

The Price of Political Opposition: Evidence from Venezuela's *Maisanta**

Chang-Tai Hsieh
University of California, Berkeley

Daniel Ortega
IESA

Edward Miguel
University of California, Berkeley

Francisco Rodríguez
Wesleyan University

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Abstract

During 2002-2004, the identities of millions of Venezuelan voters who signed petitions to recall President Hugo Chávez or opposition politicians from office was made public by the government. We match these petition signers to manufacturing firm owners and household survey respondents to measure the economic price of political expression. Put simply, do individuals who join the political opposition pay an economic price? We find that pro-opposition individuals see a fall in their income and disproportionately leave public sector employment, while pro-government individuals leave private sector employment. In addition, Pro-opposition firms have falling profits, less access to foreign exchange, and rising tax burdens (possibly due to selective audits), while the marginal products of capital and labor in pro-government firms decreased. The misallocation of resources associated with political polarization after 2002 contributed to a 5% decline in TFP in our sample of firms.

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“Whoever signs against Chávez... their name will be there, registered for history, because they’ll have to put down their first name, their last name, their signature, their identity card number, and their fingerprint.”

Hugo Chávez, nationally televised address, October 17, 2003¹

1. Introduction

How does political polarization affect individuals and societies? What cost are people willing to pay to express their political opinions, and how are these costs manipulated by rulers to hold on to power? What are the aggregate efficiency impacts of political polarization? These questions have sparked a large political economy literature ranging from theoretical studies of appropriation to empirical analyses of the links between political conflict and economic growth.²

We take a new look at these issues using unique data on individual political preferences in Hugo Chávez’s Venezuela. We study how individuals’ political expression affects firm performance and labor market success, and draw implications for aggregate total factor productivity (TFP).³ We use information on whether or not individuals signed a petition demanding the recall of President Chavez, or the counter-petitions to recall opposition politicians, for Venezuela’s 12 million registered voters. This unusual dataset, called *Maisanta*, uniquely has individual information on actual revealed preference political behaviors, rather than just stated opinions, for the entire voting population (rather than just political or business elites). The data was made publicly available during Venezuela’s recall petition battles of 2002-2004.⁴ We then match these petition signers to manufacturing firm owners and household survey respondents to measure the price of political expression during Chavez’s turbulent rule.

¹ “El que firme contra Chávez está firmando contra la patria,” *El Universal*, October 17, 2003. See also Ciudadanía Activa (2006).

² For some examples, see Hirshleifer (1991) Skaperdas (1992), Alesina and Rodrik (1994), Kuran (1995), and Benabou (2004). Cross-country studies of growth and political conflict include Londregan and Poole (1990), Easterly et al. (1993), and Alesina et al. (1996).

³ Our approach is related to Fisman’s (2001) study of how crony links to Suharto in Indonesia boosted stock market valuations and Khwaja and Mian’s (2006) analysis of Pakistani politicians’ credit access. It also relates to the large literature on patronage in less developed countries (Cox and McCubbins 1986, Barkan and Chege 1989, Case 2001), and on the returns to communist party membership in China (Li et al 2007, Morduch and Sicular 2000).

⁴ Dunning and Stokes (2007) also recently make use of a subset of this database to estimate how political affiliation affects the receipt of government social programs.

We find that opposition supporters experience moderate drops in their overall labor earnings (relative to petition non-signers) after the political affiliation information was released, by 3.9% of average earnings, the “price” of political opposition for everyday people in Chavez’s Venezuela. There is also extensive labor market “churning” across public and private sector employment during and after the recall battle, with opposition supporters significantly more likely over time to work into private sector firms (whose owners are overwhelmingly pro-opposition themselves) while government supporters move into the public sector. Multiple channels are likely to be driving these patterns, but regardless of the exact causes, these job separations could have negative social welfare consequences due to the loss of firm-specific human capital as well as worker job search costs.

We also present striking changes in manufacturing firm performance that are closely linked to firm owners’ politics. Pro-opposition firms (those whose owners signed the petitions calling for Chavez to be ousted) have shrinking profits, less access to foreign exchange (which has been controlled by the government since 2003), and pay significantly higher taxes than other firms post-2003. Local media reports indicate that selective tax audits of opposition firms is a leading explanation for the tax result.

Finally, we explore the aggregate impacts of political polarization for the Venezuelan macroeconomy. Even though many empirical studies have found a correlation between political conflict and growth at the national level (Alesina et al 1996), the cross-country empirical approach has major limitations and also provides little evidence on the underlying mechanisms. We quantify the degree of aggregate factor misallocation across firms using Hsieh and Klenow’s (2007) approach, where increasing dispersion in marginal products (of capital and labor) across firms is an indicator of the constraints and distortions firms face. If factors of production were efficiently allocated, these marginal products would be roughly equalized across firms.

The political polarization in Venezuela was not simply a matter of redistribution from one group or social class to another; we find evidence of important aggregate efficiency consequences.

There is growing dispersion in firm marginal products during and after the recall battle, with pro-government firms showing large decreases in their marginal products of capital and labor. The growing dispersion in marginal products indicates that productive opportunities are going unexploited, most likely due to the growing constraints the government placed on opposition firms, most notably the limited foreign exchange and the higher taxes. In our sample, which covers 34% of Venezuelan private sector manufacturing production, we estimate that these increased distortions across pro-opposition, pro-government, and politically neutral firms are associated with a drop of 5% in overall manufacturing total factor productivity (TFP). If similar effects hold for the rest of the economy, political polarization and favoritism could have important impacts on living standards in Venezuela and in other contexts where there are few effective constraints on executive power.

The rest of the paper proceeds as follows. We start out with a short summary of Venezuela's recent political history, including the history of the *Maisanta* database, in section 2. Section 3 provides a stylized model to help understand how we can use the data to estimate the price of political opposition. In section 4 we present our data sets and explain how we merge them with the petition signer data. Sections 5 and 6 respectively present our empirical results using firm-level and labor-market data, respectively, and the final section concludes.

2. Hugo Chavez's Venezuela and the *Maisanta* Database

Hugo Chávez was elected President of Venezuela in December 1998 with the support of 56% of the electorate. Chávez capitalized on a widespread perception that Venezuela's traditional political parties were corrupt and partly responsible for Venezuela's long economic decline: Venezuelan GDP per worker fell 32% between 1978 and 1998 (Rodríguez 2004).⁵ Once in office, Chávez sought to remake Venezuela's political institutions. One of his first actions was to pass a new Constitution that

⁵ Studies of Venezuelan political economy include Karl (1998), Rodríguez and Sachs (1999), Hausmann (2002), and Hausmann and Rodríguez (2007). See Penfold (2003) and Corrales (2007) on the emergence of Hugo Chavez.

called Presidential and Legislative elections, which he won in July 2000 (this time with nearly 60% of votes), after which he used his newfound authority to enact a series of 49 laws, including a controversial land reform bill and a law that increased the taxes paid by the state-owned oil company.

Venezuela's main business and labor organizations initiated public protests and a series of one-day national strikes to pressure Chavez to reverse course. These protests culminated in a violent confrontation with government supporters on April 11, 2002. Several high-ranking military officers announced on national television that they were disobeying Chávez's order to repress opposition demonstrators and that they had asked Chavez to resign, which he did the following day. However, Chávez's ouster was short-lived, and after two days his supporters in the military gathered enough support to re-install him in power.

Opposition groups intensified their activities, culminating in a two-month strike in December 2002 and January 2003 that crippled the Venezuelan economy. They also pursued a new strategy of petitioning for recall elections.⁶ In November 2002, opposition groups collected 1.57 million signatures (out of 12 million registered voters) calling for a non-binding referendum on Chavez's rule. The signatures were accepted by the Electoral Council, but its decision was later overturned by the Supreme Court, which argued that many signatures had been collected fraudulently, and also proceeded to appoint a new Electoral Council with a pro-government majority.

Undaunted, opposition groups submitted another petition, now backed by 2.8 million signatures, before the new Electoral Council on August 19, 2003. This date marked the midpoint of Chávez's mandate and thus the earliest date at which the Constitutional provision for a binding recall referendum could be invoked. This second petition was again rejected by the Electoral Council, arguing that the signatures were invalid by virtue of having been collected before the midpoint of Chavez's term.

⁶ The ability to petition for recall elections, if backed by the signatures of a pre-specified fraction of registered voters, was a novel feature of the 1999 Constitution. For revoking specific laws or on "matters of national interest" the threshold was 10% of voters; for a constitutional amendment, 15%; and to recall an elected official, 20%.

The Electoral Council then established new rules to govern the petition signing process. Specifically, voters could only sign a petition in one of 2,700 pre-specified polling locations where their identity could be verified by Electoral Council officials, and signing had to occur between November 28 and December 1, 2003. During this four day period, nearly 3.5 million voters signed a petition supporting a recall referendum for Chávez. In response, the government also tabled petitions to recall 38 opposition legislators, which were signed by almost 2.7 million voters.

Afterwards, the government again claimed that the signature collection process had been fraudulent, and the Electoral Council thus began a detailed examination of each signature, ultimately ruling that it could not verify the authenticity of almost 1.2 million signatures.⁷ The Electoral Council then set up yet another process by which these voters could either sign the recall petition again or could withdraw their signature. This final round took place from May 28 to 31, 2004, and over 50% of the voters whose signatures had been challenged showed up to “ratify” their signature.

The recall referendum was finally held on August 15, 2004. Over 59% of voters opposed Chavez’s recall and he retained power.⁸ A partial explanation for Chavez’s survival was the recovery of oil prices in 2004, which boosted economic growth and living standards.

For the purposes of this paper, a key fact is that the identities of all petition signers were publicly available from January of 2003 onwards. The data from the first recall petition was posted online by pro-government legislator Luis Tascón.⁹ Tascón’s webpage was subsequently updated with the names from later petition rounds. On April 20, 2004, the Electoral Council itself published the list of identity card numbers (*cédulas*) for all signers, and set up a website where voters could determine whether their signature had been accepted, rejected, or had to be ratified. The Electoral

⁷ The Electoral Council ruled that 375,000 more signatures were simply invalid, without a “resigning” option.

⁸ Although the opposition denounced fraud, international observers vouched for the legitimacy of the vote count. There has been an academic debate on the existence of statistical evidence of fraud: see Hausmann and Rigobon (2004), Weisbrot, Rosnick and Tucker (2003), Taylor (2003), and Febres-Codero and Márquez (2005).

⁹ Tascón’s stated reason for doing so was to allow citizens to find out whether their signature had been forged by the opposition. See Taynem Hernández, “[MVR Asegura que 72 dirigentes opositores no firmaron solicitud.](#)” *El Universal* January 15, 2003.

Council made the data available to both opposition and government representatives, and both groups later installed similar search programs online.

The widespread publication and use of information on individual political allegiances was unprecedented in Venezuela's democratic history. Many opposition representatives charged that revealing the identities of signers was illegal, and claimed that the list was being used to intimidate government workers and screen job applicants and recipients of social services. The government also accused private sector firms of using the lists to carry out politically-motivated employment discrimination.^{10, 11} We might thus expect there to be a difference between the behavior of potential signers in the first recall drive – carried out in late 2002, before Tascón's webpage was set up – and the later drives after voters became aware that their choices would become public knowledge.

The underlying list of petition signers was compiled into a user-friendly computer program that became known as "Maisanta." This software contains information for *all* registered voters as of March 2004 (a total of 12,394,109 voters) and provides information on: (i) their identity card number, birth date, name, and address; (ii) whether they signed against Chávez in the last petition round (in 2003-2004); (iii) whether that signature was considered valid or invalid by the Electoral Council; (iv) whether they signed the counter-petition against opposition legislators; and (v) whether they participated in any government social programs. In this paper, we combine this data with information on earlier petition rounds downloaded from Luis Tascón's website.

¹⁰ Claims that the *Maisanta* database was used to screen job applicants were widespread (Jatar 2005, Goncalves Gonsalves and Gutiérrez Lira 2005, and Ciudadanía Activa 2006). See, for example, "Denuncian lista discriminatoria en organismos públicos," *El Universal*, 8/805 or OAS (2005), p. 50. On April 15, 2005, president Chávez recognized that the list had been used to screen job applicants, and called for an end to the practice: "There are still places that use Tascón's List to determine who gets a job and who doesn't... That's over. Bury Tascón's List. Surely it had an important role at one time, but not now." See "Chávez's Blacklist of Venezuelan Opposition Intimidates Voters," *Bloomberg*, 4/17/05. The Labor Ministry opened multiple investigations against private sector firms and opposition-controlled local governments for coercing their workers to sign the recall referendum petition, see "Ministra del trabajo garantiza estabilidad a empleados públicos," *Venpres*, 3/22/04.

¹¹ The Electoral Council itself denied having given the data to Tascón, who claimed that pro-government business associates had bought the data from an opposition NGO "En dólares vendió Súmate lista de firmantes, según diputado Tascón," *Agencia Bolivariana de Noticias*, April 21, 2005.

While the *Maisanta* program was originally used by pro-government voter outreach groups during the recall referendum campaign in 2004, it has since been widely distributed to government offices, is sold by street vendors in Caracas for Bs. 10,000 (US\$2 at black market exchange rates) and, as we write this article, can still be downloaded from several websites.

3. A Theory of Petition Signing and Economic Outcomes

We next develop a stylized model to better understand the individual petition signing decision. We consider two cases. The first is where voters expect their petition signing choice is kept secret, which we argued is plausible the 2002 petition round before the Tascon list was posted online, and the second corresponds to the later petition round, captured in the *Maisanta* database. We focus on the decision to sign a petition calling for the recall of Chavez, but it is straightforward to extend this model to the largely symmetric case of signing the counter-petition against opposition officials.

First consider the secret ballot case, which we denote $t=1$. Suppose that there is heterogeneity in both individual (i) political preferences (inherent support for Chavez versus the opposition), and (ii) expected income gains if Chavez stays in power versus if he is ousted. Call T_i the utility gain from simply signing the petition drive (to recall Chavez), where T_i is positive for Chavez opponents and negative for his supporters. The assumption that utility is affected by the act of political participation is often called “expressive voting”. Let Y_{it}^O be the expected income change if the opposition wins the recall vote and removes Chavez from power (“O” stands for Opposition), while Y_{it}^C is the expected income change if Chavez (“C”) survives.

Voters share a common prior on the probability that the Opposition will win the recall vote, denoted $\rho \in (0,1)$. Finally, we assume that each individual’s infinitesimal vote has no effect on the referendum outcome and that voters recognize this fact. The expected individual utility from signing the petition in $t=1$ is then:

$$(1) \quad U_{il}^{SIGN} = T_i + \rho Y_{il}^O + (1 - \rho) Y_{il}^C$$

The expected utility from not signing is:

$$(2) \quad U_{il}^{NOT} = \rho Y_{il}^O + (1 - \rho) Y_{il}^C$$

The individual signs when $U_{il}^{SIGN} \geq U_{il}^{NOT}$. Since the individual's vote is neither publicly known nor verifiable in $t=1$, expressive voting choices in this case simply reflect individuals' political tastes.

$$(3) \quad T_i \geq 0$$

In a slight variant, there may be a positive cost $C_t > 0$ to petition signing, for instance the time costs of going to the polls or voter registration fees. As this cost grows, only Chavez's more fervent opponents choose to sign:

$$(4) \quad T_i \geq C_t.$$

We next consider the $t=2$ case where individuals realize their petition signing choices will become public knowledge, introducing the possibility of retaliation or reward. Define P_{i2} as the punishment faced by individual i if Chavez survives the recall election and individual i signed against him. Note that P_{i2} is different than Y_{i2}^C , the change in income in the event of a Chavez victory regardless of that specific person's signing decision (for instance, due to the broader effects of Chavez's economic policies on their employment sector). B_{i2} is the reward from the political opposition or their business allies if individual i signs the recall petition. For simplicity we assume that this benefit is not conditional on an opposition victory.

A key parameter in our analysis is $P_{i2} - B_{i2}$, the net political cost of signing the recall petition for an individual or a firm owner. Under the assumption that individuals perfectly forecasted that Chavez would survive the recall referendum vote (i.e., $\rho = 0$), this quantity captures their "willingness to pay" for expressing a dissident political view. For those individuals who expected the opposition had a non-trivial chance to win the August 2004 recall referendum ($\rho > 0$), as most pre-

election opinion polls suggested, the observed $P_{i2} - B_{i2}$ overstates the utility cost individuals were willing to incur for publicly expressing their opposition to the government.

The expected utility from signing is now

$$(5) \quad U_{i2}^{SIGN} = T_i - C_2 + B_{i2} + \rho Y_{i2}^O + (1 - \rho)\{Y_{i2}^C - P_{i2}\}$$

while the expected utility from not signing remains as above. Individual i chooses to sign if

$$(6) \quad T_i \geq C_2 + (1 - \rho)P_{i2} - B_{i2}$$

The number of petition signers could differ across the early and later petition signing rounds for several reasons. The number of signers increases in $t=2$ as the costs of participation fall ($C_2 < C_1$), or if the rewards from the opposition B_i are large. The later petition signing rounds featured much more concerted get-out-the-vote efforts than the early rounds, including thousands of official signing booths nationwide, so $C_2 < C_1$ is likely. The number of signers falls if the expected punishment from the government P_{i2} is sufficiently large.¹²

The resulting selection equation allows us to pinpoint what a comparison of economic outcomes for signers versus non-signers captures. As Chavez survived the 2004 recall vote, the observed change in income over time is $Y_{i2}^C + B_{i2} - P_{i2}$ for petition signers and Y_{i2}^C for non-signers, so the estimated average political “cost” of being on the list becomes:

$$(7) \quad E(B_{i2} - P_{i2} \mid SIGN_2 = 1) + \{E(Y_{i2}^C \mid SIGN_2 = 1) - E(Y_{i2}^C \mid SIGN_2 = 0)\}$$

where $SIGN_2 = I(T_i \geq C_2 + (1 - \rho)P_{i2} - B_{i2})$. The estimated difference in income changes (or firm profits) across signers and non-signers yields $E(B_{i2} - P_{i2} \mid SIGN_2 = 1)$, the net political punishment for signing against Chavez, only if the second term is zero, namely under the condition that expected income changes are identical for signers and non-signers.

¹² Two other factors that could affect signing are: (1) the distribution of individual political tastes for Chavez (T_i) could shift over time, and (2) expectations that the recall vote would succeed (ρ) may also have shifted in response to opinion polls. We abstract away from these factors for simplicity.

This selection effect is plausibly non-zero if expressive voting preferences (T_i) partially reflect underlying pocketbook interests, such that those who expect to fare poorly under Chavez are particularly likely to sign against him. There is a large literature in political science demonstrating the strong effect of personal economic interests on voters' preferences (see Markus 1988). This could generate a negative relationship between petition signing and post-2004 economic outcomes even in the absence of a political punishment effect.

We do two things to address this potential bias in the empirical analysis that follows. First, we include detailed controls for individual and firm characteristics plausibly correlated with economic outcomes (Y_{i2}^C), including socioeconomic, demographic, geographic, and sectoral trends, in addition to individual and firm fixed effects and time effects in the panel data analysis. While the rich set of firm and individual control variables in our panel data setting helps reduce omitted variable bias concerns, we cannot entirely rule them out. We also consider the subset of individuals who signed in the first (2002) petition round, corresponding to $t=1$, but did not sign in the later round, and compare their income changes to those who signed in both rounds. The intuition is that the early round are opposition supporters, with unobservable income trends similar to the *Maisanta* signers, plausibly reducing omitted variable bias.^{13, 14}

4. Data and Measurement

We first match the petition signer list in *Maisanta* with information on firm owners in the National Institute of Statistics' Industrial Survey. The Industrial Survey is a census of manufacturing plants

¹³ The comparison of economic outcomes for these two groups becomes:
 $E(B_{i2} - P_{i2} | \text{SIGN}_1 = \text{SIGN}_2 = 1) + \{E(Y_{i2}^C | \text{SIGN}_1 = \text{SIGN}_2 = 1) - E(Y_{i2}^C | \text{SIGN}_1 = 1, \text{SIGN}_2 = 0)\}$.

¹⁴ In future versions of the paper, we will use a February 2008 nationally representative survey of 1300 household heads in 35 localities and municipalities, to better understand individuals' recall petition signing decision-making. The survey contains several questions on respondents' recent labor market history, and whether they believe that political factors are the cause of any of the employment and income fluctuations they have experienced since 2002. We also asked respondents retrospectively about their expectations at the time regarding whether or now their petition signing decision would be made public, and whether they expected to experience any positive or negative consequences from signing a petition, or in the event of a Chavez victory (regardless of their signing decision).

with more than 100 employees and a representative sample of smaller plants. We focus on a balanced panel of roughly a thousand plants from 1996 to 2004. This survey has the standard variables on firm output and inputs, as well as their sector, name and physical location. In addition, in Venezuela information on firm owners is publicly available in legal registries located at government municipal offices. We visited registries in 95 municipalities (in seven states) to obtain the names and ID (*cédula*) numbers of the board members for plants in the Industrial Survey. With these ID numbers in hand, we match firm board members to *Maisanta* to identify the political leanings of 453 plants.¹⁵

The second main dataset is the Venezuelan Household Survey (HHS) also conducted by the National Institute of Statistics. This survey provides standard labor market and demographic information for a nationally representative sample of families. The survey tracks families over three years, interviewing them twice per year. We use the survey waves from the first semester of 1997 to the first semester of 2006.

To match the individuals in the household survey to *Maisanta*, we use the information on geographic location (municipality and *parroquia*) as well as gender and birth date of individuals in the household survey. This information uniquely identifies 97% of the individuals in the household survey. From *Maisanta*, we obtain the address of each individual's voting center, including their municipality and *parroquia*, a relatively small geographic unit corresponding roughly to a neighborhood and containing an average of 25,000 inhabitants. Although *Maisanta* does not provide gender, it does contain voters' names which we use to impute gender.¹⁶ The combination of voting center, birth date, and imputed gender uniquely identifies roughly 7 million individuals in *Maisanta*. In addition, there are some cases where all the individuals in the same demographic "cell" (same

¹⁵ These plants are not a representative sample since we were more likely to locate the registry records for larger plants. For our empirical analysis, we thus re-weight observations by the inverse of the proportion of firms in each sector that made it into our final sample.

¹⁶ We were able to confidently assign gender to 87% of individuals in *Maisanta* using lists of common first names.

gender and date of birth and in the same voting station) voted in the same way in the recall petitions. Including this second group of voters, we end up with almost 10 million voters (with 8.3 million unique IDs). We then match this sample to the uniquely identified individuals in the household survey (by geographic location, birth date, and sex), yielding a final sample of 87,100 individuals in the household survey analysis.¹⁷

5. Political Polarization and Firms

We start with firm-level summary statistics for three groups of firms: the first category are those firms in which some board members signed the petition against Chávez (and no board members signed against the opposition), a second set where the opposite occurred, and a third category covers “neutral” firms where there were either both pro- and anti-Chávez signers or all board members abstained from signing either way (Table 1). In the econometric analysis below, we use a continuous measure of political support (the proportion of signers in each direction), but this breakdown into three distinct groups is a useful starting point for descriptive statistics. The Venezuelan private sector is dominated by the political opposition according to this definition: 71.1% of firms are pro-opposition while only 2.4% are unambiguously pro-government. In terms of the universe of firm board members in the sample, the figures are a bit less extreme with only 55.8% signing against Chavez and 4.2% signing the counter-petition against the opposition deputies. The pro-government

¹⁷ Because this matching strategy relies on the likelihood that there will be few people with the exact same birth date and gender within a given *parroquia*, and because this probability varies depending on the population of the *parroquia*, the fraction of successful matches to the HHS varies by *parroquia* size. We therefore re-weight each observation in the final matched sample by the reciprocal of the match success rate (calculated as the ratio of the matched population to the total population over age 18 in each *parroquia*), which places greater weight on *parroquias* with a lower match success rate, in an attempt to retain sample representativeness.

firms are larger than other firms on average, regardless of whether size is measured in employees, sales, or profits.¹⁸

We pursue a difference-in-differences empirical specification to estimate whether firms whose owners expressed a particular political position saw changing economic fortunes after their political affiliations were made public in 2003:

$$(8) \quad Z_{it} = \alpha_i + \eta_t + \lambda_s t + \beta_1 \{Pro\text{-opposition}_{it} * I(t \geq 2003)\} + \beta_2 \{Pro\text{-government}_{it} * I(t \geq 2003)\} + \varepsilon_{it}$$

Z_{it} is the firm outcome of interest (e.g., profits), α_i is a firm-specific fixed effect, η_t is a time-specific effect, $\lambda_s t$ is an industrial sector-specific time trend, and $Pro\text{-opposition}_{it}$ and $Pro\text{-government}_{it}$ respectively denote the fraction of firm board members who signed against Chávez or against opposition deputies. The effects of any actual or anticipated government policies directed to particular sectors as a whole are captured in the sector-specific time trends and firm fixed effects.

We find that there were real costs to the firms whose board members signed the petition against Chávez, and benefits for signers of the counter-petitions against opposition officials. A firm whose board members all decided to sign the petition saw its log profits decline by 0.308 (Table 2, regression 1) or 27%. For the typical pro-opposition firm, where slightly more than half of its board members signed, log profitability declined by over 15 log points. In contrast, pro-government firms on average became more profitable (log profits up 0.334), though standard errors are quite large and the estimate is not statistically significant. To the extent that political favoritism had already accelerated even before the petition lists were posted in 2003 – say, because the government was already increasingly able to infer who their supporters and opponents were by other means – then these estimates could be lower bounds on the true impact of political favoritism on firm outcomes.

¹⁸ Our measure of profits is given by the firm's operating surplus, as regrettably we do not have data on financial costs by firm. If credit from state-owned banks became cheaper for pro-government firms, then our results would underestimate the differences between profitability due to differential treatment of firms.

There are many ways public policy can affect firm outcomes and a subset can be evaluated in our dataset. The Industrial Survey has information on firms' total taxes paid and subsidies received, and we combine these into a single measure of net taxes paid (and in practice it is driven by tax burden rather than subsidies). Net taxes went up substantially for this sample of firms, with taxes paid (measured as a proportion of output) increasing by 0.48 percentage points for firms whose board members signed the petition (Table 2, regression 2). This is a very large effect since the average tax paid for pro-opposition firms is 1.44 percent of total output (and 5.43 percent of pre-tax profits), implying an increase of almost 33% in taxes paid by pro-opposition firms, or an additional US\$76,340 per year in taxes on average. This large and statistically significant effect holds across a variety of specifications with firm-level controls (for production, pre-tax profits, employment and assets), and state-year fixed effects (regressions 3-4). Including sector-year fixed effects leaves the main point estimate largely unchanged (0.44, standard error 0.24, regression not shown). We do not have data on the frequency of tax audits so do not know if that is the key cause, or if the threat of an audit is driving greater pre-emptive tax compliance. In contrast, net taxes paid by pro-government firms are consistently negative across all specifications (although never statistically significant).

The timing of firm taxes paid is consistent with the view that the release of petition signers names was a major force driving these pattern. Taxes paid by pro-opposition firms are stable from the start of Chavez's term in 1999 through 2002, before a striking increase during 2003-2004 (Figure 1). Tax trends for the pro-government firms are harder to interpret since estimates for this relatively small subsample are imprecise and volatile.

Higher tax rates appear to account for one-fourth of the falling profitability among opposition firms relative to non-signers (Table 2, comparing regressions 1 and 5). Although we cannot measure every other government policy affecting firms we did access public firm-level information on the allocation of foreign exchange during 2004-2006 by the Commission for Foreign Exchange Administration, which supervises Venezuela's strict controls, and this provides further evidence on

the policy levers used to punish pro-opposition firms. Media accounts suggest that foreign exchange allocations have been politicized to favor government supporters.¹⁹ The foreign exchange database also contains firm registry identifiers, again allowing us to link it directly to the industrial survey.

The fact that there were no foreign exchange controls pre-2003 prevents us from using exactly the same econometric approach as above. Instead we employ a cross-sectional specification including detailed sector and state fixed effects as well as firm controls for pre-2002 production, imported inputs and purchases, and exports. According to these estimates, firms whose entire board signed the petition against Chávez saw a decline of 0.67 in their log foreign exchange allocation, relative to a firm where no board member signed the petition (standard error 0.22, statistically significant at 99% confidence, Table 2, regression 6). Yet there is a positive effect of signing the counter-petition against the opposition on log foreign exchange access (coefficient estimate 0.55, standard error 0.26, significant at 95% confidence), and the difference in coefficient estimates across pro-opposition and pro-government firms is highly statistically significant ($p < 0.01$). Pro-opposition firms are also 14.8 percentage points less likely to receive any foreign exchange (significant at 90% confidence, regression 7), a large reduction of roughly one quarter, on the base rate of 58% of sample firms that received some official foreign exchange.

This evidence indicates that an important effect of political polarization was to politicize the allocation of resources across firms. Table 3 investigates the extent of this misallocation. The first column shows that employment grew much more rapidly in pro-government firms than in politically neutral firms after 2003: log employment was up 0.48. The difference between the post-2003 employment growth of pro-government and pro-opposition firms is highly significant, and equality

¹⁹ See El Universal (2004) “Denuncian discriminación en CADIVI,” March 4. There have also been many accounts of the Chávez administration explicitly using tax audits to punish opposition firms. For example, in March 2004, three private TV stations were fined more than US\$2,000,000 for broadcasting political advertisements endorsing the general strike (El Universal, 2004), and in May 2006, the tax collection agency closed down the primary enterprise of opposition presidential candidate Benjamín Rausseo; Rausseo later withdrew his candidacy and his business was allowed to re-open (Castillo, 2006).

of the two coefficient estimates rejected at $p < 0.01$. The result is presented graphically in Figure 2, where some positive pre-2002 employment trends are already apparent for pro-government firms. The average capital stock declined significantly in pro-opposition firms while it increased in pro-government firms (regression 2), and the two estimates are marginally significantly different from each other ($p = 0.06$). Yet output actually *drops* somewhat on average (though not significantly) for pro-government firms (regression 3). The fact that input use is expanding without a commensurate increase in output implies that the average products of labor and capital are both decreasing within pro-government firms, while remaining roughly flat in pro-opposition firms (regressions 4-5).

The widening gap between the average products of labor and capital between pro-government and pro-opposition firms is suggestive of a growing wedge in marginal products between these two types of firms. Specifically, let each firm be characterized by a standard Cobb-Douglas production function:

$$(9) \quad Y_i = A_i K_i^\alpha L_i^{1-\alpha}$$

where the usual notation applies. We denote distortions that decrease the marginal products of capital *and* labor as τ_Y and distortions that raise the marginal cost of capital relative to labor as τ_K . Empirically, the first distortion corresponds to differential taxation (or subsidies) across firms, while the second to differential costs of capital, where one important factor determining capital input costs in Venezuela is access to foreign exchange. Allowing w to denote the wage, R the cost of capital, and P_i the price of good i , profits are:

$$(10) \quad \pi_i = (1 - \tau_{Y_i}) P_i Y_i - w L_i - (1 + \tau_{K_i}) R K_i$$

Profit maximization yields the following marginal revenue products of labor and capital:

$$(11) \quad MRPL_i = \frac{w}{1 - \tau_{Y_i}} \propto \frac{P_i Y_i}{L_i}.$$

$$(12) \quad MRPK_i = R \cdot \frac{1 + \tau_{Ki}}{1 - \tau_{Yi}} \propto \frac{P_i Y_i}{K_i}.$$

With this setup, the decline in the average products of capital and labor (Table 3) in pro-government firms is evidence of a decline in the *marginal* products of capital and labor.

To say something about the magnitude of the aggregate efficiency loss due to these distortions, we need to impose more structure. A parsimonious structure is to assume that aggregate output is a CES aggregate of M differentiated products:

$$(13) \quad Y = \left(\sum_{i=1}^M Y_i^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}}.$$

In addition, if A and the firm marginal revenue products are jointly log normally distributed, there is a simple closed form expression for aggregate TFP:

$$(14) \quad \log TFP = \log \bar{A} + \left(\frac{\sigma-1}{2} \right) Var(\log A_i) - \left(\frac{\sigma-1}{2} \right) Var(\log TFPR_i) - (\sigma-1) Cov(\log A_i, \log TFPR_i)$$

where σ is the price elasticity of demand and $TFPR = \frac{P_i Y_i}{K_i^\alpha L_i^{1-\alpha}}$ a weighted average of the marginal products of capital and labor. The effect of dispersion in the marginal products can thus be summarized by the variance of $\log TFPR$ and the covariance of $\log A_i$ and $\log TFPR$, where this second term captures additional losses in TFP that result if high productivity (A_i) firms are subject to particularly large distortions.

We are now ready to determine how increased political polarization in Venezuela, and the publication of petition signers names in particular, changed these two moments. Specifically, suppose that $TFPR$ and A can both be expressed as a function of the political affiliation of firm owners, Pro-opposition or Pro-government, and a white noise residual. (Empirically we also control for time and industrial sector, but ignore them here to simplify the notation.)

$$(15) \quad \log TFPR_i = \alpha_1 Pro\text{-}opposition_i + \alpha_2 Pro\text{-}government_i + \varepsilon_i$$

$$(16) \quad \log A_i = \beta_1 \text{Pro-opposition}_i + \beta_2 \text{Pro-government}_i + u_i$$

This implies that:

$$(17) \quad \text{Var}(\log TFPR_i) = \alpha_1^2 \text{Var}(\text{Pro-opposition})_i + \alpha_2^2 \text{Var}(\text{Pro-government})_i + \text{Var}(\varepsilon_i)$$

$$(18) \quad \text{Cov}(\log A_i, \log TFPR_i) = \alpha_1 \beta_1 \text{Var}(\text{Pro-opposition})_i + \alpha_2 \beta_2 \text{Var}(\text{Pro-government})_i + \text{Cov}(\varepsilon_i, u_i)$$

The effect of the publication of individuals' political preferences on aggregate TFP via the misallocation of resources can thus be measured by how α_1 and α_2 changed after 2003, as a function of β_1 and β_2 and the variance and covariance terms.

Regressing TFPR (basically the weighted average of Y/L and Y/K) on interactions of the pro-opposition and pro-government variables with a post-2003 indicator, as well as year and sector-year controls, yields the following estimated parameter changes (Table 3, regression 6): $\Delta\alpha_1 = -0.032$ (s.e. 0.106) and $\Delta\alpha_2 = -0.965$ (s.e. 0.420), indicating that the average marginal products of pro-government firms declined sharply after 2003. Since there was no meaningful change for pro-opposition firms, in the calibration we conservatively assume α_1 remained unchanged. The sharp drop-off in TFPR for pro-government firms after 2002 is visible in Figure 3.

We obtain the other parameter values using a similar specification (excluding firm fixed effects since we want to estimate the average A for each sector, Table 3 regression 7), and this yields estimates of $\Delta\beta_1 = 0.067$ (s.e. 0.172) and $\Delta\beta_2 = -1.430$ (s.e. 0.756). This suggests that pro-opposition firms remain similar in output while output in pro-government firms actually falls after 2003. The variances of the *Pro-opposition* and *Pro-government* indicator variables (again controlling for year and sector-year controls) are 0.127 and 0.014 respectively. To derive aggregate implications, we assume that the distribution of petition signers in the complete firm sample is the same as in our sample of "somewhat large" firms.

Putting all the components together, and assuming that the underlying distributions of firm productivity (ε_i and u_i) are unchanged during the sample period, yields the following:

$$(19) \quad \Delta Var(\log TFP) = (0.965)^2 * (0.014) = 0.013$$

$$(20) \quad \Delta Cov(\log TFP, \log A) = (-1.430) * (-0.965) * (0.014) = 0.019$$

The final parameter needed to derive aggregate TFP implications is the elasticity of substitution across sectors, and for this we use Broda and Weinstein's (2006) median estimate of 2.9 (at the 3-digit industry level):

$$(21) \quad \Delta \log TFP = (2.9-1)/2 * (-0.013) + (2.9-1) * (-0.019) = -0.050$$

That is, the cost of the unequal treatment created by political discrimination between pro-government and pro-opposition firms is a decline of 5.0 log points in aggregate TFP, or roughly 5%. Using Broda and Weinstein's 5-95 percentile range for σ (from 2.1 to 5.2) implies declines in log aggregate TFP ranging from -0.045 to -0.065 . (Note that these estimates are unlikely to capture all of the aggregate efficiency distortions caused by Chavez's increasingly unorthodox economic policies since 2003, since distortions affecting all firms, and not just those of a particular political affiliation, are captured in the year fixed effects.)

Efficiency impacts could also grow larger over time due to the endogenous response of capital accumulation to falling productivity. To illustrate in a simple Solow model, the elasticity of the capital changes in productivity is 1.5 (assuming a capital share of one third), implying a further decline of 2.5% in steady state aggregate output.

The effects that we identify in manufacturing are significantly larger than those found in the cross-country growth literature on the macroeconomic consequences of political conflict. Alesina et al. (1996) estimate that the negative effect of experiencing a coup – the closest analogue to Venezuela in our study period, with its failed 2002 coup – is a reduction of only between 0.6 and

1.4% in aggregate output. In contrast, we find that manufacturing productivity had declined by a much larger 5% two years after the 2002 coup attempt.

6. Political Polarization and the Labor Market

Political preferences could enter into the employment decisions of both workers and employers, in the former case if they choose to leave a job where their political views are out of step with their employer, and in the latter case if employers fire (or refuse to hire) qualified workers with different political views. In practice, labor supply and labor demand effects are hard to cleanly disentangle, and we do not attempt to do so in this paper.

Regardless of the exact cause, worker turnover is socially costly, since some job match surplus is destroyed when workers are exogenously forced to change jobs (see Mortensen and Pissarides 1998). The first component is the direct cost to workers of searching for a new job, perhaps enduring an unemployment spell, and adjusting to a new work environment. The second cost of increased turnover is the loss of firm-specific human capital when an experienced worker leaves a firm. This adversely affects firm productivity as well as the worker's wage, if they are unable to transfer these skills elsewhere. While voluntary job shifts have favorable welfare effects since they allow workers and firms to form more productive matches (Akerlof, Rose and Yellen, 1988), exogenous job separations involve a loss of joint surplus, often with persistent adverse effects for workers (Den Haan, Ramey and Watson 1999, Stevens, 1997).

We first compare the pre-*Maisanta* labor market characteristics for three groups of individuals, those who signed against Chávez (pro-opposition), those who signed against the opposition (pro-government), and those who did not sign any petitions. Opposition supporters make up 20% of the household survey sample, government supporters 8%, and the remaining 72% did not sign either petition. Thus the population as a whole appears far less pro-opposition than the private manufacturing firm owners described above.

Pro-opposition and pro-government individuals both earn higher average incomes than non-signers, are somewhat more likely to be employed, and are several years older on average (Table 4). There are also some noticeable differences between pro-opposition and pro-government individuals. Opposition supporters are considerably more likely to be female, are less likely to live in Caracas, and have attained more years of schooling on average than government supporters. Government supporters have slightly higher earnings at baseline, which goes against the popular perception of Chavistas as overwhelmingly poor or working class; however, some of this difference could be due to the concentration of Chavistas in Caracas.

The possibility of bias caused by time-varying omitted variables correlated with individuals' political affiliation is the leading econometric concern. To rigorously establish the impact of political polarization on labor market outcomes in Venezuela, we turn to regression analysis that controls for a range of individual characteristics and time trends. We again focus on a difference-in-differences econometric specification:

$$(22) Y_{it} = \alpha_i + \eta_t + (X_i * t)' \lambda + \beta_1 \{Pro\text{-opposition}_i * 1(t \geq 2003)\} + \beta_2 \{Pro\text{-government}_i * 1(t \geq 2003)\} + \varepsilon_{it}$$

Y is the labor market outcome of interest. α is an individual fixed effect (recall that households are retained in the panel for six semesters), and η is a semester fixed effect, capturing changes in aggregate economic conditions. The $X_i * t$ terms are individual characteristics (including gender, year of birth, educational attainment, and locality in some specifications) interacted with time trends, capturing any differential labor market trends across these groups and partially controlling for time-varying factors correlated with these observed characteristics. $1(t \geq 2003)$ is an indicator variable for the post-2003 period, when petition lists had been made public. Finally, ε is the standard white noise disturbance term, and it is allowed to be correlated across observations for the same individual.

Annual earnings drop for both opposition and government supporters after 2003, with somewhat larger impacts on government supporters in a specification without individual fixed effects (Table 5, regression 1). In our preferred specification with individual fixed effects, semester fixed effects and time trends interacted with individual characteristics, both terms remain negative but the impact on opposition supporters becomes negative and highly statistically significant (-51, standard error 23) while the effect for government supporters is smaller and no longer significant at traditional confidence levels (regression 2). This is evidence that pro-opposition supporters had deteriorating labor market outcomes after 2003. The magnitude is -3.9% of average pre-*Maisanta* income for opposition supporters, not a trivial effect. The sharp drop-off for opposition supporters, and moderate decline for pro-government individuals, are both apparent in Figure 4.²⁰ This result is robust to including locality (*entidad*) specific time trends (regression 3).

A further robustness check is the estimation of an earnings effect on the subsample that signed the first (2002) petition, before Venezuelans believed their signing choices would be made public. We find a large negative point estimate on signing the later *Maisanta* petition on earnings (-121, standard error 74 – Table 5, regression 4), and although not statistically significant at traditional confidence levels, this suggests that later signers paid a price for their political expression.²¹

Political Polarization and Labor Market Turnover

The drop in overall annual earnings is driven by both moderate earnings declines for those with jobs (Table 5, regression 5) and a decrease in the probability of employment for pro-government

²⁰ In figures created using HHS data, we focus on the 2001-2 through 2006-1 period. Recall that we are unable to match individuals across the 2001-1 and 2001-2 semesters, hence in the fixed effects specification, political affiliation effects are effectively estimated only among those individuals in the post 2001-2 sample. We retain the pre 2001-2 observations in the regression analysis in order to more precisely estimate differential time trends across demographic groups.

²¹ As a further check, we control for earnings trends for both camps of petition signers, and find that the estimate on the Signed Against Chavez * Post-2003 terms remains similar at -36 (s.e., 23, not shown). However, these estimates are sensitive to the nature of the time controls, perhaps because of the limited number of survey rounds, and hence we do not emphasize these findings.

individuals (Table 6, regression 1), although neither effect separately is statistically significant. There are much larger shifts in employment sectors. There is a decrease in the probability that opposition supporters are employed in the public sector (-0.0053, standard error 0.0028, regression 2), and a sharp decrease in formal private sector employment for opposition supporters (-0.0224, standard error 0.0077, regression 3). Anecdotal evidence and media accounts suggest that this churning in the labor market was due to both deliberate employer purges of people with differing political views and individuals choosing to sort into work environments where their views were closer to the mainstream. In either case, growing political polarization in the recall period is accompanied by marked shifts in labor market outcomes: the reduction in public sector employment for pro-opposition individuals is 4.3% of pre-*Maisanta* public sector employment, and the analogous reduction in private sector employment for government supporters is 5.9%.

Finally, there appear to be similar increases in informal private sector employment for both pro-opposition and pro-government individuals, although effects are only statistically significant for opposition supporters (Table 6, regression 4). One plausible explanation is that many who lost (or chose to leave) jobs during this turbulent period had to settle, at least temporarily, for lower paying and less secure informal sector jobs.

There is some evidence of heterogeneous labor market impacts. The degree of shifting between the private and public sectors appears greater among men than women: male pro-government supporters are more likely than females to leave formal private sector employment, and male opposition supporters are driving nearly the entire shift out of public sector employment (not shown). Venezuelan labor legislation may contribute to this differential effect, as the labor code

makes it harder to fire women.²² However, there is no evidence of differential impacts as a function of respondent years of schooling, or by residence in Caracas versus elsewhere (not shown).

We do not observe all respondent job shifts, only changes across sectors, namely private formal sector employment, private informal employment, and public employment. This likely leads us to underestimate the total extent of labor market churning due to rising political polarization in Venezuela after 2003. Computing the aggregate social cost of this excess job turnover relies on the estimated value of the job match surplus, and no such estimate exists (to our knowledge) for Venezuela or other Latin American economies.²³

7. Discussion

This paper provides evidence that individuals and firms in a politically polarized society sometimes pay a substantial cost for expressing their pro-opposition political beliefs. Our estimates indicate that signers of recall petitions against Venezuela's Hugo Chávez suffered an average decline of 3.9% in earnings as a consequence of making their political preferences public. Signers of the recall referendum petitions were significantly less likely to be employed in the public sector and more likely to be employed in the informal sector after the signature lists.

The costs paid by pro-opposition individuals were not limited to the labor market. Firms whose board members signed against the government also appear to have lost out. On average, firms with pro-opposition individuals on their board were taxed more heavily, had less access to foreign exchange, and had smaller profits relative to other firms. Pro-government firms also became less productive during this period, potentially as a consequence of bountiful government largesse.

²² For example, there is a prohibition against firing women who are either pregnant or have given birth in the last year. Bermúdez (2006) argues that the adoption of these and other restrictions are a significant cause of the greater growth of female informal sector employment during the 1990s, since they discourage hiring women as well.

²³ The estimated aggregate social costs from increased job displacement, using estimated job match social surplus in U.S. data (Hall 2005) are quite small, perhaps in part due to the undercounting of job displacement in our data (results not shown).

Although our results indicate that signing the petition against the government was unambiguously worse than not signing it, it is less clear that signing pro-Chávez petitions was an optimal strategy for individuals. There is no evidence that pro-Chávez signers had superior labor market outcomes than non-signers: if anything, the data indicates that they had somewhat lower earnings on average (though the difference with non-signers is not statistically significant). However, firm board members who signed pro-Chávez petitions do appear to have generated profits for their stockholders, a result consistent with accounts of the emergence of a new pro-government business elite in Venezuela.²⁴

This paper also provides direct evidence on channels through which political conflict affects the efficiency of resource allocation. We show that there was growing dispersion in firm marginal products across pro-government and pro-opposition firms in Venezuela, likely due to inefficient factor allocation across firms driven by political polarization and favoritism. Assuming that our sample of private manufacturing firms (covering a third of national industrial output) is representative of other firms, this increased politicization of factor allocation contributed to a decline of 5% in aggregate Venezuelan TFP after 2003.

The use of economic incentives to punish opponents in politically polarized environments has been extensively documented by political scientists and historians, and is especially salient in settings where executive power is exercised with few constraints, as in Hugo Chavez's Venezuela. In Cuba, the state uses information on the activities of suspected dissidents collected by a broad network of local committees to mete out punishments including banishment from certain parts of the country, public disgrace, and job loss (Aguirre, 2002). During the Chinese Cultural Revolution, suspected reactionaries and descendants of non-working class backgrounds were sent to live and work in the countryside, effectively barring them from access to a university education (Bernstein, 1977).

²⁴ See, for example, *The Economist* (2007) or Romero (2006).

The implications of these incentives for resource allocation and collective choice were discussed by Kuran (1995) in his study of preference falsification. Kuran argued that whenever individuals could express their political beliefs, they would also face incentives to misrepresent their true preferences. Despite the considerable interest sparked by this research (Frank, 1996, Arce and Sandler, 2003), empirical developments have been hampered by the lack of availability of data on individuals' public political expression. To the best of our knowledge, ours is the first paper to provide an econometric estimate of the economic consequences of publicly expressing a dissident political preference. Consistent with our findings, numerous media accounts and books (Jatar 2006) indicate that Venezuelans have already learned just how high the price of political opposition can be.

Data Appendix

Starting in 1995, there are firm-level identifiers in the Industrial Survey database that allow for the construction of a panel (as in Rodríguez and Pineda 2005). We use the panel for 1995-2004. The total number of plants ever covered by the Industrial Survey during this period is 2519, although the relevant sample for us is the subset of firms with observations during the recall period. In particular, we restrict attention to the 1126 privately owned plants that appeared in the 2001, 2002 and 2003 surveys. Although the Industrial Survey does not explicitly identify each firm, a separate Industrial Directory identifies all the firms surveyed by round. These directories contain information on parish locality (*parroquia*), 3-digit industrial sector code, and firm size, as well as firm name, address, and legal registry identification number, allowing us to uniquely identify 927 of 1126 plants, accounting for 71.4% of total private sector manufacturing output.

A wide variety of industrial sectors are well-represented within the sample, including fifty 3-digit sectors (see Appendix Table 1). To illustrate the fine degree of disaggregation, there are six textile or apparel sectors represented and five distinct food processing sectors.

Our second main dataset, the Venezuelan Household Survey (HHS) has been conducted since 1967. Households are retained in the survey sample for six consecutive semesters in a rotating panel. An internal identifier (IDEX), using administrative information (state of residence, primary sampling unit, household number and person number) is fixed across survey waves, allowing us to match individuals over time. In 2001, the master sample, individual weights, and primary sampling unit codes were updated to reflect the geographical distribution of the population obtained in that year's Census, and this led to changes that unfortunately prevent us from linking households across the first and second semesters of 2001. Yet from the end of 2001 onwards, we are again able to track individuals across rounds (through 2006). The IDEX is unique for 97.2% of observations before 2001-1 and for 82.5% from 2001-2 onwards.

We obtained municipality and *parroquia* of residence codes for each survey round, and based on this information and individual gender and birth date, we construct a second identifier (IDSEX). There are 335 municipalities in Venezuela and 1084 *parroquias*; with a population of 27 million in 2006 (23 million in 1997), there are 24,936 people on average in each *parroquia* (though sizes vary

significantly). The IDSEX identifier is unique for 97.5% of individuals before 2001-1 and 96.8% from 2001-2 onwards, allowing for precise matches across time and datasets.

There are 2,650,651 observations in all 19 waves of the Venezuela HHS. IDSEX has some missing values in every semester due to missing birth date, gender, municipality or *parroquia* data. In the first semester of 1997, as well as from 2004-2 onwards, the birth date variable is not included in the publicly available dataset, so IDSEX is missing and individual identities must be recovered by first matching IDSEX to IDEX in a semester where we have both pieces of data; we then match IDEX across survey rounds. After dropping observations without unique IDSEX and IDEX values within a semester, and recovering 295,371 missing IDSEX observations using IDEX (as described above), we have a total of 1,491,521 survey observations. Finally, we drop the 102,199 observations that have multiple IDEX within a single IDSEX (i.e., if two women with the same date of birth live in the same *parroquia*), since it is impossible to match these individuals across survey semesters. The final household panel thus contains 1,389,322 observations for 459,015 individuals.

Appendix table 2 describes the representativeness of our matched sample for the pre-*Maisanta* period of 1997-1 to 2002-2. The differences between matched and unmatched individuals along socioeconomic and demographic dimensions are relatively minor, and we conclude that our sample of individuals is broadly representative of the Venezuelan adult population.

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Tables and Figures

Table 1: Descriptive statistics, Firm survey (Pre-Maisanta, 1995-2002)

	Signed against Chávez Mean (s.d.)	Signed against Opposition Mean (s.d.)	Did not sign Mean (s.d.)
Total employment	210.6 (276.1)	259.0 (563.6)	227.1 (356.5)
Value of production	10.4 (32.0)	13.9 (33.6)	8.9 (19.7)
Profits (before tax)	2.76 (14.3)	4.68 (13.0)	1.93 (0.8)
Profits (after tax)	2.65 (14.1)	4.60 (12.9)	1.83 (7.9)
Taxes/Production	1.44 (2.91)	1.05 (0.75)	1.43 (2.74)
Fixed Capital	0.15 (0.40)	0.15 (0.33)	0.17 (0.53)
Labor productivity	37.9 (102.8)	29.8 (35.2)	35.5 (52.4)
Capital Productivity	0.35 (2.55)	0.76 (1.99)	0.42 (3.15)
Total firm-year observations (1995-2004)	2720	79	977

Notes: The data is for years 1995– 2002 from the household firm survey. The firm survey data was matched to Maisanta using information on owners' *cédula* number. Values presented are in '000,000 bolívares (1997 real).

Table 2: Public Policy Determinants of Firm Profitability

	Dependent variable:						
	Log profits (pre-tax), 1995-2004	Net tax rate 1995-2004	Net tax rate 1995-2004	Net tax rate 1995-2004	Log profits (post-tax), 1995-2004	Log foreign exchange allocation, 2004-2006	Any foreign exchange, 2004-2006
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Signed against Chávez * Post-2003	-0.308** (0.155)	0.478** (0.215)	0.470** (0.219)	0.561** (0.252)	-0.398** (0.174)	-0.674*** (0.218)	-0.148* (0.084)
Signed against Opposition * Post-2003	0.334 (0.504)	-0.221 (0.620)	-0.369 (0.639)	-0.479 (0.687)	0.578 (0.674)	0.551** (0.258)	-0.046 (0.162)
Firm FE, year FE, time trends*firm sector	Yes	Yes	Yes	Yes	Yes	No	No
Firm-level controls	No	No	Yes	Yes	No	Yes	Yes
State-year fixed effects	No	No	No	Yes	No	No	No
Sector and state dummies	No	No	No	No	No	Yes	Yes
F-test p-value (on equality of Post-2003 coefficient estimates)	0.18	0.28	0.21	0.15	0.13	<0.01	0.062
R-squared	0.82	0.56	0.59	0.61	0.79	0.68	0.47
Observations	3072	3072	3068	3068	3072	260	450
Number of firms	453	453	449	449	453	260	450

Notes: Robust Huber-White standard errors, clustered by firm in columns (1)-(5). Statistically significantly different than zero at 99% (***), 95% (**), 90% (*) confidence. Firm-level controls for the tax regressions include log production, log pre-tax profits, log employment and log assets. Firm-level controls for foreign exchange regressions include log production, share of imported purchases in total purchases, share of imported intermediates in all intermediates, and exports.

Table 3: Political Activity and Firm Outcomes

	Dependent variable:						
	Log total employment, 1995-2004 (1)	Log capital 1995-2004 (2)	Log value of production, 1995-2004 (3)	Log labor productivity, 1995-2004 (4)	Log capital productivity, 1995-2004 (5)	Log TFPR, 1995-2004 (6)	Log A, 1995-2004 (7)
Signed against Chávez * Post-2003	-0.057 (0.087)	-0.252 (0.206)	-0.101 (0.104)	-0.096 (0.096)	0.153 (0.205)	-0.032 (0.106)	0.067 (0.172)
Signed against Opposition * Post-2003	0.477*** (0.176)	0.666 (0.469)	-0.496 (0.414)	-0.938** (0.390)	-1.01* (0.606)	-0.965** (0.420)	-1.430* (0.756)
Firm FE	Yes	Yes	Yes	Yes	Yes	No	No
Year FE, time trends*firm sector	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-test p-value (on equality of Post-2003 coefficient estimates)	<0.01	0.06	0.34	0.03	0.06	0.03	XX
R-squared	0.89	0.77	0.90	0.73	0.63	0.66	0.28
Observations	3761	3724	3724	3724	3687	3687	3687
Number of firms	453	451	453	453	451	451	451

Notes: Robust Huber-White standard errors, clustered by firm. Statistically significantly different than zero at 99% (***), 95% (**), 90% (*) confidence. The “Post-2003” indicator includes 2003. Sector-time trends included in columns (1)-(7). Columns (6)-(7) also include sector fixed effects.

Table 4: Descriptive statistics, Household survey (Pre-*Maisanta*, 1997-2002)

	Signed against Chávez Mean (s.d.)	Signed against Opposition Mean (s.d.)	Did not sign Mean (s.d.)
Annual earnings, in '000 bolívares (2000 real)	1317 (2266)	1353 (2009)	1134 (1912)
Employed (earnings > 0)	0.562	0.598	0.551
Employed in the formal private sector	0.330	0.381	0.328
Employed in the formal public sector	0.122	0.111	0.107
Employed in the informal sector	0.134	0.136	0.137
Year of birth	1964.0	1962.1	1968.2
Female	0.555	0.511	0.487
Lives in Caracas	0.100	0.204	0.092
Years of schooling	8.7	8.2	8.1
Number of household members	2.6	2.4	2.9
Observations (by individual-semester)	57,465	23,044	199,485

Notes: The data is for years 1997 (first semester) – 2002 (second semester) from the household labor market survey. The household survey data was matched to *Maisanta* using individual gender, birth date, and parish (*parroquia*) of residence (as described in the text). The “Employed” variable includes only those with positive labor market earnings.

Table 5: Political Activity and Labor Market Outcomes, 1997-2006

	Dependent variable:				
	Annual earnings, in '000 bolívares			Log earnings, for employed	
	(1)	(2)	(3)	Signed the 2002 petition (4)	(5)
Signed against Chávez * Post-2003	-42 (34)	-51** (23)	-46** (23)		-0.019 (0.013)
Signed against Opposition * Post-2003	-94** (46)	-30 (23)	-10 (30)		-0.013 (0.017)
Signed against Chávez * Post-2004				-121 (74)	
Signed against Chávez	39 (31)				
Signed against Opposition	73* (43)				
Female	-804*** (20)				
Year of birth	-28*** (1)				
Years of schooling	170*** (4)				
Individual fixed effects	No	Yes	Yes	Yes	Yes
Locality (<i>entidad</i>) fixed effects	Yes	No	No	No	No
Year fixed effects, time trends * individual characteristics	Yes	Yes	Yes	Yes	Yes
Locality (<i>entidad</i>) time trends	No	No	Yes	No	No
F-test p-value (on equality of Post-2003 coefficient estimates)	0.31	0.54	0.30	--	0.76
R-squared	0.13	0.67	0.68	0.64	0.74
Observations	289,856	289,856	289,856	25,032	147,429
Number of individuals	85,117	85,117	85,117	7,716	56,113

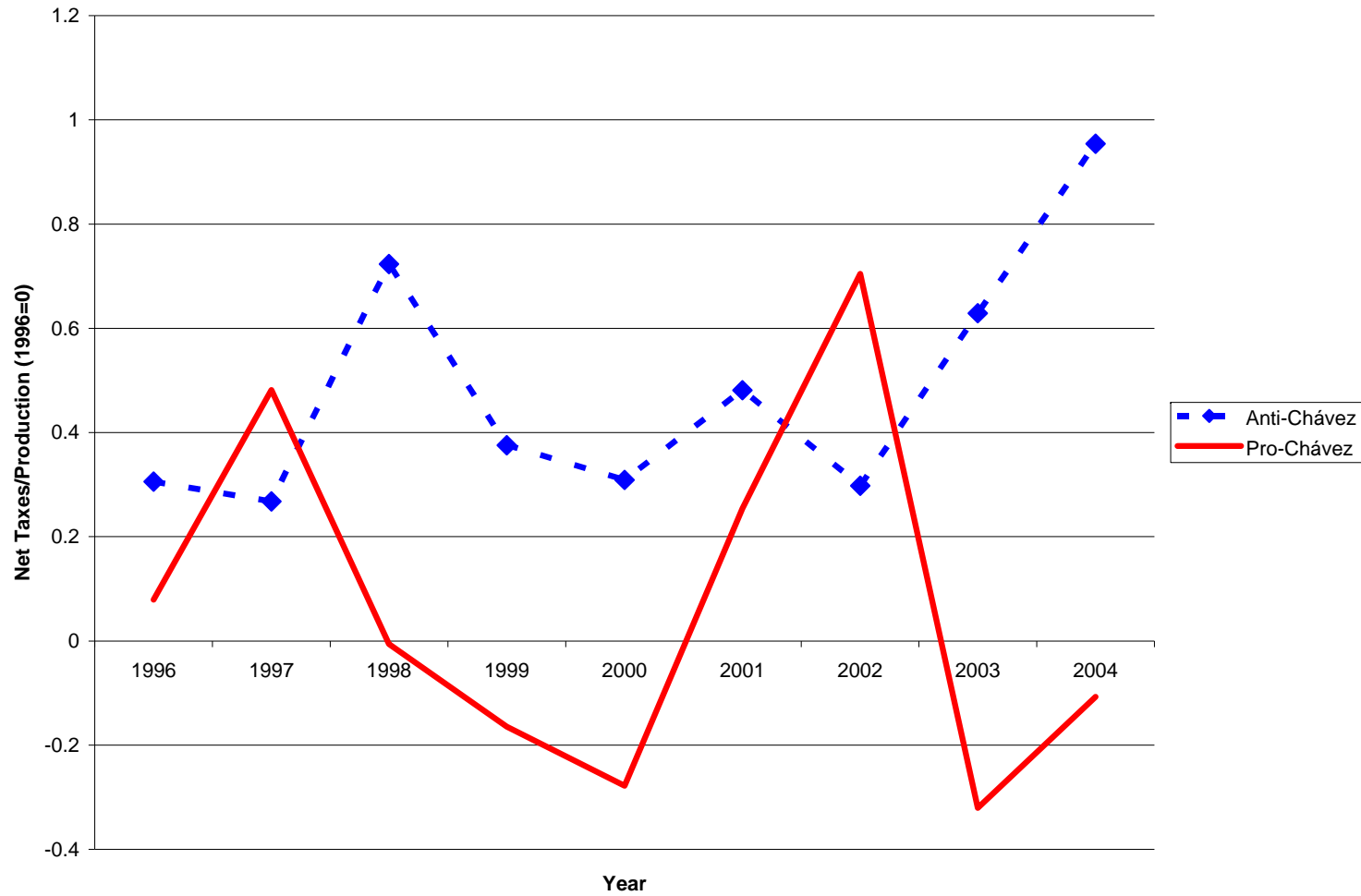
Notes: Robust Huber-White standard errors, clustered by individual. Statistically significantly different than zero at 99% (***), 95% (**), 90% (*) confidence. Controls included in all regressions for female-year, year of birth-year, Lives in Caracas-year, years of schooling-year time trends. The “Post-2003” indicator includes 2003.

Table 6: Political Activity and Labor Market Outcomes, 1997-2006

	Dependent variable:			
	Employed (earnings > 0) (1)	Public sector employment (2)	Private formal employment (3)	Informal sector employment (4)
Signed against Chávez * Post-2003	0.002 (0.005)	-0.0053* (0.0028)	0.0034 (0.0053)	0.0066* (0.0034)
Signed against Opposition * Post-2003	-0.014* (0.008)	0.0061 (0.0040)	-0.0224*** (0.0077)	0.0059 (0.0049)
Individual FE, year FE, time trends*individual characteristics	Yes	Yes	Yes	Yes
F-test p-value (on equality of Post-2003 coeff. estimates)	0.05	0.01	<0.01	0.90
R-squared	0.67	0.72	0.62	0.62
Observations	289,864	289,864	289,864	289,864
Number of individuals	85,117	85,117	85,117	85,117

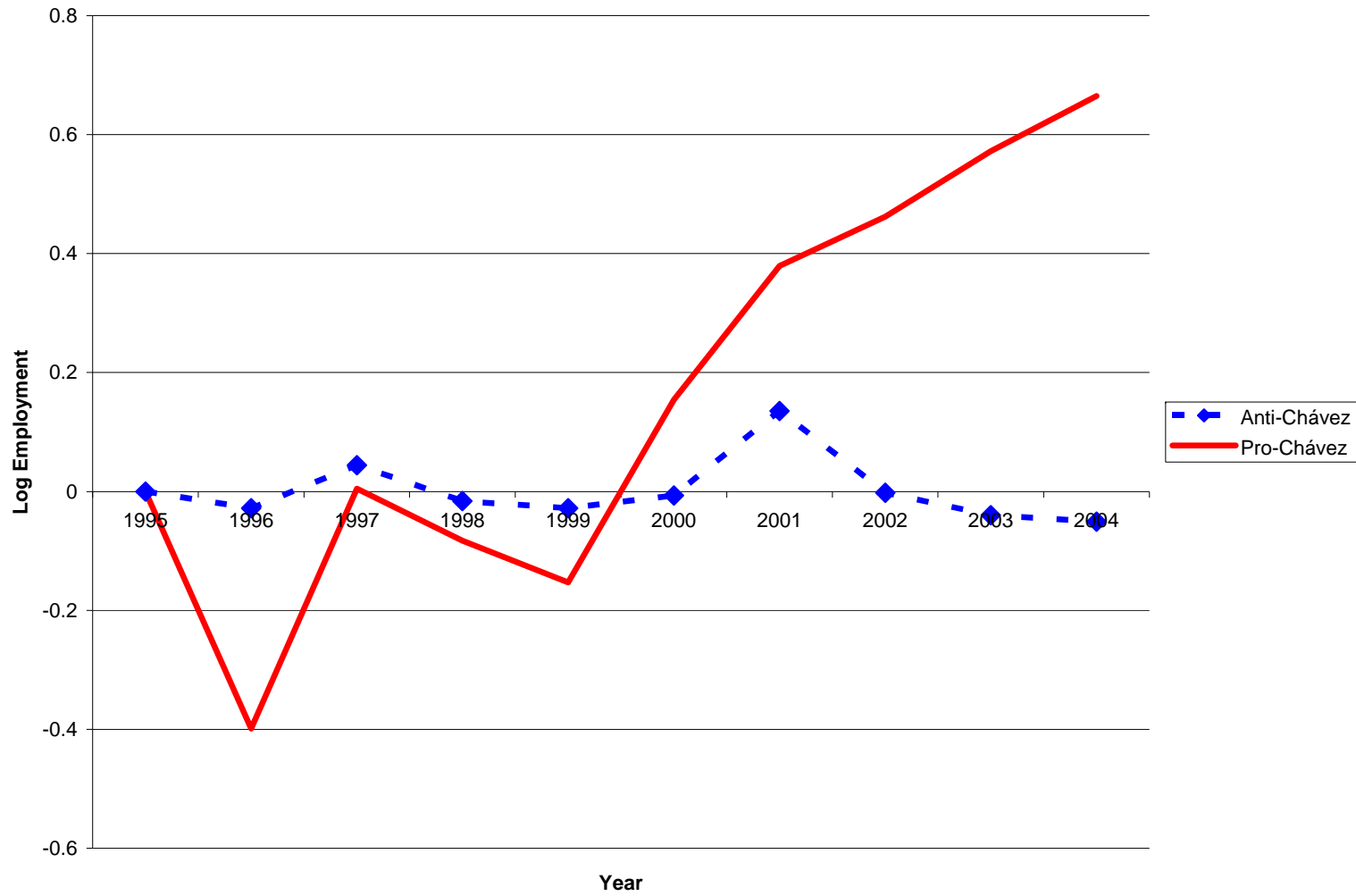
Notes: Robust Huber-White standard errors, clustered by individual. Statistically significantly different than zero at 99% (***), 95% (**), 90% (*) confidence. Controls included in all regressions for female-year, year of birth-year, Lives in Caracas-year, and years of schooling-year time trends. The “Post-2003” indicator includes 2003. The “Employed” variable includes only those with positive labor market earnings.

Figure 1: Political Activity and Firm Net Taxes, 1995-2004



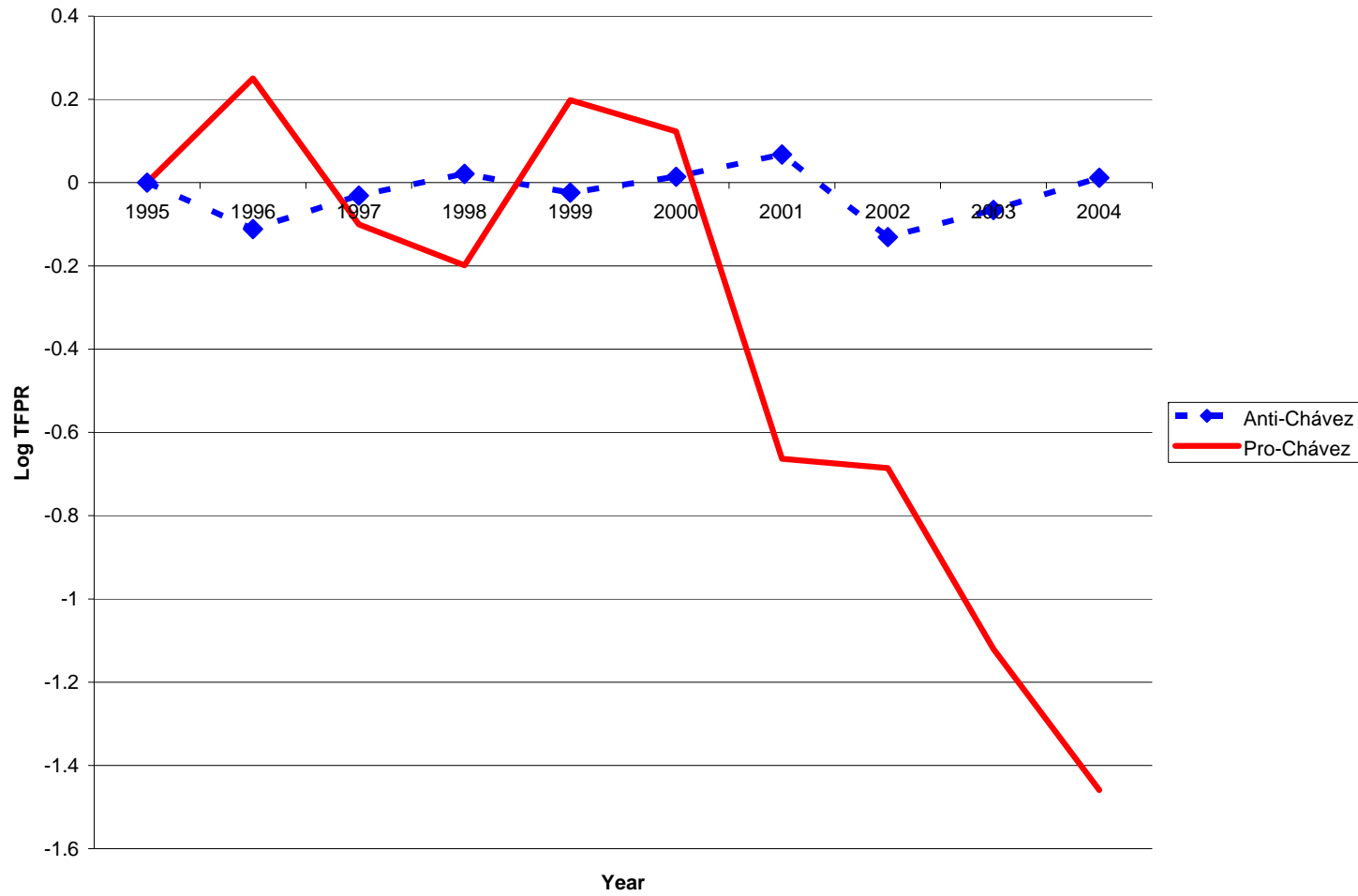
Notes: These effects are relative to petition non-signers, and are conditional on the same controls used in Table 2, regression 2.

Figure 2: Political Activity and Firm Employment, 1995-2004



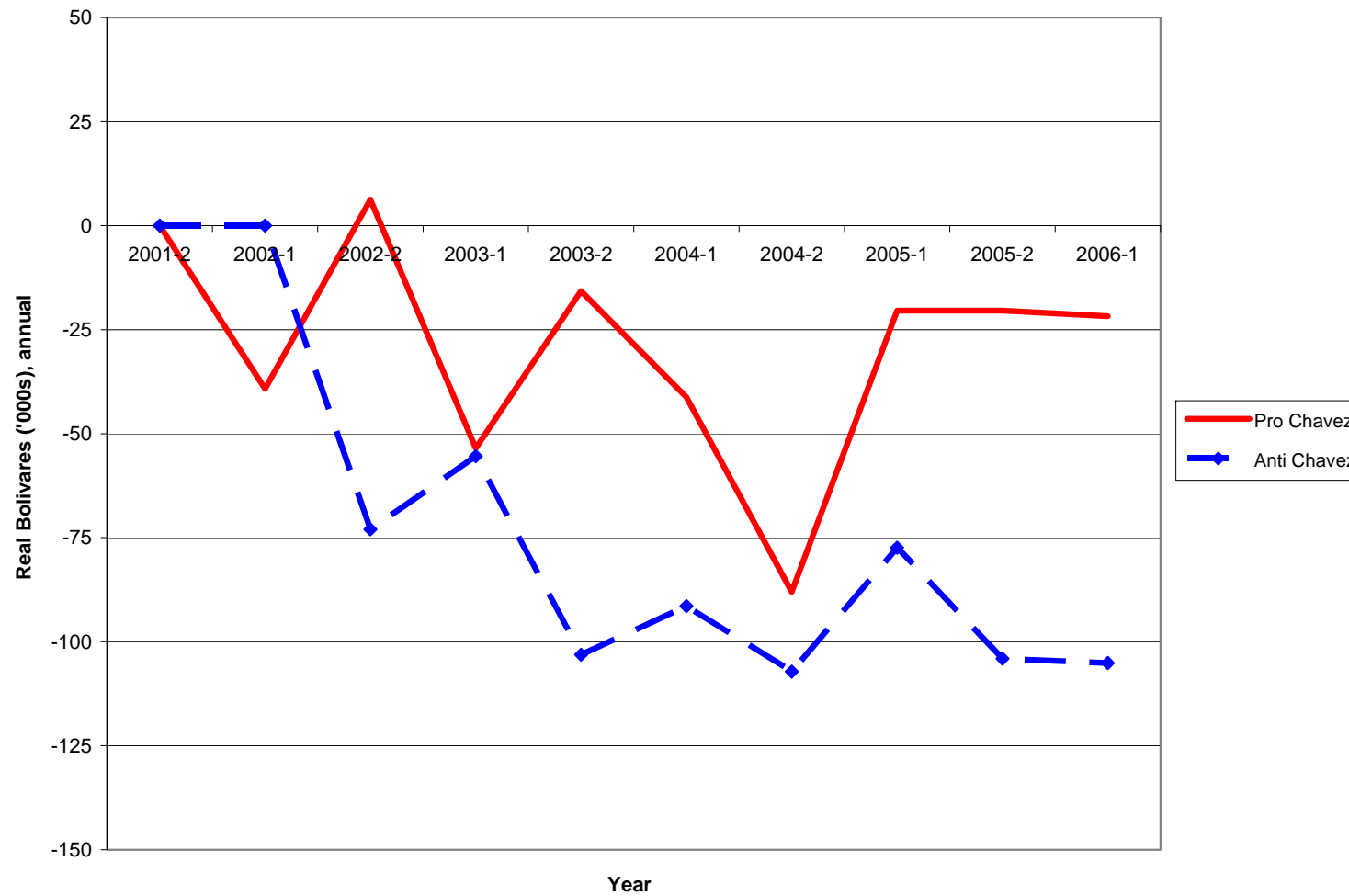
Notes: These effects are relative to petition non-signers, and are conditional on the same controls used in Table 3, regression 1.

Figure 3: Political Activity and Firm TFPR, 1995-2004



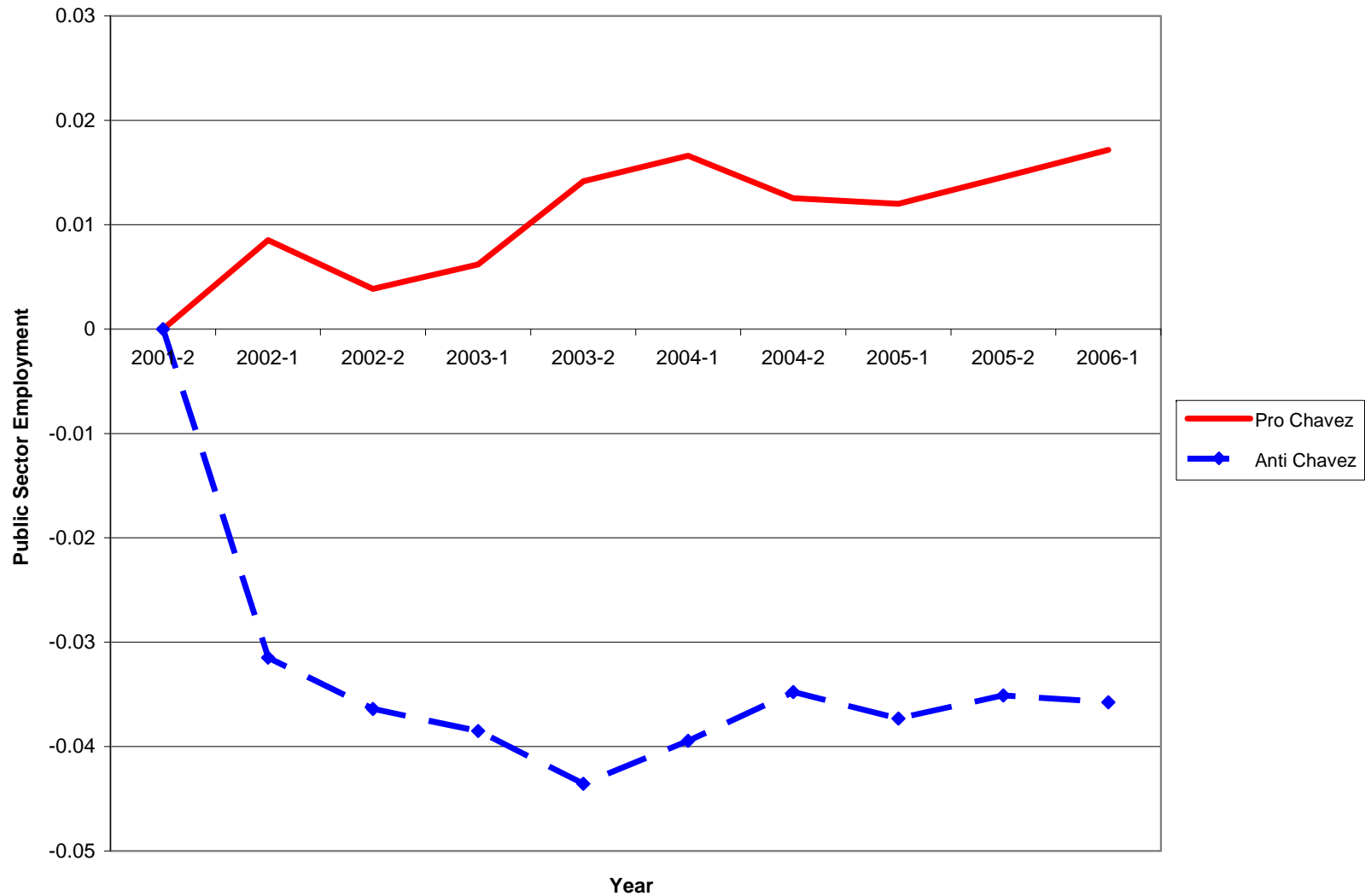
Notes: These effects are relative to petition non-signers, and are conditional on the same controls used in Table 3, regression 6.

Figure 4: Political Activity and Labor Earnings (in real '000 bolívares), 2001-2006 (Household survey data)



Notes: These effects are relative to petition non-signers, and are conditional on the same controls used in Table 5, regression 2.

Figure 5: Political Activity and Public Sector Employment, 2001-2006 (Household survey data)



Notes: These effects are relative to petition non-signers, and are conditional on the same controls used in Table 6, regression 2.

Appendix Figure 1: A Venezuelan Government advertisement calling on people to withdraw their signature (“Retira tu firma”) from the Anti-Chavez recall petition in 2003

RETIRA TU FIRMA

El 40 % de las firmas presentadas por la Coordinadora Antichavista fueron trampeadas o clonadas; dicho de otra manera, son **Firmas Chimbas**.

Utilizaron las cédulas de tus difuntos, la de los abstencionistas crónicos, de los ancianos y a lo mejor la tuya también, pretendiendo sacar al presidente fraudulentamente. Los golpistas enloquecidos y obsesionados quieren robarte la paz, quitarte las misiones y matarte de hambre.

Si tu cédula, la de un amigo o la de un familiar fue utilizada: **DEBES RETIRARLA**
Si firmaste presionado o estás arrepentido: **RETIRA TU FIRMA.**

Búscate en los listados de los centros de votación o la página web www.cne.gov.ve.
Si no puedes por estos medios, comunícate con el PPT a través de los números: 0212-577.45.45, 578.02.12, 578.15.46 y 414.10.95, disponibles las 24 horas del día.

 **RETIRA TU FIRMA**
DEFIENDE LA DEMOCRACIA

Militantes de la Unidad
www.ppt.org.ve

Appendix Table 1: Distribution of Firms by Industrial Sector

ISIC Code	Sector name	Firm-year observations	Firms in 2003
151	Processed meat, fish, fruit, vegetables, fats	307	36
152	Dairy products	58	6
153	Grain mill products; starches; animal feeds	197	22
154	Other food products	260	32
155	Beverages	127	15
171	Spinning, weaving and finishing of textiles	83	9
172	Other textiles	80	9
173	Knitted and crocheted fabrics and articles	14	2
181	Wearing apparel, except fur apparel	101	11
182	Dressing & dyeing of fur; processing of fur	37	4
191	Tanning, dressing and processing of leather	12	3
192	Footwear	101	12
201	Sawmilling and planing of wood	1	
202	Products of wood, cork, straw, etc.	19	3
210	Paper and paper products	157	14
221	Publishing	143	17
222	Printing and related service activities	61	10
231	Coke oven products	5	1
232	Refined petroleum products	8	1
241	Basic chemicals	107	11
242	Other chemicals	389	47
243	Man-made fibres	4	2
251	Rubber products	55	7
252	Plastic products	166	19
261	Glass and glass products	46	4
269	Non-metallic mineral products n.e.c.	191	24
271	Basic iron and steel	70	13
272	Basic precious and non-ferrous metals	70	9
273	Casting of metals	48	2
281	Struct. metal products; tanks; steam generators	70	9
289	Other metal products; metal working services	102	11
291	General purpose machinery	96	9
292	Special purpose machinery	57	7
293	Domestic appliances n.e.c.	17	3
300	Office, accounting and computing machinery	3	1
311	Electric motors, generators and transformers	37	5
312	Electricity distribution & control apparatus	31	3
313	Insulated wire and cable	27	4
314	Accumulators, primary cells and batteries	23	2
315	Lighting equipment and electric lamps	15	3
319	Other electrical equipment n.e.c.	37	7
321	Electronic valves, tubes, etc.	2	
322	TV/radio transmitters; line comm. apparatus	5	1
323	TV and radio receivers and associated goods	9	1
331	Medical, measuring, testing appliances, etc.	29	5
341	Motor vehicles	66	8
342	Automobile bodies, trailers & semi-trailers	26	3
343	Parts/accessories for automobiles	18	3
351	Building and repairing of ships and boats	29	4
359	Transport equipment n.e.c.	18	3
361	Furniture	64	8
369	Manufacturing n.e.c.	63	8
	Total	3761	453

Appendix Table 2: Representativeness of the Matched Household Survey–*Maisanta* sample

	Matched: Household survey to <i>Maisanta</i>	Unmatched: Household survey to <i>Maisanta</i>	Matched – Unmatched
	Mean (s.d.)	Mean (s.d.)	(s.e.)
Annual earnings, in ‘000 bolivares	1187 (1995)	1186 (2021)	1.2 (8.9)
Employed (earnings > 0)	0.526	0.519	0.007 ^{***} (0.002)
Employed in the formal public sector	0.110	0.101	0.010 ^{***} (0.001)
Year of birth	1966.9	1965.5	1.4 ^{***} (0.1)
Female	0.502	0.517	-0.014 ^{***} (0.002)
Lives in Caracas	0.051	0.055	-0.035 ^{***} (0.001)
Years of schooling	8.2 (3.8)	7.8 (3.9)	0.37 ^{***} (0.02)
Number of household members	2.80 (2.09)	2.89 (2.22)	-0.10 ^{***} (0.01)
Observations (households)	137,318	638,911	

Notes: The data is for years 1997 (first semester) – 2002 (second semester) from the household labor market survey. The household survey data was matched to *Maisanta* using individual gender, birth date, and parish (*parroquia*) of residence, and only unique matched retained. Statistically significantly different than zero at 99% (***), 95% (**), 90% (*) confidence.