claim that voting is not in fact compulsory in Brazil, since the option of justifying the absence is available. However, this practice is not commonly used. According to records from the Tribunal Superior Eleitoral (TSE), in the 2006 Presidential Elections, 83% of the total electorate opted to turn up at the polls instead of justifying an absence[15]. Official records only

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constitution was imposed by President Vargas extinguishing the Electoral Justice, dissolving the existent political parties and suspending direct elections. The deposition of President Vargas in 1945 marked the redemocratization of the country, with the reestablishment of the Electoral Justice and the restoration of rights suppressed in 1937. Voting once again became mandatory for all citizens over 18, except for military officers and citizens over 65 years (illiterates were not allowed to register).

[13] www.tse.jus.br/internet/ingles/historia_eleicoes/eleicoes_brasil.htm

[14] In 2011, the fee was between R $1.06 (US $0.66) and R $3.51 (US $2.19), which is equivalent to 0.29% of the average income in the country, according to IBGE, Population Census 2010.

[15] This includes Brazilians living abroad or in cities others than those in which they are registered. Brazilians can only vote in the states in which they are registered, and they can only vote in person. According to TSE, 40.8% of Brazil's residents that justified their absence in the 2006 Election were living in different states from where they were registered.

give information about turnout and only at the aggregate level. An analysis like the one proposed in this study demands survey collection. This took place in the week immediately following the first round of the 2010 Presidential Elections (October 3). At that time, there were three main candidates running for election: Jose Serra, Marina Silva and Dilma Rousseff. Their share of votes accounted for 98.8% of the total valid votes[16].

1.2.2 Survey, Sample and Descriptives

A total of 5,559 students were surveyed in their classrooms from October 4-7, 2010. To exploit the dual system, we conducted the survey among individuals who face compulsory voting and among those who face voluntary voting. The survey sample included students in three types of institutions---public high schools, a preparatory school for college admission and a large university---in 109 classrooms in the city of São Paulo, Brazil[17].

While conducting the surveys, the same procedure was applied across all institutions: An interviewer entered the classroom about 15 minutes before the end of a class, read an introductory script, and distributed the questionnaires to all students. Teachers also collaborated in this project, soliciting attention and consideration to the survey. Students then had 10 to 12 minutes to individually answer the questions. To limit strategic responses, survey participants were not informed about the precise purpose of the survey, and there was no specific mention about compulsory or voluntary electoral systems. The survey was entitled "Young Adults' Political Behavior" and related to the Universidade de São Paulo[18].

In every classroom, four types of questionnaires containing exactly the same questions but in different orders were randomly distributed to students in order to prevent cheating. The survey consisted of a comprehensive set of questions about demographics, political inclination, vote, media consumption, sentiment toward voting and a political quiz to evaluate the respondents' levels of political knowledge. Most students agreed to answer the survey, and

[16] In the 2010 Election, no candidate received more than 50% of the valid votes in the first round, so there was a runoff between the two leading candidates. In the second round, Dilma Rousseff beat Jose Serra by 12.2 p.p. (56.1% versus 43.9%).

[17] São Paulo is the largest metropolis in Brazil and among the cities with the highest income per capita in the country.

[18] After returning the completed questionnaire, students received an information sheet containing contact information for the authors.

93% of the respondents declared to have answered it in a serious manner. Respondents had been told they could skip any question, but the vast majority of students answered them. Regarding one sensitive question (whether they had voted and for whom), only 1.26% abstained from answering; 0.27% chose the alternative. In addition, only 2.36% of them skipped open questions in the political quiz. These high rates of participation might be a response to their teachers' request to collaborate and take the survey seriously.

The sample is composed partly of high school seniors from three public high schools: Escola Estadual Professor Ascendino Reis, Escola Estadual Rui Bloem and Escola Estadual Professor Leopoldo Santana. The second sample is composed of students taking a preparatory course for college admission exams (cursinho) at Anglo Vestibulares. These are referred to as Anglo students. They are mostly high school seniors or students who just finished high school but have not yet been admitted to college. While public high school and Anglo students have similar ages, they differ in socioeconomic characteristics, the latter group being more affluent. The last sample consists of freshmen from the Universidade de São Paulo (USP). We surveyed freshmen from the following majors: history, sociology, business administration, economics, physics, architecture, law, mathematics and languages. The students in this sample are older. From the 5,559 surveys collected, 3,703 were completed by Anglo students, 728 by public high-school students and 1,128 by college students. In the Appendix, Table A describes the socioeconomic characteristics for these three samples.

Table 1 shows turnout rates (as a fraction of total population) by age group for Brazil and within the sample. Turnout rates are higher among individuals who face compulsory voting. Of note is that, over-reporting of voting turnout is a recognized issue in surveys (ANDERSON; SILVER, 1986). While this is possible among our survey participants, the resemblance between the rates of self-reported turnout by age in our sample and official rates in the country, as shown in Table 1, is much higher than typically reported in international surveys in the US or the UK, for example[19].

In section 1.3.1, we address this issue in greater detail and provide some evidence that voting over-reporting does not affect our estimated effects of turnout.

[19] For example, Swaddle and Heath (1989) finds that reported turnout in the 1987 British General Election Study was 10 p.p. higher than the official rate, and larger discrepancies are detected in the American National Election Study in the US, as reported by Anderson and Silver (1986).

Table 1: Turnout in Brazil

Turnout (%)		
Age Group	**Brazil**	**Sample**
16	17.7	17.6
17	42.6	39.7
18 to 20	82.6	85.6

Note: Brazil's turnout is from TSE and IBGE

Through this paper, we examine five main set of outcomes related to information and ideology. The first outcome is the respondent's actual knowledge about politics, measured by the performance in the political quiz[20]. The second is based on the performance in one of the quiz questions[21]. The outcome is an indicator of whether individuals were able to correctly distinguish the most right-wing party among two extreme alternatives (distinguish right-wing party). The third set of outcomes are several mechanisms of information acquisition, namely the number of days an individual consumes politics in the media per week via several outlets---TV, newspaper or magazines and the Internet---and whether he discusses politics frequently with parents or friends.

The remaining outcomes are ideology measures. The fourth outcome is based on students' ideological position. We record whether a respondent was self-declared extreme oriented---strongly left- or right-wing---as opposed to moderately left- or right-wing or center oriented. The fifth outcome is a measure of individuals' partisanship. We asked, "Do you have a preference for a political party?" We classified those who answered positively to the question as Partisan. Whereas these are only self-reported measures of ideology, they are highly correlated with candidate voting choices and political engagement. While partisans vote at least 70% of the time for the candidate from the preferred party and only 2.1% declare the intention to invalidate their votes, non-partisans are equally likely to vote for any of the three

[20] The quiz consisted of 14 questions, which are provided in the appendix. Twelve were about the three main candidates running in the presidential elections. More specifically, there were three open-ended questions about the previous political experience of each of the candidates, and four multiple-choice questions about policies previously implemented or supported by the candidates.

[21] Question 14 in the quiz.

main candidates, and 7.8% plan to invalidate their votes. A preference for a political party may convey conviction for its policy and thought for choosing a preferred alternative[22].

Table 2 shows the descriptive statistics of respondents' behavior and characteristics for the whole sample (Column 1) and a comparison according to their voting turnout decisions in the 2010 Presidential Elections (Columns 2 and 3). Out of the 5,559 students surveyed, 77.1% declared to have voted in the 2010 Presidential Election. Voting turnout is positively correlated with exposure to the compulsory voting system, which explains, at least in part, why voters are older than non-voters in the sample.

Consistent with evidence from the US (DEGAN; MERLO, 2011), voters consume more information and are better informed about politics. They are also more likely to declare a preference for a political party and are more politically polarized (more likely to self-declare as right- or left-wing). In terms of demographic characteristics, voters are slightly older, richer, more likely to be white, and more likely to have a mother with a college degree than non-voters are.

[22] The data also show that partisans are more knowledgeable and more positive towards voting than non-partisans. All of these patterns are consistent with the understanding about determinants of alignment with a political party (BRADER; TUCKER, 2001; MILLER; SHANKS, 1996).

Table 2: Outcomes and Characteristics of Individuals by Turnout Decision

Outcomes	Total (1)	Voter (2)	Non-Voter (3)	Difference (2)-(3)	
Political Information (in %)					
Political Quiz (% correct answers)	58.38	60.63	50.94	9.69	**
Able to distinguish which party is more right oriented among two choices (%)	75.23	79.13	62.34	16.79	**
Number of days a week learns about politics from:					
TV news	3.47	3.50	3.39	0.10	
Newspapers and magazines	2.45	2.55	2.14	0.41	**
Internet	3.44	3.61	2.87	0.74	**
Talk to parents frequently about politics	43.14	45.89	33.86	12.03	**
Talk to friends frequently about politics	46.2	46.56	44.89	1.67	
Political Inclination and Voting (in %)					
Has a political party preference	35.62	38.99	23.96	15.03	**
Extreme Left-wing	3.42	3.49	2.57	0.92	
Moderately Left-wing	23.01	28.05	20.15	7.90	**
Center	48.65	46.06	58.21	-12.15	**
Moderately Right-wing	23.27	25.88	21.64	4.24	**
Extreme Right-wing	1.65	1.59	1.82	-0.23	
Voted in the 2010 Election	77.07				
Voted before the 2010 Election	33.91	37.98	19.79	18.19	**
Characteristics					
Age	19.16	19.43	18.19	1.24	**
Female (in %)	57.09	56.66	59.16	-2.50	
White (in %)	76.32	78.00	70.93	7.07	**
Mother education : college or more	67.62	70.42	58.83	11.59	**
Has the Requirement to Vote (%)	80.09	90.17	45.89	44.28	**
Number of Observations	5,514	4,250	1,264		

Note: **Significant at the 5 percent level.

Differences between voters' and non-voters' preferences and attitudes can cause and/or be caused by voting. In order to overcome this endogeneity issue and estimate the causal effects of voting on behavior, we explore exposure to the compulsory legislation using a regression framework.

1.3 Results

1.3.1 The Effects of Being Forced to Vote

In this section, we present the results of the immediate impact of exposure to the compulsory voting legislation. We restrict the sample to individuals that could face up to one compulsory election in order to estimate the effect of just being forced to vote. First, we present the results from a regression discontinuity framework to identify the effects of the introduction of compulsory voting legislation in the population's behavior. Then we show graphical evidence suggestive of a causal effect of forced voting on turnout and on other political outcomes. Finally, we conduct IV regressions to quantify voting causal effects.

1.3.1.1 Regression Discontinuity Results

We use a sharp regression discontinuity framework comparing individuals whose ages are around the threshold that determines the change from the voluntary to the compulsory voting system[23].

We estimate the following equation:

$$y_i = \gamma + m(S) + \beta_1 1(S > 0) + u_i(1) \tag{1}$$

where y_i is the outcome of individual i, $m(S)$ is a continuous function of S, or the distance between the 2010 Election Day and the date the individual had turned or will turn eighteen, $1(S>0)$ is an indicator equal to one if the respondent was required to vote on 2010 Election Day and u_i is a random error term. We estimate (1) assuming a lower-order polynomial functional form for *m( )* that is flexible on each side of the cutoff, and clustered standard errors on classrooms. In addition, we estimate the effects controlling for demographic observable variables.

A possible concern is that the results may be sensitive to outcome values for observations far from the cutoff that determines the change in the voting system. For this reason, our estimates

[23] The sharp RDD design is equivalent to the case in which respondents have a perfect knowledge about their voting rights and obligations. In an earlier version of this paper, we estimated the causal effects of voting in the context of "fuzzy RDD." We tested whether political outcomes responded to the perception of the obligation to vote, when this variable was instrumented by exposure to compulsory voting. The results are similar and have the same qualitative implications as the ones presented in this section.

use only data within a bandwidth of 15 months from the cutoff, excluding individuals that faced more than one compulsory election or that had not yet had the opportunity to vote.

The identification relies on the orthogonality assumption between age and unobservable factors (such as political preferences that determine individuals' political outcomes) for those close to turning 18 (under VS) or those who have just turned 18 (under CS) by election time. This assumption cannot be entirely verifiable; however, it can easily be rejected. As discussed by Lee and Lemieux (2010), a simple test is to fit regressions for possible confounding variables and test for jumps at S=0. We estimate (1) using several covariates, such as demographic characteristics (X_i) as the endogenous variable. Table 3 shows the results. As discussed in Section 1.2, participants in different schools differ in terms of demographics and age. For this reason, our analysis is conducted by exploring variation within schools. Table 3 shows these results. Coefficients were not statistically significant for most of the variables, including demographics and family characteristics[24].

One relevant exception is previous voting experience. The 2010 Election was the first opportunity for all respondents in this sample to vote in a presidential election; nonetheless, a small part of the participants older than 18 had the opportunity to vote in the 2008 local elections. The predicted fraction of second-time voters on the right side of the threshold is approximately 5% higher than on its left side. The fact that local elections are not as renowned as presidential elections and that the fraction of second-time voters is small in comparison to the change in turnout (as it will be shown in Table 4, Figure 2) gives us some confidence that this is not a relevant confounding.

Since the effects are identified at the age of 18, one can claim that, at this particular age, youngsters start feeling more responsible because they reach the legal majority, which could result in a confounding factor. We obviously accept the fact that other opportunities and responsibilities that become available after one turns 18 might change individuals. However, we believe that this happens gradually and not abruptly at the 18th birthday. We tested whether students changed their behavior regarding their propensity to apply for college admission exams or to respond seriously to the survey at 18-year old threshold. In line with our expectations, none of these changed (Table 3).

[24] For the specification in Column (1), we find that females are under-represented on the right-side of the threshold. In spite of that, we do not find evidence that gender explains voting behavior in our sample.

Table 3: Estimated Discontinuities in Pre-determined Characteristics

	(1)	(2)
Sample: All		
Dependent variable:		
White	0.0046	0.0142
	[0.0300]	[0.0422]
Female	-0.0816	-0.0589
	[0.0355]**	[0.0485]
Mother has some college education	0.0362	0.0447
	[0.0293]	[0.0400]
Mother has a political party preference	-0.0238	0.0281
	[0.0399]	[0.0549]
Live with a parent	-0.0205	0.0317
	[0.0224]	[0.0351]
Attend church frequently	-0.0659	-0.0544
	[0.0439]	[0.0571]
Plan to apply to College	-0.0047	0.0123
	[0.0226]	[0.0322]
Responded seriously to the survey	0.0161	0.0056
	[0.0183]	[0.0244]
Voted before the 2010 Election	0.0552	0.0406
	[0.0134]**	[0.0181]**
Sample: Voters		
Political Quiz (% correct answers)	-0.0117	-0.0098
	[0.0158]	[0.0208]
Age polynomial controls	linear	quadratic

Notes: 1) Explanations about the samples are in the text. 2) Standard errors robust to heteroskedasticity are in brackets. Entries are estimated regression discontinuities at S=0, from models that include age polynomial controls for S fully interacted with a dummy for age 18 or older. Other controls include school fixed effects. 3) **Significant at the 5 percent level.

Lastly, there is concern about the fact that the survey information is based on self-reported behavior. This could result in non-random sorting across the threshold (i.e., the choice to participate in the survey correlates with participation in the election). In this case, a jump in the number of observations around the threshold would occur. Figures 1A and 1B show a plot with the number of observations by age and the percentage of observations with non-missing reporting values in any of the characteristics controlled in the main regressions, respectively. There are no visible discontinuities around the threshold for both of these variables.

A related concern is that voting over-reporting might be accentuated among those exposed to the compulsory system, if those legally obliged to vote feel more compelled to declare to have voted. One simple test to detect such behavior is to verify differences among voters' level of political knowledge around the 18-year-old threshold. This is a proxy for actual vote participation, given that non-voters are less informed than voters as shown in Table 2. We conducted RD regressions restricting the sample to voters and found no difference among voters on either side of the 18-year old threshold, as illustrated in Table 3[25]. Graphical evidence is presented in the Appendix. This gives us some confidence that respondents have been sincere about their voting participation and that over-reporting is not biasing our estimates on turnout effects presented next.

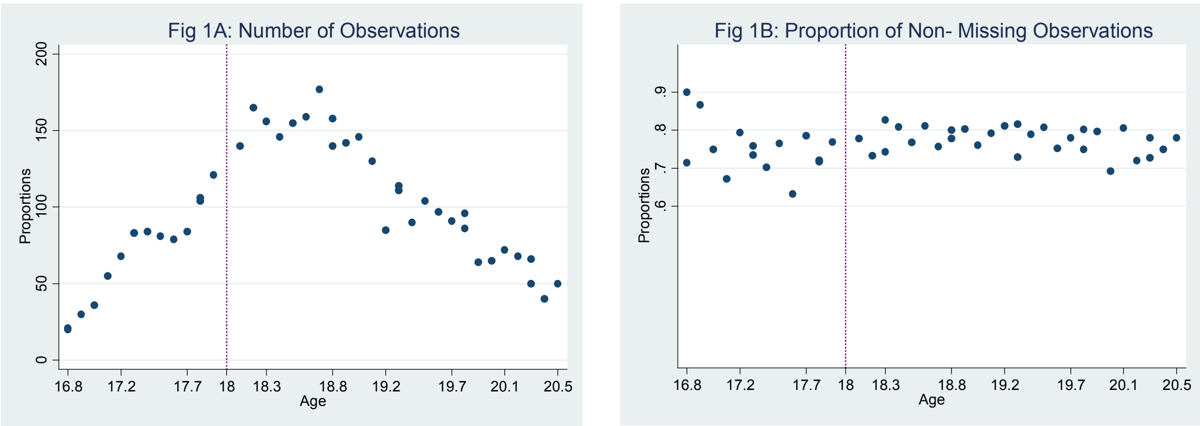

Figure 1: Number of observations and non-missing observations

Notes: Dots in Figure 1A indicate the number of respondents with a distance from the cutoff within one month. Dots in Figure 1B indicate the ration between the total number of non-valid answers and the total number of respondents from the cutoff within one month.

Before presenting the results, we briefly comment on the regression specification. In this section, we report regression results using first- and second-order polynomials. To determine the choice of polynomial order, we run regressions including a set of bin dummies and conduct an F-test to their joint significance. For most of the outcome variables, only first-order polynomials are relevant in explaining outcomes and specifications, and using higher-order polynomials represents an overfit of the data[26]. In addition, we conduct a cross-validation procedure (IMBENS; LEMIEUX, 2008) to identify the optimal window width, which turned out to be 15 months for most outcome variables[27].

Turning to the results, we first perform a simple graphical analysis to check for discontinuities at the 18-year threshold. Figure 2 plots turnout (as a share of population) and the proportion

[25] We conduct this exercise for other outcomes in which voters and non-voters present different behaviour. The regressions do not detect any jump around the 18-year old threshold, for any variable. These results are not reported for the sake of space, but are available under request.
[26] We repeat this procedure for regressions using higher-order polynomials (third and fourth) and find similar results.
[27] We conducted a regression considering smaller windows of width, 9 and 6 months, and find the same qualitative results with similar coefficients' size.

of those who perceive to face the voting requirement by age on Election Day. Dots indicate the average values in a one-month interval, and we include a predicted line based on a second-order polynomial flexible on each side of the cutoff for ease of visualization. The vertical line indicates the 18-year threshold. While turnout raises progressively with age for individuals younger than 18, this pattern disappears after the exposure to the compulsory voting legislation; there is a clear spike in these variables among those at the age of 18. Figure 2 suggests awareness about the legislation, and the effect on the compulsory legislation in increasing voting turnout. The magnitude of the jump suggests that the legal obligation to vote affects the majority of non-voters.

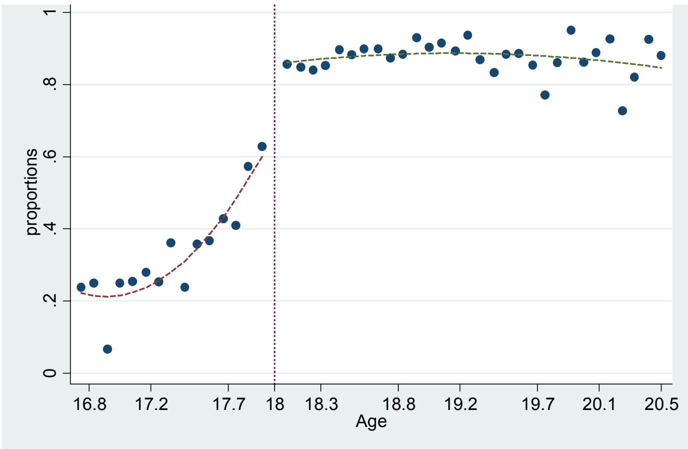

Figure 2: Turnout

Notes: Dots indicate average turnout in a one month interval. The curve is predicted from a second order polynomial flexible on each side of the 18- year threshold.

The regression results are consistent with Figure 2 (Table 4, Column 1). The estimates for the effect of compulsory voting on turnout vary between 27.2 and 18.6 p.p., depending on the specification. These numbers are higher than previous estimates[28] and represent an increase of at least 50% of the voting population. Since this effect is estimated in a country where most of the adult population votes, it is possibly only a lower bound number for the effect. Those under a voluntary voting system are potentially exposed to some social pressure from the remaining population[29].

[28] Using aggregate data in cross-country comparisons, Jackman (1987) and Power and Timmons Roberts (1995) estimate the magnitude of this effect to be between 10 and 15 p.p. Hirczy (1994) finds that the turnout in Carinthia, Austria, increases by 3 p.p. in comparison to other Austrian provinces after the adoption of compulsory voting. He also finds that the abolition of compulsory voting in the Netherlands in 1970 caused a drop of 10 p.p. in turnout.

[29]

We proceed by looking for changes around the 18-year old threshold for measures of ideology and political information as a possible consequence of this increase in turnout. Table 4 presents the results for two indicators of political information measured by participants' performance in the political quiz. When considering the overall performance in the test (Columns (3)-(4)) and Figure 3, the results do not reveal any discontinuity around the 18-year threshold. The estimated coefficients for any of the tested specifications are not statistically significant, and their size is practically equal to zero. Additionally, we report the results for the probability of being able to distinguish the most right-wing party among two alternatives (Columns 5-6, and Figure 4) and find only weak evidence of an increase in knowledge about the political spectrum at the 18-year threshold.

Table 4: Effects of the Compulsory Voting Legislation on Turnout, Political Information and Ideology RD Results

Coefficient on Turning 18 (Required to Vote)										
Outcomes:	**Voting Turnout**		**Political Quiz**		**Distinguish most right wing party**		**Prefers a Political Party**		**Ideologically Extreme**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Mean Outcome - VS population	0.3728		0.4975		0.553		0.2937		0.049	
Linear regression on both sides of discontinuity	0.2665	0.2719	-0.0002	0.00741	0.0496	0.0576	0.0479	0.0554	0.0276	0.0232
	[0.0315]*	[0.0310]**	[0.0129]	[0.0128]	[0.0326]	[0.0317]*	[0.0342]	[0.0333]*	[0.0144]*	[0.0144]
Second order polynomial on both sides of the discontinuity	0.1864	0.1965	0.0009	0.0029	0.0294	0.0374	0.0864	0.0895	0.0355	0.0297
	[0.0427]**	[0.0421]**	[0.0177]	[0.0176]	[0.0452]	[0.0438]	[0.0475]*	[0.0464]**	[0.0195]*	[0.0195]
Joint significance of polynomial dummies (p-value of F-test)	0.7938	0.6529	0.3921	0.4405	0.2002	0.1253	0.1523	0.1498	0.0462	0.0482
N	3053	3242	3059	3251	3059	3251	3037	3227	2978	3163
Demographics controls	yes	no	yes	no	yes	no	yes	no	yes	no

Notes: 1) Same from Table 3. 2) All regressions include school-fixed effects and an indicator for whether an individual has voted before. Demographic controls include dummies for gender, race and mothers' education.2) *Significant at the 10 percent level, **Significant at the 5 percent level.

Figures 5 and 6 and Columns (7)-(10) in Table 4 present the results for ideological outcomes, namely preference for a political party and identification as extreme oriented. The coefficients related to the 18-year threshold are positive and statistically significant. The magnitude is relevant; it indicates that the exposure to compulsory voting leads to an increase in individuals' propensity to self-identify as extreme oriented in 3.5 p.p. (or by an increase of 72% on the mean of the population exposed to the voluntary voting) and to an increase of approximately 5 p.p. in the probability of having a party preference (or to an increase of 18% on mean of the VV population).

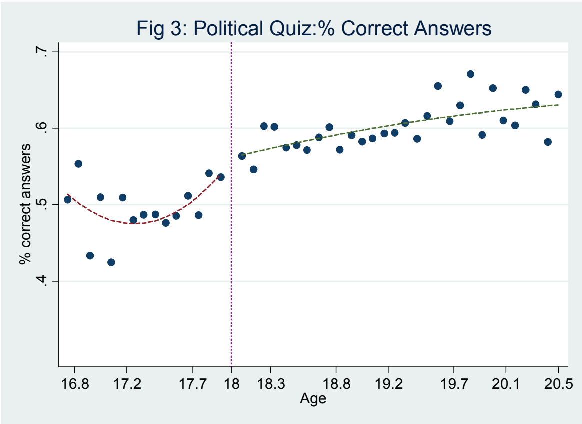

Figure 3 Political Quiz Score

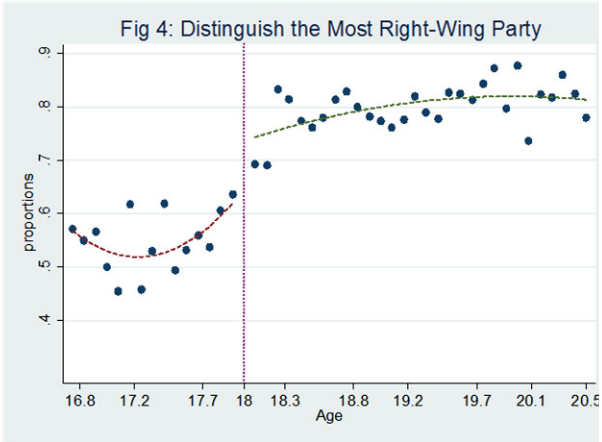

Figure 4: Knowledge of Political Spectrum

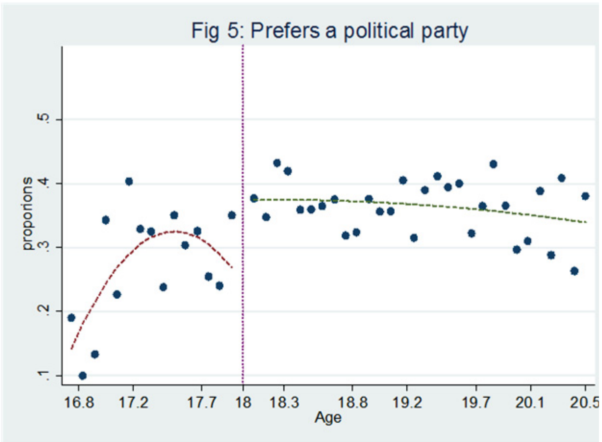

Figure 5: Preference for Political Party

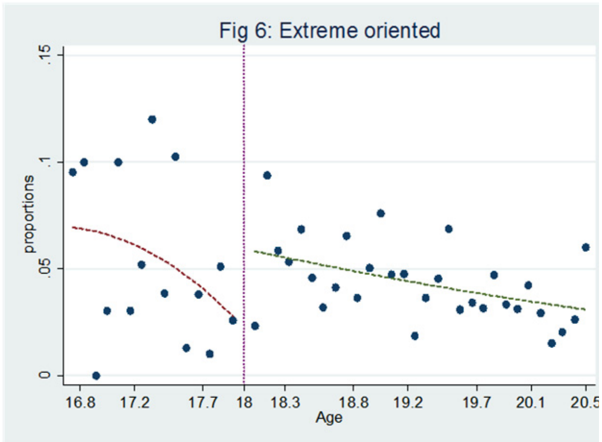

Figure 6: Polarization

It is important to note that these RD regressions only identify the average impacts for the population. Based on these results, it is unclear which segment of the population is reacting to the compulsory voting legislation or if non-voters are encouraged to engage in politics once they are forced to vote. We test the later hypothesis in the next section, with an instrumental variable approach.

1.3.1.2 Instrumental Variable Analysis

In this section, we perform IV regressions to estimate the effect of voting on information acquisition and political preferences using exposure to the compulsory voting system as an instrument for turnout. This variable is highly correlated with turnout but conditional on age;

variation on the exposure to the compulsory system should not, by itself, increase political engagement.

In the presence of heterogeneous effects, this method estimates the average treatment effect for individuals who change their treatment status (i.e., become voters), because they react to the instrument (IMBENS; ANGRIST, 1994; OREOPOULOS, 2006). We estimate the following equations:

First Stage: $VoteTurnout_i = \eta_0 + \gamma_1 1(S_\alpha > 0) + H(age) + \varpi_i$

Second Stage: $y_i = \nu_0 + \gamma_2 VoteTurnout_i + \beta X_i + H(age) + \varepsilon_i$

Table 5 reports the IV results for the effect of compulsory voting and citizens' political behavior for subgroups according to their mothers' education level and uses alternative age polynomials, H(age), as controls. Consistent with the results from Table 4, there is some evidence that individuals become more polarized in response to compulsory voting (Columns 1 to 6). We find heterogeneous effects according to the mothers' level of education. Among the group whose mothers had some college education, once forced to vote, individuals become 35.5% more likely to declare to have a preference for a political party and become 15.4% more likely to self-declare ideological extreme (Columns 2 and 5). For the group whose mothers do not have any college education, the regression results do not detect any change in ideology (Columns 3 and 6).

Table 5: IV Results for Compulsory Voting on Ideology and Information

Outcomes:	**Prefers a Political Party**			**Extreme (Strongly Lelf- or Right-wing)**		
Samples:	All	Mother with some college	Mother with no college	All	Mother with some college	Mother with no college
	(1)	(2)	(3)	(4)	(5)	(6)
Year of birth fixed-effect, quarter of birth and quarter of birth squared	0.2572 [0.1450]*	0.4649 [0.1872]**	0.0000 [0.2056]	0.1484 [0.0660]**	0.1878 [0.0908]**	0.0390 [0.0890]
Linear regression on both sides of discontinuity	0.1852 [0.1269]	0.3554 [0.1698]**	0.0054 [0.1716]	0.1054 [0.0565]*	0.1548 [0.0807]*	0.0084 [0.0715]
N	3024	2025	999	2964	2003	961
Outcomes:	**iz (% correct answers)**			**Distinguish most right wing party**		
Samples:	All	Mother with some college	Mother with no college	All	Mother with some college	Mother with no college
	(7)	(8)	(9)	(10)	(11)	(12)
Year of birth fixed-effect, quarter of birth and quarter of birth squared	0.0020 [0.0552]	0.0072 [0.0718]	0.0422 [0.0795]	0.2287 [0.1392]*	0.0898 [0.1737]	0.3889 [0.2092]*
Linear regression on both sides of discontinuity	-0.0018 [0.0483]	0.005 [0.0670]	0.027 [0.0659]	0.1839 [0.1231]	0.1314 [0.1635]	0.2659 [0.1752]
N	3045	2034	1011	3045	2034	1011

Note: Same from note 2 in Table 4.

In terms of political information, neither group, once forced to vote, shows improvement in the performance on the political quiz. We find some weak evidence of an increase in knowledge of the political spectrum in response to compulsory voting. As shown in Column 11, individuals whose mothers do not have any college education become approximately 30% more likely to be able to distinguish the most right-wing party, once forced to vote. This effect is only statistically significant at the 13% level, in regressions using a first-order polynomial of the forcing variable as a control for age.

A natural question is why, once forced to vote, individuals that have more educated mothers become more polarized and take a stand in choosing a political party, while others from less-educated backgrounds do not. Do they exert more effort to acquire information? In Table 6, we present the results from IV regressions in which the related dependent variables are several mechanisms of information acquisition, including discussion with parents and friends and media consumption. Results for the sample with college-educated mothers are reported in Column (2). None of the related coefficients are statistically significant, indicating that none of these variables explains the change in ideology. On the other hand, among those whose mothers do not have any college education (Column 3), the regression results indicate that, once forced to vote, they increase the consumption of political information by 1.6 days per week on the Internet. That may partly explain how they enhance their comprehension about the political spectrum. Some possible alternative explanations for why individuals from more educated families react by changing their ideological position rely on the fact that they are more knowledgeable about and involved in politics. Based on the results not shown in the paper, they follow politics more often in the media and demonstrate better performance on the political quiz. These are contributing factors for individuals as they make a decision. Alternatively, parents can be information shortcuts, providing simple advice, which, once individuals must vote, are used to decide on a position.

Table 6: IV Results for Effects of Compulsory Voting - Mechanisms of Information Acquisition

Samples:	All	Mother with some college	Mother without any college education
Watched 2010 TV political advertisement	0.0019	0.0857	-0.0159
	[0.1065]	[0.1274]	[0.0958]
	3045	*2034*	*1011*
Talk to parents frequently about politics	-0.1796	-0.1974	-0.0871
	[0.1391]	[0.1994]	[0.1756]
	3032	*2028*	*1004*
Talk to friends frequently about politics	-0.0714	-0.1069	0.0111
	[0.1370]	[0.1926]	[0.1806]
	3044	*2033*	*1011*
Number of days follows politics per week			
TV news	-0.5505	0.04901	-1.168
	[0.5763]	[0.8163]	[0.7560]
	3016	*2011*	*1005*
Newspapers/Magazines	-0.2702	-0.5981	0.526
	[0.5409]	[0.7798]	[0.6926]
	2998	*2013*	*985*
Internet	0.7775	0.3978	1.603
	[0.6215]	[0.8691]	[0.806]**
	2993	*2016*	*977*

Notes: 1) Standard errors are in brackets. All regressions include a first-order age polynomial, school-fixed effects, an indicator for whether an individual has voted before and indicators for gender, race and mother's education. 2) Numbers of observations for each regression are in italics 3) *Significant at the 10 percent level, **Significant at the 5 percent level.

The effects estimated in this section reflect the immediate impact of the exposure to compulsory voting. The polarization effect---measured by individuals' decision to align with a political party---may or may not accentuate or dissipate with more voting experience over the individual's life cycle. We address this issue in the next section.

1.3.2 Frequency and Permanent Effects of Voting on Political Preferences

In order to estimate the frequency effects of compulsory voting and test whether they are persistent, we exploit the variation in the number of compulsory election seasons experienced by individuals. We consider a broader age sample of survey respondents and make cross-age comparisons to identify voting effects. This is a valid exercise, since the whole population has been exposed to the compulsory voting intervention at the age of eighteen. To conduct this exercise, we restricted the sample in two ways. First, we excluded those born before 1988, as they constitute only 7.5% of the sample, and due to the low number of observations, the results regarding the effects of further voting could be inconclusive. In addition, due to the fact that the data come from a survey conducted in classrooms and not from the general population, age can convey information on other possible relevant individuals' unobservable characteristics. For these reasons, we excluded the oldest 15% and the youngest 15% in every school among non-college students and in every major among college students[30].

We start by investigating the marginal effects of each additional compulsory election faced by individuals. In this analysis, the main confounding factor is age. To circumvent this problem, we conduct regressions controlling for year-of-birth fixed effects (θ_a), quarter of birth, and quarter of birth squared (θ_q), estimating (2). As a result, the effects are identified within the year-of-birth variation in individuals' quarter of birth[31]. For this specification, only frequency effects of voting are identified, as the coefficients capture the effect, in the margin, of having faced an additional election.

$$y_i = \delta_0 + \delta_1 OneElection + \delta_2 TwoElections + \delta_3 ThreeElections + \theta_a + \theta_q + \beta X_i + \varpi_i \qquad (2)$$

The key independent variables are dummies indicating whether the respondent has been exposed to one (OneElection), two (TwoElections) or three (ThreeElections) compulsory elections. Columns (1) to (3) in Table 7A present the results, respectively, for the samples, including respondents born no earlier than 1988 (exposed to up to three compulsory elections), 1990 (exposed to up to two compulsory elections) and 1992 (exposed to up to one compulsory election).

[30] The results are robust to the exclusion of longer tail of age distribution, of 20%.

[31] Recently, Buckles and Hungerman (2010) have shown that, for the US case, differences in quarter and month of birth convey differences in children's socio-economic backgrounds. If this relationship holds for the Brazilian data, it can be a potential confounder to identify the effects of voting. We test this hypothesis in our dataset, and the tests do not detect individuals' demographic differences based on quarter or month of birth.

The results in Table 7A, Column 3, show that individuals exposed to one compulsory election are 17.2% more likely to declare a preference for a party than those who never faced a compulsory election. While the size of coefficients does not decrease with the number of experienced compulsory elections, we cannot infer increasing effects of voting. For all relevant regressions (Columns 1-2 in Table 7A) and specifically for the regression specification shown in Column 1, a Wald test does not reject the hypothesis that the size of the coefficient related to the first compulsory election is statistically different from the second compulsory election (p-value=92%) or the third one (p-value=48%).

Table 7: Frequency and permanent effects of compulsory voting

Table 7A - Effects of Compulsory Voting on Preference for a Party - Frequency Effects

	(1)	(2)	(3)
Number of Experienced Compulsory Elections			
Zero (omitted)			
One (α1)	0.1481	0.1535	0.1717
	[0.0605]**	[0.0600]**	[0.0608]**
Two (α2)	0.1713	0.1691	
	[0.0887]**	[0.0896]*	
Three (α3)	0.2881		
	[0.2366]		
Year of Birth- fixed effects	yes	yes	yes
Quarter of Birth and Quarter of Birth squared	yes	yes	yes
R2	0.0113	0.109	0.0181

Table 7B - Effects of Compulsory Voting on Preference for a Party - Permanent Effects

	(1)	(2)	(3)
Number of Experienced Compulsory Elections			
Zero (omitted)			
At least one (α1)	0.1542	0.1536	0.1703
	[0.0528]**	[0.0530]**	[0.0566]**
At least two (α2)	-0.0291	-0.0217	
	[0.0364]	[0.0392]	
At least three (α3)	0.0657		
	[0.0162]		
At least four (α4)			
P-value(F-stat: αi=0, i?1)	0.6115		
Year of Birth and Year of Birth Squared	yes	yes	yes
Quarter of Birth and Quarter of Birth squared	yes	yes	yes
Sample by year of birth' range:			
1994-1988	x		
1994-1990		x	
1994-1992			x
R2	0.011	0.0108	0.0169
Number of Observations	3,337	3,266	1,555

Notes: 1) Standard errors robust to heteroskedasticity are in brackets. All regressions include school-fixed and major fixed-effects, indicators for gender, race and mothers' education. 2) **Significant at the 5% level.

To determine whether and to which extent the effect of the exposure to compulsory elections is a lasting one, we test whether political preference changes permanently according to the number of experienced compulsory elections. We construct three dummies indicating whether the respondent has been exposed to at least one election, at least two elections or at least three elections. To control for the age effect, instead of including year-of-birth fixed effects controls, we use year of birth and year of birth squared as controls in these specifications. The results are reported in Table 7B. Column 1 shows that, after being exposed to at least one compulsory election, individuals become 15.4% more likely to declare a preference for a political party than those who have never experienced a compulsory election, for at least 3 elections cycles (or 6 years). This result is robust to different sample sizes. As shown by the standard errors of coefficients alfa1 and alfa2 in Columns 2 and 3, we cannot infer that the exposure to more compulsory elections (than the first one) changes individuals' probability to express preference for a political party.

These results are somewhat in line with those of previous studies, based on the notion that voting may change citizens' attitudes, such as their level of partisanship (FINKEL, 1985). For example, Meredith (2009) finds that past eligibility to vote increases citizens' likelihood of registering with a political party in the subsequent elections by 2.2% when compared to first-time-eligible voters. His findings are consistent with increasing effects of voting, which can be an argument in favor of the compulsory voting system. In contrast, the results presented in Table 7B, Columns 1-3, suggest that exposure to the first compulsory election is the relevant event to change people's preferences. An F-test does not reject the hypothesis that the coefficients associated with exposure to more than one compulsory election are jointly zero at the five-percent level (Column 1).

1.3.3 Discussion

This paper presents new evidence of the effect of forced voting on political engagement---measured by an expressed preference for a political party. This is observed in the general population in an environment in which a comparison is made between potential voting populations exposed to voluntary or compulsory voting systems. Our results, to some small extent, confirm the conjecture made by Lijphart (1997, p. 10) about one of the benefits of compulsory voting regarding its potential "to serve as an equivalent form of civic education and political stimulation."

The magnitude of this effect is relevant. On average, non-voters become as likely to take a political position by expressing a preference for political party as (voluntary) voters. This is illustrated in Figure 7.

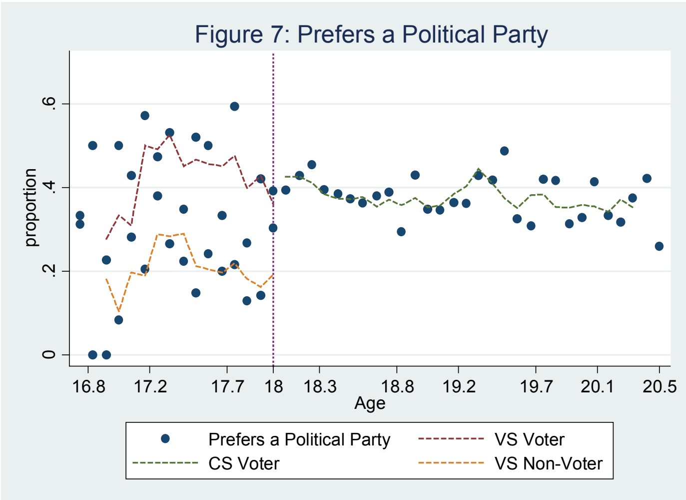

Figure 7: Political Position by voters and non-voters
Notes: Dots indicate average turnout in a one-month interval. The curve is predicted from a 3-month moving average calculated separately for the each of the three categories (VS voters, VS non-voters and CV voters).

We also find that this effect depends on family background. Only those whose mother has some college education react in changing their preferences. This indicates that the requirement to vote is not, by itself, a sufficient mechanism of political stimulation. Access to political information and knowledge are important, in conjunction to the obligation to vote..

As it is widely documented in the U.S. (DEGAN; MERLO, 2011), our data also shows that (voluntary) voters are more informed than non-voters (Table 2). We do not find strong evidence of an effect of compulsory voting on political knowledge. Most of our results are in line with the classical view of rational ignorance, in that the obligation to vote, by itself, would not lead people to engage in the costly process of acquiring information. On one hand, this gives support to the common view about possible negative consequences related to the compulsory voting legislation in terms of increasing in the polls the share of uninformed voters.[32] Addressing this issue is beyond the scope of this paper[33]. On the other hand, we also

[32] "High Turnout Would Be a Disaster", Jason Brennan for The New York Times (7/11/2011).
[33] Leon (2012) studies the relationship between voting systems and election outcomes.

find some weak evidence of self-educational effects related to the obligation to vote among individuals from low economic background (whose mothers do not have any college education). This suggests that policies that encourage voting might be a way to decrease political apathy among the poor.

1.4 Conclusion

Voting lies at the heart of democracy. This study investigates the effects of compulsory voting on people's knowledge and political preferences. It circumvents the endogeneity problem and identifies the effects in question by exploring Brazil's dual voting system, which provides an exogenous shift in individuals' likelihood to vote. This paper presents a set of results, new and relevant to public policy, in terms of understanding some of the consequences of the adoption of compulsory voting legislation. It also complements the current understanding of the effects of voting on individuals' citizenship.

We find large and significant effects of the legal requirement to vote on turnout (between 16 p.p. and 28 p.p.) These lead to some positive consequences in terms of increasing political involvement among the population, specifically the group of (voluntary) non-voters. We find only weak evidence that individuals react to the obligation to vote by acquiring information, and that holds only for individuals from lower economic backgrounds. On the other hand, our results are consistent with the fact that citizens give more thought to elections and politics when obliged to vote (as measured by a declared preference for a political party). This positive effect seems to be permanent (lasting at least 6 years) and related to the first voting experience. In this sense, these results put in question the need for adopting a compulsory system in order to foster political involvement among the population.

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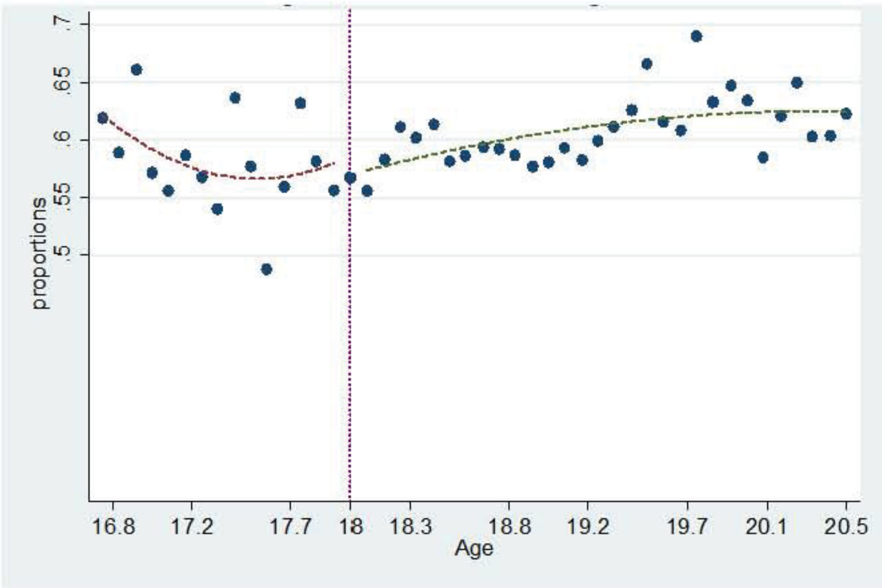
APPENDIX

Table A: Summary Statistics

Samples:	Total	Public High Schools	Anglo	USP
Outcomes and Characteristics (in %):				
Information				
Number of days a week learns about politics from:				
TV news	3.48	4.10	3.37	3.41
Newspapers and magazines	2.46	2.05	2.46	2.72
Internet	3.45	3.01	3.16	4.65
Watched 2010 TV political advertisement	88.15	92.99	87.63	86.73
Talk to parents frequently about politics	43.14	36.02	46.67	38.10
Talk to friends frequently about politics	46.21	46.24	46.19	47.38
Political Quiz (% correct answers)	58.38	44.31	58.29	67.71
Able to distinguish which party is right oriented among two extreme choices	85.37	56.87	87.15	94.13
Political Inclination				
Has a political party preference	35.63	26.99	36.16	39.41
Extreme Left-wing	3.42	2.70	2.68	6.32
Moderately Left-wing	26.43	14.39	23.87	42.25
Center	48.65	75.11	47.39	36.66
Moderately Right-wing	24.92	10.49	28.73	21.08
Extreme Right-wing	1.65	1.80	1.85	0.92
Voted in the 2010 Election	77.07	39.72	80.78	89.23
Age				
16 or younger	1.56	7.97	0.72	0.18
17	18.28	75.94	11.48	3.25
18	34.51	14.27	41.55	24.62
19-20	31.9	1.68	35.94	38.24
21-22	6.93	0.14	6.74	11.9
23 or older	6.82	0.02	3.57	21.81
Female	57.09	60.08	57.85	49.20
White	76.33	58.19	79.54	78.20
Mother education : college or more	67.62	23.49	77.21	61.88
Required to Vote	80.00	16.03	87.75	96.49
Voted Before the 2010 Election	33.92	2.37	31.87	61.07
Responded seriously to the survey (%)	93.70	87.46	94.38	95.53
% of total sample		13.09	66.59	20.32
Number of Observations	5,561	728	3,703	1,130

Quiz Score among Voters



Political Quiz

1. Cite a political position held by Dilma Rousseff before running for President in the 2010 Election.
2. Cite a political position held by Marina Silva before running for President in the 2010 Election.
3. Cite a political position held by Jose Serra before running for President in the 2010 Election.
4. What is the political party affiliation of Dilma Rousseff?
5. What is the political party affiliation of Dilma Rousseff's running mate?
6. What is the political party affiliation of Marina Silva?
7. What is the political party affiliation of Marina Silva's running mate?
8. What is the political party affiliation of Jose Serra?
9. What is the political party affiliation of Jose Serra's running mate?
10. Which candidate was partly responsible for the introduction of generic drugs?
    ( ) I do not know
    ( ) Dilma Rousseff
    ( ) Marina Silva
    ( ) Jose Serra
    ( ) Other/None
11. Which candidate was partly responsible for the implementation of the PAC[34]?
    ( ) I do not know
    ( ) Dilma Rousseff
    ( ) Marina Silva
    ( ) Jose Serra
    ( ) Other/None
12. Which candidate was partly responsible for the increase in the basic interest rate (SELIC)?
    ( ) I do not know
    ( ) Dilma Rousseff
    ( ) Marina Silva
    ( ) Jose Serra
    ( ) Other/None
13. Which candidate was partly responsible for the creation of protected areas in the Amazon region?
    ( ) I do not know
    ( ) Dilma Rousseff
    ( ) Marina Silva
    ( ) Jose Serra
    ( ) Other/None
14. Which of these parties is more right-wing oriented?
    ( ) DEM ( ) PSOL

[34] Programa de Aceleração do Crescimento (PAC) refers to the growth acceleration program.

2 NOT ALL DEFAULTS ARE CREATED EQUAL: AN EMPIRICAL INVESTIGATION OF OCCURRENCES IN "GOOD & BAD TIMES"[35]

2.1 Introduction

Sovereign debt defaults have been an integral part of the economic landscape for many centuries (REINHART; ROGOFF, 2009). Over the past 200 years for which data has been compiled in a more consistent fashion, these occurrences have been widespread and their costs substantial, both in terms of economic and social hardships[36]. But despite 30 years of intense, creative theoretical research on the subject, since Eaton and Gersovitz (1981) first published their seminal paper on debt repudiation, several of the salient features in the data remain unaccounted for by the profession's most widely used models.

Tomz and Wright (2007), using a proprietary dataset covering 179 countries and spanning 185 years, from 1820 to 2004, show that only about 62% of all sovereign defaults occur in so-called bad times, as measured by a Hodrick-Prescott filter[37]. In other words, over 38% of defaults happen in good times and, therefore, as shall be seen below, cannot be satisfactorily explained by the dominant strands of the theoretical literature in international economics. But there is more to this story than meets the eye. Here, we propose to further explore these occurrences, highlighting characteristics that should be taken into account by any researcher looking for models that explain these rather curious facts.

First, however, it is important to understand that the most common view of defaults states that these events should be examined through the lenses of economic self-insurance. This class of models counts among its most recent developments the sovereign default framework proposed by Aguiar and Gopinath (2007)[38], which, following Tomz and Wright (2007), we

[35] With João Moreira Salles.
[36] See Hatchondo, Martinez and Sapriza (2007), Tomz and Wright (2007), among others.
[37] The HP filter defines the trend growth rate of output and allows the authors to compare current output with the one implied by the trend. If output is above trend, we have a good time. When output is below trend, it is called a bad time.
[38] See Arellano (2008) and Yue (2010) for other contributions in a similar vein that share many characteristics with the paper by Aguiar and Gopinath.

will use as a reference point here. This model assumes that defaults provide costly insurance against negative shocks and rationalizes the decision to renege on the debt as a pure cost-benefit analysis[39]. This mechanism, however, builds-in a significant negative correlation between output and defaults, clearly implicating that *almost* all defaults should take place when the gross domestic product (GDP) is below potential.

The *almost* qualifier used in the previous sentence comes – as will be made clear by the model simulation reproduced in Table 8 below – from the fact that Aguiar and Gopinath look not only at transitory shocks to the economy, but also at permanent disturbances that alter the trend of its expansion, thus establishing that some defaults could take place as the country converges from above to a new, lower growth standard. In any case, both types of shocks imply that defaults occur, on average, with output significantly below trend – and more so in the case of transitory shocks.

The model suggests that, even after accounting for permanent shocks, roughly 86% of all defaults should take place with output below potential. Only the remaining 14% of repudiation episodes could, therefore, be potentially characterized as strategic or inexcusable, in the sense that the country is not forced into this situation by dire circumstances which trigger the implicit – and imperfect – contingency clauses of sovereign debt[40]. In these repudiations, the sovereign acts in a manner that goes against the reputation-building aspect of recurrent interactions with (international) credit markets (see Grossman and Van Huyck, 1988). That is one reason why such incidents are hard to justify in equilibrium.

Alas, as mentioned above, these predictions are in stark contrast to what is established by the data. Tomz and Wright (2007) also show that defaults occur, on average, when output is just marginally below trend (-1.6%). What is more, many countries suffering major drops in output decide not to renege on the debt despite the fact that, at that point, the model suggests

[39] There is a 2% direct output cost of default, introduced in a *ad-hoc* fashion, and calibrated using the values put forth by Chahuan and Sturzenegger (2005).

[40] This interpretation is, obviously, not strictly consistent with Aguiar and Gopinath's analysis, or our own views, as the occurrence of a permanent negative shock to growth could be seen as a dire enough circumstance to justify the use of a debt default as an insurance device. Nevertheless, this type of shock introduces enough uncertainty as to the country's situation relative to the new trend that defaults on those instances could be seen by some as strategic, especially in purely empirical analysis.

that defaulting would be the reasonable choice. Given the common intuition about these episodes, this is almost as surprising as the authors' finding that over 38% of all debt repudiations are unjustifiable (inexcusable). All these empirical observations fall considerably outside our current understanding of the issues involved.

But these observations lead to a fundamental question: are defaults in good and bad times really the same thing or do they have distinct determining factors? For if they are different events altogether, the quest for a unique, all-encompassing model of sovereign defaults may not necessarily be the correct research agenda. One objective of the present work is to approach this question in a systematic way, applying modern micro-econometric tools to derive interesting implications from the data that may inform future theoretical research on the subject.

Our investigation is driven by the view that defaults have important political – as well as other environmental – components besides the economic one (besides the fact that different definitions of good and bad times can have a significant impact on the interpretation of the data[41]). Several researchers have previously discussed the relevance of the political environment in determining the nature of the interaction between the sovereign and its creditors. Hatchondo et al (2007), for instance, offer a nice review of the political factors that may, potentially, drive a country towards default. And a number of works, such as Citron and Nickelsburg (1987) and Kohlscheen (2003), among others[42], point to the empirical relevance of these hypotheses. Our contribution is to further refine this analysis by taking into account the existence of good and bad times and testing whether political and other economic factors are more prevalent in one type of default rather than the other.

Before looking at the implications of our hypotheses, though, we first establish that the findings of Tomz and Wright's (2007) are not a feature of a specific proprietary dataset. Using only publicly available data, we construct, in section 2.2, a significantly shorter panel – focusing on modern defaults, after the 1970's, which seem more relevant for understanding today's environment – and still obtain roughly the same results as them, despite the

[41] See Appendix for a discussion of this point.
[42] See, for example, Van Rijckeghem and Weder (2004), Enderlein, Müller and Trebesch (2008), Sturzenegger and Zettelmeyer (2006).

differences in the group of countries and in the time span covered in our panel. This shows that, at least as a first pass, the Tomz and Wright critique of the theoretical literature seems robust to alternative selections of data[43].

Subsequently, we present the results of our econometric estimates of the determinants of sovereign defaults, which seem to support the basic premise that political and economic environmental factors are more prevalent in determining inexcusable defaults. Finally, we perform some basic duration analysis to understand if and how the length of default episodes depends on our covariates. We find that the duration of defaults is significantly impacted by the prevailing international economic conditions when it starts, but not so much affected by the type of default in question. We conclude, in section 2.5, that defaults are, indeed, very diverse events depending in which point of the business cycle they start, and suggest further avenues for empirical research on this topic.

2.2 Exploring the Data

2.2.1 Dataset

Our dataset spans from 1970 to 2004 and includes 71 economies, forming a panel with 2,485 data points, for which all the descriptive statistics can be found in the Appendix. The source for economic and financial indicators is the World Bank. (World Development Indicators and Global Development Finance databases). We use the Standard and Poor's definition of sovereign default and focus on national government defaults on foreign currency loans and bonds to private creditors. We draw on Beers & Chambers (2004) to construct two series of dummy variables for defaults. The first of these deals with the beginning of a default episode, and hence only takes the value 1 on the year a given country reneges on its debt. The second dummy variable measures the duration of any given default episode, taking the value 1 for the entire period the country remains in default. We start with a dataset of 210 countries but reduce it to 71 countries by including only those that report positive amounts of public and

[43] In the Appendix, we review a few definitions of good and bad times, and illustrate how many of the previously mentioned findings depend crucially on how these periods are classified.

publicly guaranteed debt to the World Bank and for which default data is available from S&P's. The scope of this paper is to differentiate between good and bad time defaults, not to understand what determines a country's propensity to be a defaulter.

Having established the relevant economic and financial data to be used, we then focus on the political environment, which is of major importance for the underlying hypotheses we intend to test. The Polity 2 Index, from the Polity IV Project[44], is employed as a measure of political regime characteristics and as an indicator of regime transitions (political shocks). It is commonly used in the political economy literature as well as in political science research for this purpose and, thus, fits are goals quite well. It is opportune to note that the Polity score is a measure of regime type that reflects the degree of political competition, the qualities of executive recruitment and institutional constraints on the executive authority. There are no economic variables among its components.

2.2.2 Probability of Default

In this subsection, we present some interesting results that can be drawn from the simple exercise of examining the data in this fully publicly available dataset. First, we replicate TW's analysis and generate numbers that are not very different from what they report (Table 8). The puzzle remains there: almost 40% of defaults begin in good times[45].

[44] www.systemicpeace.org/polity/polity4.htm

[45] Although Tomz and Wright (2007) classified good and bad times by applying the Hodrick-Prescott filter to real gross domestic product, they did recognize that alternative definitions of bad times could produce a stronger negative relationship between default and output in the data. Our own experiments indicate that simple changes in the methodology used for classification do, in fact, yield quite different results. We include a brief discussion on the HP filter and potential alternatives in the Appendix.

Table 8: The relationship between default and output

	Historical Data		Simulation	
	Tomz & Wright Data	Rizzi & Salles Data	Transitory Shocks	Permanent Shocks
Mean Deviation from trend (%)				
In the first year of default	-1.6	-1.1	-4.6	-7.4
In periods of default	-1.4	-2.1	-25.7	-5.6
In last year of defaults	-1.3	0.4	-13.6	-4.5
In periods of non-default	0.2	0.8	0.0	0.4
Country years below trend (%)				
In the first year of default	61.5	61.7	100.0	85.9
In periods of default	56.2	63.6	83.8	78.1
In last year of defaults	58.8	50.5	75.5	72.9
In periods of non-default	47.2	45.5	50.0	48.3

Note: Simulations and proprietary dataset presented by Tomz and Wright (2007). Rizzi and Salles statistics come from the dataset constructed in the present paper using only publicly available sources.

Second, we examine mean values of selected variables in years of non-default and in years when defaults start and get quite interesting "hints" (Table 9). Although defaults occur with output only marginally below trend, we find that GDP in the first year of default is, on average, 7.5% below its historical peak. Also, before defaults start, U.S. interest rates experience an average 2-year growth of 5.9%, as opposed to a 0.4% drop in years of non-default.

Table 9: What do simple means tell us?

	First Year of Default	Year of Non-default	Total
GDP HP deviation from trend	-1.1%	0.8%	-0.3%
Current GDP/historical GDP peak	-7.5%	-3.0%	-5.1%
Two-year U.S. interest rate growth	5.9%	-0.4%	-1.2%
Dummy for bad HP time	0.62	0.46	0.52
Dummy for GDP below historical peak	0.66	0.28	0.38
Polity2 Index	-1.32	-0.97	-0.85

Finally, we differentiate between default events that start in bad and in good HP times and redo the analysis. Good-time-defaults seem to be explained partly by increases in international interest rates and partly by political factors (Table 10). We consider three "political" variables in particular: a dummy to indicate whether a country is an autocracy or not[46], a dummy to

[46] According to the classification in Polity IV project.

signal that a change in political regime took place within the two previous years[47] and a dummy that takes value 1 in years for which GDP in constant terms is below the country's historical peak. In the first year of good-time defaults, 53.3% of country-years correspond to autocracies, 55.6% take place below historical GDP peak and 26.7% are years of political regime transition. Furthermore, 52.8% of country-years are concomitant with U.S. interest rate hikes[48].

Table 10: Frequency of each dummy in different sub-samples

	Begin of good-time default	Begin of bad-time default	Total sample
Autocracy	53.3%	37.5%	46.6%
Below historical peak	55.6%	72.4%	38.1%
Political regime change	26.7%	16.1%	24.1%
U.S. interest rate hike (2-year growth > 10% in the rate)	52.8%	43.1%	38.1%

Finally, inspired by Levy Yeyati and Panizza (2011), we replicate the exercise using quarterly data as opposed to annual data (Table 11). Our original findings are strongly reinforced by high-frequency data, as the share of good HP times defaults that cannot be explained via political variables drops to as low as 5%[49].

Table 11: Yearly versus quarterly data

% of defaults beginning in	Bad HP Time	Below historical GDP peak
Yearly Data	62%	66%
Quarterly Data	75%	95%

2.2.3 Duration of Default

We also perform some basic duration analysis to understand if the length of the default episodes also varies with the kind of defaults that precede them. Duration (or survival) analysis was originally employed to estimate how the survival time of patients was affected by different factors. By now, the methodology has become widespread in empirical work,

[47] We use the variable "durable", from Polity IV project. This variable provides a running measure of the durability of the regime's authority pattern for a given year, that is, the number of years since the last substantive change in authority characteristics (defined as a 3-point change in the Polity score).
[48] Defined as a two-year-growth of more than 10% in interest of U.S. 10-year treasury bonds.
[49] These results are available upon request.

mainly in labor and population economics applications. In short, it aims at understanding movements from one state to another and at relating the time elapsed until that movement occurs to one or more explanatory variables. The analytical unit in most studies is the individual (sometimes the firm or the country) and examples of explained variables include the duration of unemployment, the lifetime of firms, the duration of marriages, the length of wars and newborn survival time. We include a short description of the duration models used in the Appendix.

In our case, the idea is to complement the findings of Sandleris, Gelos and Sahay (2004), Benjamin and Wright (2009) and some others[50], who find that, besides a renegotiation period of about eight years, countries are, on average, excluded from international capital markets for only four years[51]. We look at the length of defaults (with start and end points as defined by Standard and Poor's) and our goal is to make a distinction (if we find one) between the duration of good and bad times defaults, autocratic and democratic regime defaults, excusable and inexcusable defaults.

Although our dataset does not exhibit the problems normally associated with duration data[52], this kind of modeling is a natural approach to this analysis since the dependent variable is generated by a series of sequential decisions.

We start by looking at the Kaplan-Meier curves estimating the distribution of the survival function in our sample which, in this case, relates to the probability that a given default will last beyond a certain period of time. We study the curves, not only for the overall dataset, but also for some of its sub-samples, taking into account the dominant political regime and the economic situation of the country.

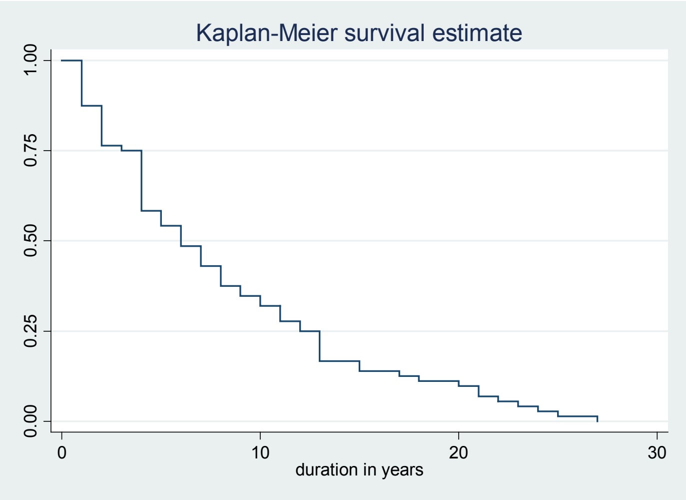
The visual examination of the Kaplan-Meier non-parametric survivor estimates (Figure 8) is quite revealing.

[50] Richmond and Dias (2008), Arráiz (2006), Panizza, Sturzenegger and Zettelmeyer (2009) .

[51] In stark contrast with the permanent exclusion assumption needed for modern calibrated theoretical models to match the data.

[52] Censoring, truncation and left-bias sampling.

Aggregate



Good and Bad HP Times

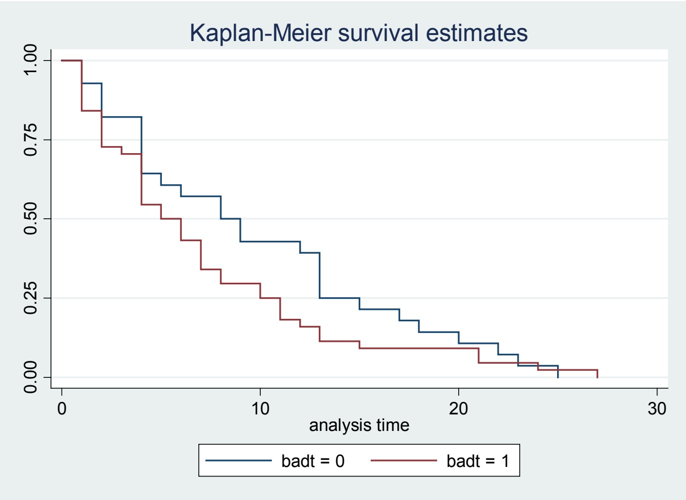
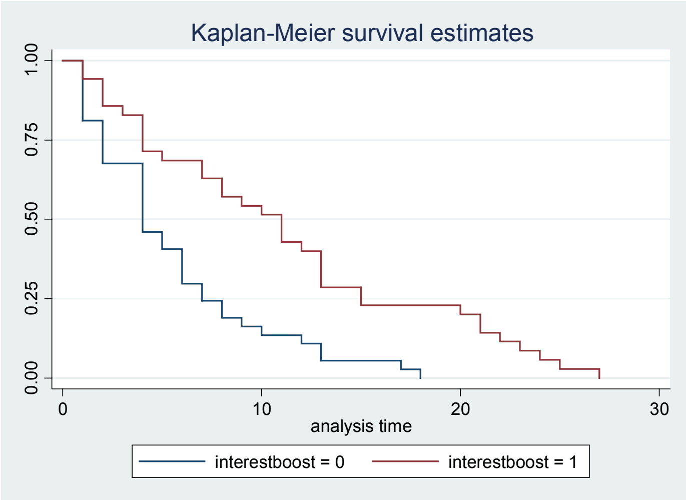


Figure 8: Kaplan Meier Survival Curves

Interest Rate Boost Dummy



Autocracy Dummy

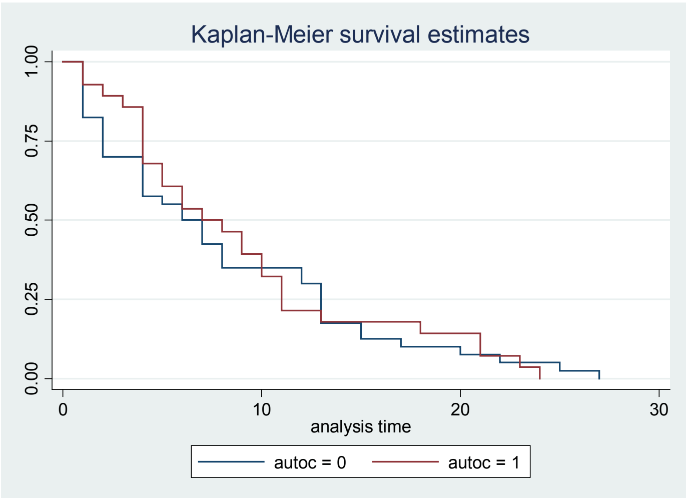


Figure 8: Kaplan Meier Survival Curves

Approximately 25% of all defaults end within one year and a little more than 30% last for more than ten years. But aggregate numbers are misleading. When countries declare default in bad times, the length of the episode is typically shorter than when they repudiate debt in good times. Roughly 45% of defaults in good times last for more than ten years. It looks like outright repudiations may indeed receive harsher punishment. Additionally, in line with Guimarães (2011)'s theoretical predictions about the impact of interest rates on the incentive compatible level of debt, the data shows that approximately 60% of defaults that start after a hike in interest rates last for more than ten years. There seems to be no significant difference between the duration of defaults declared by autocratic and non-autocratic countries.

2.3 Empirical Analysis

The empirical literature on debt defaults has commonly used probit and logit estimates to identify factors influencing the probability of such events occurring, given a set of – usually macroeconomic – control variables[53].

We choose to estimate a Logit Model with Fixed Effects[54] that allows the unobserved variable to be arbitrarily related to the explanatory variables[55], which are lagged to avoid any endogeneity.

We, therefore, run the following regression:

$$P(Y=1|X)=\frac{\exp(\beta X)}{1+\exp(\beta X)} \quad , where \;\; Y = Begin\ of\ Default\ Dummy$$

$$\beta X = \beta_0 + \beta_1 X_1 + \beta_2 X_2 + \beta_3 (Good\ Times\ Dummy).X_2$$

[53] See Catão and Sutton (2002), Detragiache and Spilimbergo (2001), Reinhart (2002), among many others.

[54] We do perform Hausman tests for the random effects (RE) model and do not reject its validity and, therefore, its superior efficiency in our case. So, we present the RE estimates as well. They do not vary substantially from our preferred fixed effects model (FE). Nevertheless, given the potential size problems of these tests, we feel more comfortable with our FE approach, which yields consistent results in any case.

[55] For more details, see chapter 16 of Wooldridge (2002). As several authors point out, including Greene (2000), the differences between probit and logit estimation techniques are usually not very significant, and a choice between them can simply be made on the basis of standard maximum likelihood criteria (see Catão and Kapur, 2004). However, as Manasse, Roubini and Schimmelpfennig (2003) explain, the logit approach usually performs better than probit when the dependent variable is not evenly distributed between the two possible outcomes – a common feature of default episodes. Another issue one must take into account when choosing the estimation procedure is the potential for so-called unobserved effects. When employing probit models, as well as random-effects logit, one must assume the normality of the distribution of the unobserved random variable, conditional on the vector of explanatory variables. One could, conceivably, deal with a potential correlation between the unobserved variable and the controls by including a more specific conditional distribution for that variable, as proposed by Chamberlain (1980), but such technique requires some very specific knowledge on the part of the econometrician.

Where:

$$X_1 = \left[\frac{GDP}{historical\ peak}\right]$$

$$X_2 = \begin{bmatrix} \left(\frac{Reserves}{External\ Debt}\right) \\ \left(\frac{Dollar\ denominated}{Total\ Debt}\right) \\ \left(\frac{Service + Interest}{GDP}\right) \\ \left(\frac{External\ Debt}{GDP}\right) \\ U.S. Interest\ Growth \\ Regime\ Change\ Dummy \\ Change\ towards\ autocracy\ dummy \\ Autocracy\ Dummy \end{bmatrix}$$

$Reference$:

$Reserves = Foreign\ Exchange\ Reserves$

$Service + Interest = Public\ and\ Publicly\ Guaranteed\ Debt\ Service, including\ Interest$

$External\ debt = Public\ and\ Publicly\ Guaranteed\ External\ Debt$

$GDP = Gross\ Domestic\ Product\ in\ constant\ U.S. dollars$

$U.S. Interest\ Growth = 2\ year\ Interest\ Rate\ Growth$ *(U.S. 10-year treasury bonds)*

$Regime\ Change\ Dummy = equal\ to\ 1\ if\ variable$ Durable $>$ 3 years ($Polity$ 2)

$Change\ Towards\ Autocracy\ Dummy = equal\ to\ 1\ if\ change\ in\ Polity\ 2\ was\ negative$

$Good\ Times\ Dummy = equal\ to\ 1\ if\ Default\ occurs\ in\ Good\ HP\ Times$

$Dollar\ denominated\ debt = U.S. Dollar\ denominated\ PPG\ Debt$

$Autocracy\ Dummy = equal\ to\ 1\ if\ Polity\ 2\ index\ between - 10\ and - 6$

This specification tests for differences between the two types of defaults.

In order to evaluate the duration of default episodes, we assign to each default observation the values of its covariates in the year default was declared. We estimate hazard models experimenting with different theoretical distributions (exponential, Weibull, lognormal and loglogistic) and select the Weibull distribution, which clearly best fits our data (both according to the AIC criterion and given the graphical similarity between the Kaplan-Meier estimated hazard curve and the distribution itself).

In the models, we include the world GDP growth and all covariates that determine the beginning of default (from the logit analysis), so as to avoid confusion between what causes a default to be long and what causes a default to start in the first place. We also include year dummies to account for the state of the world economy in each episode or cluster of episodes. Finally, we estimate a Cox proportional hazard model with time invariant covariates.

2.4 Results

2.4.1 Probability of Default

We first reproduced TW's analysis using our dataset and reached very similar conclusions. We then performed logistic regressions to estimate the coefficients of interest, as discussed in section 2.3. Our results indicate that defaults in bad and good times are indeed distinct phenomena. Table 12 displays the resulting coefficients of fixed and random effects logistic regressions when the dependent variable is the "begin of default dummy" and the explanatory variables are interacted with the Good Times Dummy in order to segregate the effects.

All defaults were found to be associated with inter-temporal variations in the liquidity and solvency status of countries[56], with the dummy indicating that output stands below its last peak and with the growth in U.S. interest rates in the two years preceding default. The results indicate that increased reserves/external debt ratios are negatively correlated to the probability of default (and even more with the probability of default in good times), whereas higher U.S. interest rate growth rates and required levels of debt service and interest raise the chances of default. The negative coefficient on external debt/GDP may reflect output declines or credit constraints in the run-up to default. Also, defaulting is usually more costly the more it ends up

[56] As measured by the following variables: ratio of service and interest obligations to GDP, ratio of reserves to external debt, ratio of external debt to GDP.

hurting creditors. Therefore, countries with too much external debt may also feel compelled not to default in order to avoid too costly a process. Larger portions of debt denominated in dollars do not change the probability of default. And finally, changes in the political regime raise the odds of a default only in good times. This result corroborates the notion that mixed-motive models may be needed to satisfactorily explain what we see in the data.

Table 12: Logistic regressions

Dependent Variable: Begin of Default Dummy	**Fixed Effects Logit Coefficients**	**Random Effects Logit Coefficients**
GDP/historical peak	-6.3654*** (1.8126)	-2.7484*** (0.8646)
Reserves/external debt	-0.0510*** (0.0193)	-0.0171 (0.0115)
Dollar-denominated/total debt	0.0182 (0.0129)	0.0064 (0.0076)
Service+interest/GDP	12.3990*** (4.5475)	10.4942*** (3.5270)
External debt/GDP	-2.5620*** (0.7288)	-1.6948*** (0.5557)
2-year U.S. interest rate growth	1.9456** (0.8604)	2.1701** (0.8528)
Political regime change dummy	-0.7875 (0.5089)	-0.3914 (0.4434)
Change towards autocracy dummy	-1.1008 (1.1919)	-1.2490 (1.1065)
Autocracy dummy	-0.4564 (0.4647)	-0.2396 (0.3460)
Interaction with Good Times Dummy		
Reserves/external debt	-0.0564** (0.0282)	-0.0486** (0.0240)
Dollar-denominated/total debt	0.0016 (0.0117)	0.0060 (0.0100)
Service+interest/GDP	3.1847 (6.2092)	-1.3447 (5.3466)
External debt/GDP	-0.1662 (0.8823)	0.4418 (0.7638)
2-year U.S. interest rate growth	-0.7416 (1.3772)	-0.1156 (1.3550)
Political regime change dummy	1.2999* (0.7317)	1.0820* (0.6636)
Change towards autocracy dummy	1.3023 (1.5455)	1.1647 (1.3707)
Autocracy dummy	0.1943 (0.5450)	0.1943 (0.5450)
Observations	1,487	1,687
Number of countries	49	64

Stars indicate statistical significance at 1% (***), 5% (**) and 10% (*) levels.
Independent variables are lagged one period, but for political change dummy and U.S. interest rate growth.

As mentioned above, we have performed Hausman tests to evaluate if the more efficient random effects model would also be consistent for our case and the null hypothesis could not be rejected. The results obtained are very similar to those generated by the fixed effects model, our preferred specification.

We have performed robustness checks of two types. We repeated the econometric estimation: i) excluding countries with debt to concessional debt ratio higher than 80%, ii) including a "contagion dummy"[57] in the analysis, and iii) doing both (i and ii). For the construction of the contagion dummy, we have generated groups of countries based on the pairwise correlation of the variation in the spreads of their sovereign bonds in time (see the groups in the Appendix). We have then created a dummy which takes value 1 for a certain country in a given year if in that same year, any of the countries in its "contagion group" is in default. The results (with the fixed effects specification) are shown in table 13. The contagion dummy is significant, indicating that the probability of default in one country is positively influenced by the occurrence of default in one or more countries in its contagion group (but only when the whole sample is considered). Additionally, we verify that our main findings are robust to these changes in the model.

[57] We thank Marcos Rangel and João Manoel Pinho de Mello for the suggestion.

Table 13: Fixed effects logistic regressions with concessional debt/total debt < 80% and "contagion dummy"

Dependent Variable: Begin of Default Dummy	**Concessional debt < 80%**		**Contagion dummy**		**Both**	
GDP/historical peak	-6.1480 (1.9338)	***	-6.0524 (1.8050)	***	-5.7868 (1.9195)	***
Reserves/external debt	-0.0394 (0.0210)	*	-0.0523 (0.0198)	***	-0.0401 (0.0216)	*
Dollar-denominated/total debt	0.0278 (0.0148)	*	0.0191 (0.0132)		0.0291 (0.0152)	*
Service+interest/GDP	13.8965 (5.2219)	***	12.0591 (4.5609)	***	13.1107 (5.2445)	**
External debt/GDP	-3.1738 (0.9743)	***	-2.5165 (0.7270)	***	-3.0589 (0.9675)	***
2-year U.S. interest rate growth	1.2816 (0.9891)		1.6627 (0.9692)	*	0.8655 (1.0057)	
Political regime change dummy	-0.7945 (0.5775)		-0.8243 (0.5169)		-0.8580 (0.5901)	
Change towards autocracy dummy	-0.8895 (1.2612)		-1.0789 (1.1941)		-0.8271 (1.2666)	
Autocracy dummy	-0.6083 (0.5231)		-0.4460 (0.4735)		-0.5996 (0.5354)	
Contagion dummy			1.4478 (0.7536)	*	1.5428 (0.7632)	
Interaction with Good Times Dummy						
Reserves/external debt	-0.0544 (0.0299)	*	-0.0548 (0.0295)	*	-0.0517 (0.0312)	*
Dollar-denominated/total debt	-0.0015 (0.0124)		-0.0033 (0.0126)		-0.0072 (0.0134)	
Service+interest/GDP	-1.5379 (6.9728)		2.0866 (6.3631)		-2.3044 (7.1475)	
External debt/GDP	0.9816 (1.0442)		0.0058 (0.8937)		1.0959 (1.0556)	
2-year U.S. interest rate growth	0.0742 (1.5280)		-1.2807 (1.4060)		-0.3994 (1.5618)	
Political regime change dummy	1.4343 (0.7959)	*	1.3210 (0.7568)	*	1.4969 (0.8288)	*
Change towards autocracy dummy	1.0322 (1.6280)		1.4415 (1.5504)		1.1428 (1.6380)	
Autocracy dummy	0.3715 (0.6321)		0.3949 (0.5520)		0.6260 (0.6411)	
Contagion dummy			1.5677 (1.0030)		1.5265 (0.9990)	
Observations	1,179		1,487		1,179	
Number of countries	39		49		39	

Stars indicate statistical significance at 1% (***), 5% (**) and 10% (*) levels.
Independent variables are lagged one period, but for political change dummy and U.S. interest rate growth.

2.4.2 Duration of Default

Table 14 presents the outcomes of the estimation of the parametric model following a Weibull baseline distribution and of the semi-parametric Cox regression.

Table 14: Duration models

Dependent Variable: Duration of default Coefficients: Hazard Ratios	Weibull Distribution	Cox Regression
GDP/historical peak	1.5376	2.2761
World GDP growth	0.0174 ***	0.0319 ***
Reserves/external debt	1.0575 ***	1.0517 **
Dollar-denominated/total debt	1.0093	1.0007
Service+interest/GDP	2.4608	2.4270
External debt/GDP	1.6484	1.7599
2-year U.S. interest rate growth	4.49×10^{-9} ***	1.96×10^{-7} ***
Political regime change dummy	0.5576	0.6893
Polity2 index	1.0542 *	1.0442
Number of countries	58	58

Stars indicate statistical significance at 1% (***), 5% (**) and 10% (*) levels.

The coefficients of both models that are similar and statistically significant indicate that longer defaults are associated with higher growth in world GDP or U.S. interest rates when they start. In other words, the international economic situation is important to determine the length of a default episode, not only its occurrence, as many intuitively believe. Additionally, the level of reserves matters for the duration of a default event. We find weak evidence that the type of political regime when a default starts affects its duration.

The results of this procedure strengthen our premise that not all defaults are created equal. As far as our knowledge of the literature extends, this is the first time such duration analysis is applied to this type of problem. The findings presented here shed light on some important, previously unknown characteristics of sovereign debt defaults.

2.5 Concluding Remarks

The findings of Tomz and Wright (2007) have changed the way many economists look at sovereign debt defaults. The data put forth by these authors have crushed many of the common intuitions of researchers, who had not previously considered the overall importance of defaults in good economic times. A disconnect between the dominant strand of the theoretical literature and the behavior implied by the data remains ever present. More recently, though, some noteworthy efforts to bridge this gap have started to emerge (see Benjamin and Wright, 2009). But, so far, our knowledge of the data remains too sketchy, at best, to really shore up the quest for new, all-encompassing models. The present paper, with its more in-depth look at the underlying diversity in default events, aimed to close some of the gaps in our comprehension.

First, we established the robustness of the results published by Tomz and Wright, which do not seem to depend on specific datasets or time periods. We have obtained results that are qualitatively (and quantitatively, actually) equivalent to theirs with a much shorter span of data and a different set of countries under examination. We have, of course, also shown that these findings rely entirely on the use of Hodrick-Prescott filters to determine good and bad times. But since this is a common approach to simulating these models, the fact that over 30% of defaults occur in such good times is of great relevance to any researcher who wishes to match the existing data. We show here that most good-time defaults can be rationalized. They either take place in i) bad interest rate times, ii) bad political times (in terms of a GDP below historical peak), or iii) political transition times.

Second, using logistic regressions, we have found that defaults can be extremely different events depending on whether they start in good or bad times. When a country reneges on its sovereign debt, the determining factors for its decision can all be found in the usual financial/economic restrictions. However, when defaults occur in good times, an additional determining factor becomes important: the political motivation, as represented by our measure of regime transitions. Hence, we have found evidence that these two kinds of defaults are quite different from each other.

But there is more to it. The third, and final, finding of this paper is that the international economic environment, together with the level of reserves when a default episode starts, are key in determining its length. Politics does not seem to play a major role when it comes to duration.

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APPENDIX

Descriptive Statistics

Variable	Observations	Mean	Std. Dev.	Min	Max
% concessional debt/total external debt	2268	36.68	27.34	0.00	100.00
% dollar debt/total PPG debt	2268	47.63	21.52	0.00	100.00
Interest plus service on PPG external debt/GDP	2101	0.05	0.05	0.00	0.54
Reserves/external debt	2107	21.88	35.95	-0.17	502.18
HP deviation of local currency GDP	2182	0.00	0.08	-0.72	0.60
Bad times dummy	2182	0.51	0.50	0.00	1.00
Year default begins dummy	2125	0.04	0.21	0.00	1.00
Year of default dummy	2125	0.35	0.48	0.00	1.00
Regime transition dummy	2150	0.24	0.43	0.00	1.00
Polity 2 index	2148	-1.40	6.70	-10.00	10.00
Openess	2375	53.67	29.89	0.31	220.41
PPG external debt/GDP	2101	0.61	0.72	0.00	8.64
2-year U.S. interest rate growth (ten-year notes)	2272	-0.01	0.19	-0.42	0.43
10-year GDP volatility	2356	0.13	0.07	0.00	0.99
Autocracy dummy	2149	0.47	0.50	0.00	1.00
GDP depth compared to last peak	2182	-0.05	0.11	-0.92	0.00
GDP under last peak dummy	2182	0.38	0.49	0.00	1.00

Theoretical Model Simulated by Tomz and Wright(2007)

The authors focus on the model of sovereign default used by Aguiar and Gopinath (2006). They consider a country which has an exogenous stochastic endowment and borrows to smooth consumption. The country can only trade risk free bonds and default is costly, leading to exclusion from financial markets and lost output (they calibrate it with 2.5 years of exclusion and 2% output cost).

The country is represented by an agent with preferences over state contingent consumption sequences given by:

$$E_0 \sum_{t=0}^{\infty} \beta^t \frac{c_t^{1-\sigma} - 1}{1-\sigma}$$

for some $\sigma>0$, subject to: $c_t + q_t a_{t+1} \leq y_t + a_t$

where c_t is consumption, y_t the output, q_t is the price of the foreign bonds and a_t is the stock of such bonds owned by a country.

Bond price satisfies:

$$q_t = \frac{1 - \pi_t}{1 + r^*}$$

where π_t is the probability of default and r^* is the world interest rate.

Output evolves according to:

$$\ln y_t = \ln \Gamma_t + z_t$$

where Γ_t represents a stochastic trend in output which evolves according to:

$$\ln \Gamma_{t+1} = \ln \Gamma_t + \ln g_{t+1}$$

and

$$\ln g_{t+1} = (1 - \rho_g)\left(\ln \mu_g - \frac{1}{2}\frac{\sigma_g^2}{1-\rho_g^2}\right) + \rho_g \ln g_t + \varepsilon_{gt+1} \text{ and } \varepsilon_{gt+1} \sim N(0, \sigma_g^2)$$

The term z_t captures transitory movements in output and evolves according to:

$$z_{t+1} = \mu_z(1 - \rho_z) + \rho_z z_t + \varepsilon_{zt+1} \text{ and } \varepsilon_{zt+1} \sim N(0, \sigma_z^2)$$

Tomz and Wright (2007) consider two cases: purely transitory and purely permanent shocks. They calibrate the model as in Aguiar and Gopinath (2006) modified for annual data and simulate it for 4000 years, 100 times, discarding the first 2000 years. The results of this simulation are shown in Table 8.

Duration Analysis

We present a simplified layout of the duration models used. Suppose T is a nonnegative random variable that represents the length of a spell and X is a vector of covariates associated with it. The cumulative distribution function for the duration of an event is $F(t|X) = Pr(T < t|X)$, or the probability that the spell will end before time t. An alternative manner of specifying the distribution of T is the survival function, defined as $S(t|X) = 1 - F(t|X)$. The instantaneous rate at which spells terminate at t, given that they have lasted that long is the hazard function: $h(t, X) = f(t, X)/S(t, X)$.

The Kaplan-Meier estimator (a nonparametric method used to estimate the survival and hazard functions) can serve as a graphical aid for the selection of functional forms for the parametric model specification. The Weibull distribution is one of the most commonly used for duration analysis in economics. It is a two-parameter generalization of the exponential distribution which allows a rising or falling monotonic hazard rate depending on the parameter α: $h(t, X) = \alpha\, exp(\beta' X) t^{\alpha-1}$.

Additionally, there is a semi-parametric alternative to the parametric model which allows for the estimation of the slope parameters in the β vector without assuming a particular shape for the hazard function: the Cox model. This is a proportional hazards model and its basic specification can be written as: $h(t, X) = h_{0(t)}\, exp(\beta' X)$. It is possible to estimate the parameters in β without having to make any assumptions about the functional form of the baseline hazard through the use of partial likelihood methods.

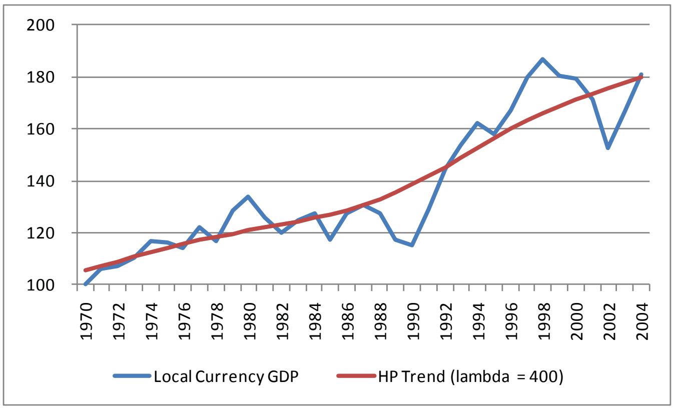
Bad Times are not Robust to the Definition Employed

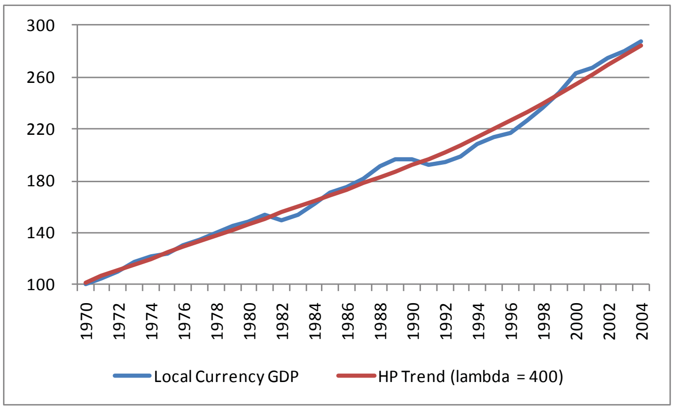
Although not in the scope of this paper, we show here briefly that the classification of a period as a bad (or good) HP time is quite dependent on the definition used. One possibility of changing the definition of bad times used would be to filter the GDP series considering the incidence of shocks to trend growth on top of transitory fluctuations around a stable trend, as advocated by Aguiar & Gopinath (2007). Using the methodology by King, Plosser, Stock and

Watson (1991) to decompose the variance of the series into that due to permanent shocks and transitory shocks would certainly alter the classification of bad and good times (particularly for emerging economies).

The figure below shows the evolution of output for Argentina and Canada and reveals the high degree of variation in trend volatility among countries. It is easy to see that, in the case of Argentina, the introduction of a stochastic trend would transform some of the good HP times in bad times and vice-versa.

Evolution of GDP in Argentina (top) and Canada (bottom)

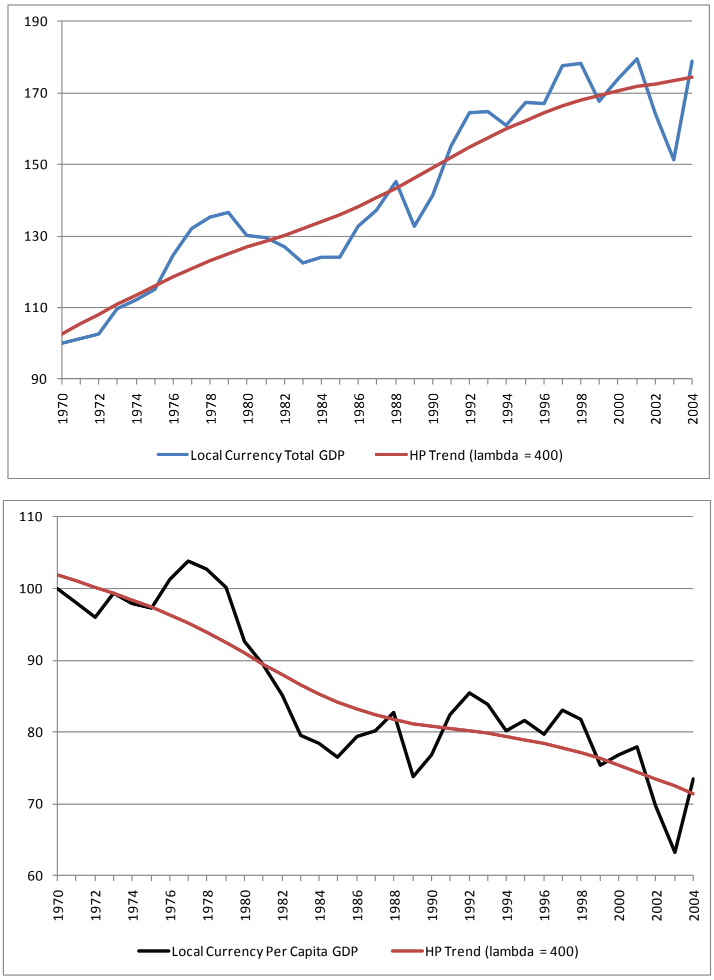




Using a different statistic to classify the state of the world could also be an alternative. One could argue that filtering the GDP per capita series instead of aggregate GDP would be more appropriate, even though GDP per capita does not have a direct theoretical counterpart – few papers deal with population growth. The case of Venezuela, displayed in the figure below, shows the potential disparity between these two series and helps rationalize the 1995 default episode, which took place in a good HP time. The country had experienced the last peak of its GDP per capita 18 years before it declared default. That does not exactly sound like the description of a good time.

The Venezuelan example is convenient to illustrate this further aspect that we wish to bring up, namely the peculiar but recurrent situation in which a country's output stands below (or way below) its last peak during good HP times. This was the case in Venezuela between 1991 and 1999, for example. The fact that a country may experience negative growth during good times (and they do, otherwise GDP would never return to trend) is somewhat counterintuitive but well-understood. Now, when a country faces a downward sloping trend, the label "good times" seems unsuitable even when employed to refer to positive growth years. In such instances, the HP classification really fails to match our basic intuitive notion of "good times".

Evolution of GDP (top) and GDP per capita (bottom) in Venezuela



The point we make here is simply that although fluctuations above and below trend are undeniably the proper statistics for comparisons with theoretical outcomes, the understanding of stories beyond this binary classification may help guide future research. Testing for robustness of the definition of bad times is beyond of the scope of this paper.

Table 1 - "Contagion groups" - countries with correlated risk (correlation > 0.5)

abudhabi	argentina	australia	austria	bahrain	belgium	brazil	bulgaria	canada	chile	china	colombia	croatia
qatar		newzealand	ireland		spain	turkey	turkey			vietnam	vietnam	turkey
malta		malta	denmark		portugal	poland	southafrica			turkey	venezuela	southafrica
dubai		cyprus	czech		ireland	philippines	russia			southafrica	turkey	slovakia
cyprus					estonia	panama	romania			skorea	poland	skorea
					egypt	mexico	poland			russia	philippines	russia
					denmark	croatia	lithuania			poland	panama	romania
					czech	colombia	hungary			philippines	mexico	poland
							czech			panama	indonesia	philippines
							croatia			malaysia	croatia	peru
										indonesia	brazil	panama
										czech		mexico
										croatia		malta
												malaysia
												lithuania
												latvia
												kazakhstan
												indonesia
												hungary
												estonia
												czech
												cyprus
												colombia
												china
												bulgaria
												brazil

Table 2 - "Contagion groups" - countries with correlated risk (correlation > 0.5)

cyprus	czech	denmark	dominican rep	dubai	ecuador	egypt	estonia	finland	france	greece	guatemala	hungary
saudiarabia	ukraine	spain	malta	abudhabi	newzealand	belgium	poland	ireland	spain	spain		turkey
newzealand	turkey	netherlands	ecuador		dominican rep		hungary	france	netherlands	romania		portugal
dominicanr~i	spain	malta	cyprus		denmark		czech	denmark	ireland	portugal		poland
denmark	southafrica	italy			czech		croatia	czech	iceland	poland		philippines
czech	slovenia	france					belgium		finland	malta		indonesia
croatia	slovakia	finland							denmark	lithuania		estonia
australia	skorea	ecuador								italy		czech
abudhabi	russia	cyprus								ireland		croatia
	romania	belgium								czech		bulgaria
	portugal	austria										
	poland											
	philippines											
	panama											
	malta											
	malaysia											
	lithuania											
	latvia											
	kazakhstan											
	italy											
	ireland											
	indonesia											
	hungary											
	greece											
	finland											
	estonia											
	ecuador											
	cyprus											
	croatia											
	china											
	bulgaria											
	belgium											
	austria											

Table 3 - "Contagion groups" - countries with correlated risk (correlation > 0.5)

iceland	indonesia	ireland	italy	kazakhstan	latvia	lebanon	lithuania	morocco	pakistan	peru	philippines	poland
netherlands	vietnam	spain	spain	turkey	czech	malta	turkey			turkey	vietnam	turkey
malta	turkey	portugal	portugal	russia	croatia		portugal			philippines	turkey	spain
france	southafrica	netherlands	greece	philippines			poland			croatia	southafrica	southafrica
	skorea	greece	denmark	indonesia			philippines				skorea	slovakia
	russia	france	czech	czech			indonesia				russia	skorea
	romania	finland		croatia			greece				romania	russia
	poland	czech					czech				poland	romania
	philippines	belgium					croatia				peru	portugal
	panama	austria					bulgaria				panama	philippines
	malta										mexico	panama
	malaysia										malta	mexico
	lithuania										malaysia	malaysia
	kazakhstan										lithuania	lithuania
	hungary										kazakhstan	indonesia
	czech										indonesia	hungary
	croatia										hungary	greece
	colombia										czech	estonia
	china										croatia	czech
											colombia	croatia
											china	colombia
											brazil	china
												bulgaria
												brazil

Table 4 - "Contagion groups" - countries with correlated risk (correlation > 0.5)

portugal	romania	russia	southafrica	spain	turkey	ukraine	venezuela	vietnam	netherlands	qatar	saudiarabia	malta
spain	turkey	vietnam	turkey	portugal	vietnam	czech	mexico	turkey	ireland	abudhabi	malta	turkey
romania	southafrica	turkey	russia	poland	southafrica		colombia	skorea	iceland		cyprus	saudiarabia
poland	portugal	southafrica	romania	italy	russia			russia	france			portugal
malta	poland	poland	poland	ireland	romania			philippines	denmark			philippines
lithuania	philippines	philippines	philippines	greece	poland			malaysia				norway
italy	indonesia	panama	malaysia	france	philippines			indonesia				newzealand
ireland	greece	kazakhstan	indonesia	denmark	peru			colombia				lebanon
hungary	czech	indonesia	czech	czech	panama			china				indonesia
greece	croatia	czech	croatia	belgium	mexico							iceland
czech	bulgaria	croatia	china		malta							greece
belgium		china	bulgaria		malaysia							dominicanr~i
		bulgaria			lithuania							denmark
					kazakhstan							czech
					indonesia							croatia
					hungary							australia
					czech							abudhabi
					croatia							
					colombia							
					china							
					bulgaria							
					brazil							

Table 5 - "Contagion groups" - countries with correlated risk (correlation > 0.5)

slovakia	slovenia	malaysia	norway	mexico	israel	panama	newzealand	japan	skorea
poland	czech	vietnam	malta	venezuela		turkey	malta		vietnam
czech		turkey		turkey		russia	ecuador		poland
croatia		southafrica		poland		poland	cyprus		philippines
		skorea		philippines		philippines	australia		malaysia
		poland		croatia		indonesia			indonesia
		philippines		colombia		czech			czech
		indonesia		brazil		croatia			croatia
		czech				colombia			china
		croatia				china			
		china				brazil			

3 CAMPAIGN CONTRIBUTIONS AS A COMPULSORY TOLL: PAY-TO-PLAY EVIDENCE FROM BRAZIL

3.1. Introduction

Public opinion's perception of the relationship between money and politics typically reflects the sordid image pictured by Grossman and Helpman (2001, page 225), with "*corrupt politicians in smoke-filled rooms peddling favors and haggling over the price*". It is commonly held that interest groups' money, traded for some form of influence in policies, accounts for most of the funds raised in connection with political campaigns. The academic literature, however, has not yet found consensual explanations to the empirically documented "missing money" puzzle[58], first introduced by Tullock (1972). Why is there so little money in U.S. politics if there are so many political prizes on the table, in the form of public expenditures and protective regulations, for example? I argue that the existence of pay-to-play regulations, which prohibit public contractors to make contributions, are a part of the answer. These laws exclude all consumption expenses and gross investments by the federal government from the total pie of political favors available to be sold by politicians and parties.

In this short paper, I present the case of Brazil as an example of country where government contractors are allowed to finance candidates and parties. In relative terms, elections in Brazil involve much more money than in the Unites States: total reported contributions in one electoral cycle account for about 0.5% of Brazil's GDP (SAMUELS (2001)), whereas in the U.S. they add up to no more than 0.05% of the country's output[59]. Ansolabehere, Figueiredo and Snyder (2003) argue that, in the United States, most contributions are not done for the policy-buying motive, but as a form of personal consumption. According to the authors, approximately \$2.4 billion of a total of \$3 billion raised in the 2000 electoral cycle was donated by individuals in small amounts, while only \$380 million (less than 13% of total donations) came from the treasuries of corporations. In Brazil, the bulk of campaign money comes from corporate donors. More than 90% of total reported contributions for the

[58] As called by Chamon and Kaplan (forthcoming).
[59] www.opensecrets.org

presidential elections of 1998 came from private firms according to Samuels (2001), and the pattern continues in more recent election cycles. Official campaign finance data also shows that contributions in Brazil are highly concentrated in industry terms, with roughly 75% of the money raised in presidential campaigns coming from the construction, finance and heavy industry sectors (SAMUELS, 2001)[60].

Campaign contributions have been modeled in many different ways in the past. In Tullock (1980)'s seminal rent-seeking game, interest groups compete for an indivisible policy favor, and the Nash equilibrium probabilistically designates the winner of the contest according to how much each group spends. Grossman and Helpman (2001) distinguish between the influence motive (interest groups contribute to influence parties' positions regarding one or more policy dimensions), and the electoral motive for donations (interest groups contribute after parties have announced their platforms, aiming to alter election odds in the direction of their preferred positions). Snyder (1990) models corporate donations as investments in contingent assets that pay off only if the candidate is elected. The return for such assets is in the form of relatively small private benefits or political favors to donors.

In all these cases, however, the political favor at stake is either individualistic (i.e. only valuable for a certain player) or indivisible. When pay-to-play laws are absent, though, a large share of the political favors at stake is connected to the granting of government contracts. And because a large number of such contracts are auctioned within a certain politician's term in office (road construction contracts, for example), government contracts can be considered a divisible prize. When that is the case, campaign finance games for sectors that are intensive in government revenues (such as the heavy contractors sub-sector) can be modeled as all-pay-auctions in which players have common valuations of the political prize and rewards are variable with the number of bidders.

One can imagine a simple static example: 50 identical contractors compete for a package of 500 construction contracts. Each of them has an ex-ante probability of attaining 10% of the contracts. Politicians may, in this game, condition the granting of contracts on the ex-ante contribution by contractors. Those who don't donate don't get any contract. Those who pay to play will divide the pie equally (under the simplifying assumption that contributions are

[60] It is important to note that official campaign finance data from Brazilian Tribunal Superior Eleitoral (TSE) is not 100% reliable. More details in the next section.

identical, like a compulsory toll). Such a set-up results in a prisoner's dilemma for contractors. The only Nash equilibrium maximizes revenues for the politician while contractors end up with a share of total contracts equivalent to the expected value of their stakes ex-ante (10% times 500).

Another possibility would be to model this interaction as a winner-takes-all contest. Each competitor would bid a value and the player with the highest bid would receive all 500 contracts. Obvious legal risks and concerns with public image rule out this theoretical possibility.

In the United States, pay-to-pay regulations at the federal level have been effective since 1940, when the Hatch Act was amended to prohibit federal government contractors to make donations. The Federal Election Campaign Act of 1971 maintained the prohibition, but permitted such contractors to establish Political Action Committees (PACs) for the purpose of raising money from individuals to make donations. On top of the federal law, a series of states[61] and a large number of local jurisdictions have their own "pay-to-play" restrictions and reporting requirements.

In Latin America, several countries prohibit public contractors from financing political campaigns. However, according to Speck (2004), the definition of "public contractors" varies across the region. In Argentina and Ecuador, the description is similar to the U.S. definition (contractors are suppliers of public goods and services). In Brazil and Paraguay, only companies that depend on public licenses (such as broadcasting companies) are banned. In reality, until 1994, Brazil used to prohibit contributions by any corporation. Paradoxically, right after the corruption scandal that resulted in the impeachment of President Fernando Collor de Mello, the law was altered to accommodate these donations. "*Reformers recognized that campaign funding by private companies was an undeniable reality and concluded that the law must be adapted to be applicable*" (SPECK (2004), page 33). Strict disclosure requirements were introduced and campaign contribution statements submitted by parties and candidates are readily available on the internet. Nevertheless, the accuracy of such statements

[61] California, Colorado, Connecticut, Florida, Hawaii, Illinois, Indiana, Kentucky, Louisiana, Maryland, Missouri, New Jersey, New Mexico, Ohio, Pennsylvania, Rhode Island, South Carolina, Vermont, and West Virginia.

is poorly enforced and the existence of "caixa dois" (over-the-counter) financing is undisputed.

The effect of the lack of pay-to-play regulations in Brazil goes way beyond the simple addition of otherwise excluded potential contributors to the marketplace. The fundamental distinction is that future bidders in government procurement auctions are not allowed to simply evaluate the trade-off between the contingent political favor and the money spent[62]. Prior to election, candidates virtually blackmail current and prospective public contractors. Under this pay-to-play scheme, players of industries that heavily rely on government contracts have no alternative but to donate. It is a matter of survival. And for these companies, contributions become an extra cost with no matching reward. The existence of sectors that are highly dependent on revenues from public contracts and the absence of pay-to-lay laws can generate a great amount of extra campaign contributions.

In section 3.2, I discuss the existing theory and present a simple model of the pay-to-play scheme and then, in section 3.3, I present the data and test the main implications of the model. Section 3.4 concludes.

3.2. Theory

Snyder (1990) models corporate donors as investor-contributors seeking relatively small private benefits or political favors. By contributing, firms are actually investing in contingent claims (in the form of political favors) that pay off if the candidate is elected. The author assumes that the demand for these favors is perfectly elastic and finds an equilibrium through the application of a no-arbitrage condition: the return on political investments equals the rate of return that the firm could obtain on a similar investment. Favors promised to different firms are orthogonal and the behavior of a certain firm has no influence on the payoff of other firms, or on the ability of the politician to offer favors to another firm. This is a realistic assumption for a part of the contributions observed, specially for open-seat House races, as Snyder points out. When it comes to presidential races, however, other classes of investor-contributors may also play an important role in campaign finance. Namely: i) special interest

[62] As in Baron (1989) and Snyder (1990).

groups of companies that manage to overcome the free-rider problem (jointly trading money for sector regulations, taxes, subsidies), and ii) players of sectors or sub-sectors that are highly dependent on government purchases and contracts (such as civil engineering firms and heavy contractors involved in the construction of roads, railways and utility projects, for example).

In Tullock (1980)'s model of lobbying, the political favor is seen as the prize of a contest and the probability of winning the "political prize" is proportional to the value of a certain lobbyist's contributions relative to the total value of bids made. All those in the contest incur costs for sure while there is no certainty that the lobbyist with the higher contribution gets the prize. The outcome of the contest is the assignment to each lobbyist of a probability that she will be the winner. Another way to model corporate political participation is through first-price all-pay auctions, introduced by Hillman and Samet (1987), Hillman and Riley (1989), Baye et al. (1996). In such contests, the highest bidder wins the prize with certainty, while all bidders have to pay their bids.

These models assume asymmetric valuations of the prize by bidders and suppose players have information of the asymmetries among them (the valuation distribution function is known, individual valuations are not). Under these assumptions (as opposed to the assumption of symmetric valuations), the incentives of agents to enter the contest are considerably altered. "*A larger value assigned to the political prize by a rival is a barrier to entry for lower-valuation contenders. Smaller numbers thus enter to actively participate in political contests when valuations are asymmetric than when all potential contenders have the same stake in the outcome.*" (HILLMAN; RILEY (1989), page 5).

In this paper, because the value of the political prize is determined by the size of the procurement contract, I assume agents have common valuations of the prize. What they compete for are shares of future procurement contracts. In another simplification, I assume the size of bids to be constant. Although this looks like an unrealistic assumption, it is introduced to make the problem simpler and because the usefulness of the Brazilian campaign contribution database is limited to its qualitative aspects, as explained earlier. The presence of a certain company's name in the list is valuable information (though its absence does not guarantee that it did not donate money). The declared size of donations is largely regarded as being understated. Empirically evaluating the decision to donate or not by a firm has its pitfalls. An empirical exercise involving the numbers in the database would be even more questionable.

Now, although in the proposed model valuations are common and bids are constant, rewards are variable, as in Kaplan et al. (2002). While these authors model bid-dependent rewards, which vary with the size of a player's bid I model rewards that vary with the number of bidders.

Below is the outline of the simple model to illustrate the pay-to-play game.

Environment

There are two parties, A and B, competing to win elections. Their probability of winning a certain election is solely a function of campaign contributions raised. Elections are held every period. There is a set of N identical contractors competing in a sector in which all revenues come from public contracts. In an infinitely repeated game, contractors are allowed to make contributions to parties in the beginning of each period and contracts are granted after election takes place at the end of each period. Contractors have no preference for any political party and no information about contributions raised by parties. From their perspective, parties A and B have equal probability of winning.

Parties

Parties maximize campaign contributions raised. The maximization problem by parties is subject to one restriction: there is an informal cap on individual contractor contributions which corresponds to the maximum value that allows parties to mask eventual correlations between donations and contracts granted. The payoff function of party *p* is given by:

$$F_p = \sum_{t=1}^{\infty} \sum_{i=1}^{N} c_{ipt}$$

$$s.t. \qquad c_{ipt} \leq \bar{c} \quad , for \quad all \quad i, \quad p \quad and \quad t$$

Where *i* indexes contractors, c_{ipt} denotes the contribution from contractor *i* to party *p* in period *t* and $\bar{c}$ the contribution cap.

Parties choose the auction format that maximizes their payoff. They distribute contracts among contractors after they are elected, with no costs and full information about campaign contributions.

Contractors

Contractors seek to obtain favors from elected parties in the form of advantages in the granting of contracts within their term in office. Contractors have a common valuation of the contracts granted per period (*V*) and this value is the same for every period. Contractors must decide whether to make a contribution to a party before they know what share of *V* they will receive in the period. Their payoff function is given by:

$$F_i = \sum_{t=0}^{\infty} \left[s_{it} \cdot V - \sum_{p=A,B} c_{ipt} \right]$$

where s_{it} is contractor's *i* share of total sector revenues from government contracts (*V*) in period *t*.

Equilibrium

Parties recognize that, because c_{ipt} is caped at $\bar{c}$, an all-pay-auction has a higher revenue-generation potential than a winner-takes-all auction. The all-pay auction may reach the maximum revenue-generation potential caped at $N \cdot \bar{c}$ and does not have the burden of producing a spotlight effect around the winner of a winner-takes-all auction.

In the subgame-perfect Nash equilibrium that illustrates the mechanism proposed by this paper, the parties choose the following strategy:

$$S_p = \begin{cases} \text{give each of the M contractors who contribute } \bar{c} \text{ in period t a share 1/M of the contracts of period t} \\ \\ \text{give no contracts in period t to contractors who do not contribute } \bar{c} \text{ in period t} \end{cases}$$

And contractors chose the strategy:

S_i = contribute $\bar{c}$ to both parties in every period

Under the condition that $\frac{V}{M} \geq 2 \cdot \bar{c}$, contractors have no incentives to deviate. Parties also don't have incentives to deviate because distributing contracts is costless.

As a result, the outcome of the game is that all N contractors in the sector always contribute to both parties and always receive a share equal to 1/N of the contracts. A total of $2 \cdot \bar{c} \cdot N$ of campaign contributions are generated in the political process and firms share the market equally.

In terms of strategic considerations between the contractors themselves, the all-pay-auction puts them in a sort of prisoner's dilemma. An illustrative static version of their interaction when N=2 is shown below:

Contractor1 \ *Contractor 2*	Bid	Don't bid
Bid	$\left(\frac{V}{2} - \bar{c}, \frac{V}{2} - \bar{c}\right)$	$\left(V - \bar{c}, 0\right)$
Don't bid	$\left(0, V - \bar{c}\right)$	$\left(\frac{V}{2}, \frac{V}{2}\right)$

In the Nash equilibrium, all contractors contribute and receive the share of contracts they would get anyway in a competitive environment. They cannot, unless they collude, reach the cooperative equilibrium. And collusion is not a realistic assumption because if $\bar{c} < \frac{V}{\alpha}$ for some $\alpha < N$ (α being the number of players in a deviating subgroup of contractors), no subgame-perfect equilibrium can be sustained.

Testable Implications of the Model

Although very simple, the model outlined above generates a couple of implications that I test using Brazilian campaign contribution by heavy construction contractors and contract granting data.

The theoretical implications I test are the following:

i. All contractors contribute every period
ii. All contractors contribute to both parties
iii. All contractors who contribute to the winning party receive a share of the contracts
iv. All contractors who don't contribute to the winning party don't receive any contracts

Obviously some adaptations must be made in order to test these propositions using real data. In the next section, I present a detailed explanation of the data used and also the actual tests made.

3.3. Data and results

The dataset used includes information on the 50 largest construction companies in Brazil downloaded from Câmara Brasileira da Indústria da Construção (CBIC)'s website[63]. Besides revenues and percentage of revenues from the public sector over total revenues, the dataset includes, for each company, the specific subsectors in which it competes. I restricted the sample to the 39 companies which work in the construction of highways, hydroeletric plants, dams, tunnels, ports, railways, nuclear power plants, bridges, viaducts, airports, pipelines, subway, water and sewage. Firms which focus only on typically private sector projects were excluded (hotels, residential buildings, shopping centers).

Data on campaign contributions for the last two presidential election cycles (2010 and 2006) was obtained from the Tribunal Superior Eleitoral[64]. Finally, yearly data on contracts granted by the federal government is available at Portal da Transparência[65].

[63] http://www.cbicdados.com.br/menu/empresas-de-construcao/maiores-empresas-de-construcao
[64] http://spce2010.tse.jus.br/spceweb.consulta.receitasdespesas2010/abrirTelaReceitasCandidato.action

To test the first proposition (whether all contractors always contribute), I look at 2006 and 2010 total contributions to parties by firms in the dataset. These include contributions to parties directly and also to individual candidates running for president, governor, senator and member of the lower chamber. Because firms have incentives to camouflage high amounts of donations, the practice of "engineering" the contributions is very common. In some cases a company contributes to a party, which then transfers the funds to different candidates pre-selected by the company[66]. In other instances, companies donate directly to a certain candidate who then transfers the amount to the party for redistribution. That is why adding up all contributions, even those made for spheres other than the federal one, is the most appropriate approach. Table 15 shows the set of contractors and other descriptive statistics for the 2010 election cycle.

[65] www.portaltransparencia.gov.br.
[66] This practice is referred to as "doações ocultas", or hidden contributions

Table 15: 2010 Elections Dataset

Contractor	Contributions 2010 (R$MM)	Gross Revenues 2010 (R$MM)	Revenues public sector / total revenues	Contributions/Revenues public sector 2010	Contributions/Total revenues 2010	Revenues Federal Government 2011 (R$MM)
	(1)	(2)	(3)	(4)	(5)	(6)
Norberto Odebrecht	6.2	5,292.3	62.0%	0.2%	0.1%	11.0
Camargo Corrêa	114.3	5,264.9	35.0%	6.2%	2.2%	89.3
Andrade Gutierrez	64.7	4,183.0	72.0%	2.1%	1.5%	393.2
Queiroz Galvão	66.0	4,035.7	100.0%	1.6%	1.6%	366.1
OAS	52.8	2,612.4	59.0%	3.4%	2.0%	93.9
Galvão Engenharia	25.3	2,128.7	51.0%	2.3%	1.2%	35.5
Delta Construções	2.3	2,109.4	-	-	0.1%	878.1
Mendes Júnior Trading	13.8	1,379.7	80.0%	1.3%	1.0%	47.2
Carioca Christiani-Nielsen	15.3	1,201.7	50.0%	2.6%	1.3%	45.0
Construcap CCPS1	7.6	1,094.6	50.0%	1.4%	0.7%	31.2
EIT - Empresa Industrial Técnica	10.8	943.2	70.0%	1.6%	1.1%	45.1
Egesa Engenharia	13.4	805.1	87.0%	1.9%	1.7%	431.2
WTorre1	4.4	702.2	**0.0%**	-	0.6%	**0.0**
Serveng-Civilsan	13.3	680.9	77.0%	2.5%	2.0%	45.4
ICEC	0.4	618.6	**-**	-	0.1%	**0.0**
Santa Bárbara	3.7	618.0	**55.0%**	1.1%	0.6%	**0.0**
Barbosa Mello	6.4	609.8	70.0%	1.5%	1.0%	295.2
Schahin Engenharia	0.0	589.1	42.0%	0.0%	0.0%	138.4
Via Engenharia	11.6	586.0	53.0%	3.7%	2.0%	18.1
C.R. Almeida Engenharia de Obras	8.9	521.7	95.0%	1.8%	1.7%	27.6
Fidens Engenharia	6.0	481.8	84.0%	1.5%	1.2%	237.7
Azevedo & Travassos	0.0	448.2	**0.0%**	-	0.0%	**0.0**
S. A. Paulista	3.0	440.8	100.0%	0.7%	0.7%	25.5
Toniolo, Busnello	2.5	415.3	28.0%	2.2%	0.6%	49.8
Construtora Aterpa	3.7	395.5	100.0%	0.9%	0.9%	292.4
U&M Mineração e Construção	0.4	375.4	**0.0%**	-	0.1%	**0.0**
Mascarenhas Barbosa Roscoe1	0.5	367.0	**0.0%**	-	0.1%	**0.0**
A.R.G	6.4	350.2	30.0%	6.1%	1.8%	7.2
EMSA	0.6	329.9	96.0%	0.2%	0.2%	79.8
Construtora Triunfo	3.7	291.8	0.0%	-	1.3%	78.9
Construtora Cowan	2.3	284.1	83.0%	1.0%	0.8%	0.8
Grupo Thá	0.0	280.5	0.0%	-	0.0%	**0.0**
Gomes Lourenço	1.3	268.6	70.0%	0.7%	0.5%	50.3
Leão Engenharia	0.02	264.5	25.0%	0.0%	0.0%	**0.0**
Paranasa Engenharia	0.0	263.1	0.0%	-	0.0%	**0.0**
Construtora Sucesso	4.6	248.9	0.0%	-	1.9%	85.8
Constran	4.5	235.5	100.0%	1.9%	1.9%	157.9
J. Malucelli Construtora	0.5	232.9	75.0%	0.3%	0.2%	7.8
Camter	0.7	226.3	94.0%	0.3%	0.3%	80.7

Column 1 shows that contributions by contractors are quite high in absolute terms. Considering an average exchange rate of 1.763 for year 2010, single contractors have made political contributions of as much as US$ 64.8MM. For the sake of comparison, the total value of funds raised by Barack Obama in the 2008 election cycle was of US$ 745MM[67]. In relative terms, donations by contractors are also surprisingly elevated, reaching an average of 0.9% of total gross revenues (Column 5). But because the values of contributions declared by parties are questionable, I prefer to focus the analysis on the binary variable that separates firms that donate from firms that don't. The grey shaded lines support proposition i) showing that only 4 amongst 39 contractors refrain from making a contribution (or an official one). From the 4 contractors who do not contribute, only one of them receives a federal contract in the year following the elections (column 6). On the other hand, 6 contractors who do contribute receive no contracts in 2011 (although 3 of these do not have the government as their commercial focus - column 3). These last two pieces of information, however, while interesting, are not enough to validate or reject propositions iii and iv because the term of the current president is still not finished.

I proceed to evaluate 2006 contribution data now in order to test these two implications and confirm proposition i. I include federal contracts for years 2007, 2008 and 2009, but exclude year 2010 because, given that the same party remained in power, contracts granted after the elections in October 2010 may be connected to contributions made for the 2010 electoral cycle, not the 2006 one.

Table 16 shows the set of contractors and other descriptive statistics for the 2006 election cycle.

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[67] http://www.opensecrets.org/pres08/index.php?cycle=2008&type=SF

Table 16: 2006 Elections Dataset

Contractor	Contributions 2006 (R$MM)	Gross Revenues 2006 (R$MM)	Revenues public sector / total revenues	Contributions/Revenues public sector 2006	Contributions/Total revenues 2006	Revenues Federal Government 2007 - 2009 (R$MM)
	(1)	(2)	(3)	(4)	(5)	(6)
Norberto Odebrecht	1.90	3,894.5	56.0%	0.1%	0.0%	425.4
Queiroz Galvão	0.00	1,206.4	78.0%	0.0%	0.0%	1,022.0
Camargo Corrêa	13.78	1,109.2	48.0%	2.6%	1.2%	304.8
Andrade Gutierrez	4.24	1,016.1	64.0%	0.7%	0.4%	204.0
Construtora OAS	10.85	701.7	90.0%	1.7%	1.5%	377.8
Delta Construções	0.21	585.7	100.0%	0.0%	0.0%	1,607.2
Mendes Junior Trading	2.05	338.8	35.0%	1.7%	0.6%	31.9
Construcap CCPS	0.03	328.6	60.0%	0.0%	0.0%	296.5
Serveng Civilsan	1.50	319.3	55.0%	0.9%	0.5%	50.7
Egesa	5.10	298.6	99.0%	1.7%	1.7%	405.7
A.R.G.	0.34	290.9	80.0%	0.1%	0.1%	442.3
Hochtief do Brasil	0.00	289.9	7.0%	0.0%	0.0%	**0.0**
Galvão Engenharia	0.00	271.1	96.0%	0.0%	0.0%	273.7
EMSA	3.75	267.6	99.0%	1.4%	1.4%	200.7
Fidens Engenharia	2.65	264.4	55.0%	-	1.0%	480.1
Barbosa Melo	4.38	252.0	71.0%	2.4%	1.7%	179.4
Schain	0.00	206.3	46.0%	0.0%	0.0%	**0.0**
Carioca Christiani Nielsen	3.32	201.9	87.0%	1.9%	1.6%	264.3
Triunfo	0.02	195.9	-	-	0.0%	169.1
Estacon	0.27	187.0	79.0%	0.2%	0.1%	0.4
ICEC	0.00	169.2	-	-	0.0%	**0.0**
Via Engenharia	2.33	168.4	93.0%	-	1.4%	30.6
EIT	1.75	160.5	90.0%	1.2%	1.1%	91.8
U&M Mineração e Construção	0.07	157.7	-	-	0.0%	**0.0**
Equipav	0.50	140.9	-	-	0.4%	3.0
CR Almeida Eng. De Obras	0.58	140.6	67.0%	0.6%	0.4%	446.6
Constran	0.01	133.8	60.0%	0.0%	0.0%	156.0
Azevedo & Travassos	0.50	133.5	1.0%	37.8%	0.4%	**0.0**
Beter	0.01	125.0	100.0%	0.0%	0.0%	25.3
Passarelli	0.57	113.5	88.0%	0.6%	0.5%	**0.0**
Integral Engenharia	0.06	110.5	40.0%	0.1%	0.1%	112.0
S. A. Paulista	1.25	103.0	-	-	1.2%	47.9
Aterpa	0.10	102.3	92.0%	0.1%	0.1%	201.6
Ápia	0.02	100.8	79.0%	0.0%	0.0%	35.8
Gomes Lourenço	0.45	99.2	84.0%	0.5%	0.5%	**0.0**
Oriente Construção Civil	0.00	93.8	95.0%	0.0%	0.0%	38.7
Tracomal	0.05	91.5	12.0%	0.5%	0.1%	8.4

Column 1 shows that contractors' contributions in absolute terms in 2006 were much lower than in 2010. In relative terms, donations by contractors were also lower, with an average of 0.5% of total gross revenues as opposed to 0.9% in 2010 (Column 5). The grey shaded lines support propositions i) and iv) showing that only 4 amongst 37 contractors refrain from making official contributions and 3 of these 4 who do not contribute don't receive any contracts from the federal government (column 6). Additionally, in accordance with proposition iii, only 4 of the 33 contractors who do contribute do not receive any contracts in years 2007, 2008 and 2009 (although one of them receives a contract in year 2010).

The last proposition that has to be tested is proposition number ii: "All contractors contribute to both parties". Table 17 shows which contractors contributed with candidates or committees from PT and PSDB in 2010, the two parties competing with similar chances of winning in that year. I selected the 2010 election cycle to test proposition ii because this was the tightest presidential race for which contribution data is available. While the model assumes for simplicity that both parties have equal chances of winning to the eyes of contractors, there is obviously an endogeneity between the decision to contribute and the probability of election of candidates, one affecting the other. Table 17 shows that only 5 of the 34 contractors which contribute to either party do not donate to both[68].

Table 17: Contractors Contributions to PT and PSDB 2010

	Contributed in 2010 to	
Contractor	**PT**	**PSDB**
Norberto Odebrecht		
Camargo Corrêa		
Andrade Gutierrez		
Queiroz Galvão		
OAS		
Galvão Engenharia		
Delta Construções		
Mendes Júnior Trading		
Carioca Christiani-Nielsen		
Construcap CCPS1		
EIT - Empresa Industrial Técnica		
Egesa Engenharia		
WTorre1		
Serveng-Civilsan		
ICEC		
Santa Bárbara		
Barbosa Mello		
Schahin Engenharia		
Via Engenharia		

	Contributed in 2010 to	
Contractor	**PT**	**PSDB**
C.R. Almeida Engenharia de Obras		
Fidens Engenharia		
Azevedo & Travassos		
S. A. Paulista		
Toniolo, Busnello		
Construtora Aterpa		
U&M Mineração e Construção		
Mascarenhas Barbosa Roscoe1		
A.R.G		
EMSA		
Construtora Triunfo		
Construtora Cowan		
Grupo Thá		
Gomes Lourenço		
Leão Engenharia		
Paranasa Engenharia		
Construtora Sucesso		
Constran		
J. Malucelli Construtora		
Camter		

68 Although table 15 shows Construtora Sucesso as a contributor, it contributes to a party allied with PT, but not to PT or PSDB.

3.4. Conclusion

The absence of pay-to-play regulations can account for a considerable volume of political campaign contributions in Brazil. The contribution of this short paper is to propose yet another mechanism to rationalize political campaign contributions by firms. In the story proposed, heavy construction companies are forced to contribute to candidates that have a high probability of winning the elections as parties condition the granting of future contracts to political donations ex-ante.

The game is modeled as an all-pay auction with common valuations by players, divisible prize and constant bid value. In the subgame-perfect Nash equilibrium that illustrates the pay-to-play scheme, construction companies have no incentive to deviate (by not making political contributions) as they are highly dependent on government revenues. Parties, on the other hand, stick to their strategy (of only rewarding those who donate), as granting contracts is costless for them. The obvious implications of this simple pay-to-play model are that all contractors always contribute to both leading parties and all contractors who do contribute to the winning party receive a share of the contracts (as opposed to those who don't). I test and confirm these implications using Brazilian data.

Politicians are able to raise ten times more money in Brazil than in the U.S. (as a fraction of output). In the U.S., political favors available to be sold by candidates are immensely lower than what they would be if contributions by government contractors were permitted. Pay-to-play regulations are certainly desirable. The granting of government contracts on the basis of money rather than merit is not fair nor efficient. And the practice of exchanging cash for contracts is not only detrimental for the society as a whole, but also, and primarily, for the companies stuck in the scheme.

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