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The effects of fiscal windfalls on corrruption and selection into politics: Evidence from the Peruvian commodity boom (2003-2014)

TESIS PARA OPTAR EL TÍTULO DE LICENCIADA EN ECONOMÍA

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Abstract

Some resource-dependent countries experience substantial increases in government revenue during natural resource booms. Recent theoretical and empirical contributions (Robinson et al. 2006, Brollo et al. 2013) have argued that such abundance of windfalls can have a negative effect on the functioning of local institutions. In particular, abundance of windfalls could increase the incidence of corruption because their presence aggravates the moral hazard problem that exists between citizens and their elected authorities. This is so because an increased budget means that officials can appropiate rents illegally without compromising their obligations with the electorate, thus distorting the inferences that citizens make about their authorities' competence. Furthermore, such agency problem could be aggravated by the self-selection of relatively worse candidates entering politics (assuming rents are more valuable for the relatively less skilled), because incumbent mayors (including corrupt ones) would face weaker competition. Exploiting substantial time and spatial variation in the amount of mining-related transfers received by the districts of Peru during the 2000s commodity boom, I implement a difference-in-differences strategy to put the mentioned theory to test. I find evidence supporting the hypothesis of a non-monotonic effect of windfalls on the incidence of corruption, and a negative effect of windfalls on the quality of candidates drawn to challenge incumbents and compete for public office. I do not find strong evidence of the theorized relationship between the two phenomena, but I propose further research pathways to improve our understanding of the results.

Key Words: corruption, moral hazard, selection into politics, natural resource curse, political economy

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Introduction

The boom of international mineral prices that took place from 2003 through 2012 translated into an unprecedented increase in transfers targeting subnational governments in Peru. While resource-based growth has a long history in this country, the arrangement of decentralized polities with access to such a large share of its gains is relatively new. Indeed, the massive redistribution that took place was the result of the *Canon* Law (its latest version[1]), which had been passed as recently as 2001, and stated that 50% of the revenue tax paid by mining companies should return to producing areas.

This context of abundance generated a renewed discussion on the consequences of resource-based development; in particular, whether resource abundance is a blessing or a curse. The present study draws from the theoretical and empirical literature of the "Natural resource curse" (NRC hereafter) in order to contribute to our understanding of the political effects of this commodity boom.

Exploiting a source of data that has been underutilized, this paper studies how the increase of fiscal windfalls affected the incidence of corruption and the composition and quality of political representation at the subnational level. As posited by the NRC literature (Robinson et al. 2006, Brollo et al. 2013) resource windfalls can have a negative effect on such outcomes through the following channels: enhanced agency problems (an increased budget means that officials can benefit from increased rents without compromising their obligations with the electorate), and, closely related, self-selection of relatively worse candidates entering politics (assuming rents are more valuable for the relatively less skilled).

The structure of this document is as follows. Section 1 provides an overview of the political explanations of the resource curse developed to date. This is followed by the theoretical framework in Section 2, which draws from recent developments in the field of positive political economy. Section 3 describes the way in which mining rents' transfers work in Peru and also the available sources of data.

[1] The institution of the *Canon* has been in place for a long time, but it has had important variations and its last modification implied a significant increase in the share that returned to the producing areas. For details, see Gruber and Orihuela 2017.

Section 4 describes the methodology. Section 5 presents the results, and the final section concludes.

1 Related Literature

1.1 The Political Aspects of the Natural Resource Curse: An Overview

The stylized fact that resource rich economies tend to have a worse economic performance than their resource-poor counterparts has been revisited multiple times. The evidence supporting this counterintuitive regularity was so overwhelming (Sachs and Warner 1995) that it received a rather fatalist denomination: the *natural resource curse*. For a long time, however, there was a persistent gap between our understanding of the economic mechanisms through which this curse operates[2] and the still weak understanding of the political mechanisms.

Following a seminal paper by M. Ross (1999), the political mechanisms through which a resource curse opperates can be classified into three broad categories. The first one, *cognitive explanations*, includes works such as Nurske and Watkins' about how resource rents generate a "get-rich-quick" mentality among businessmen and a short-sighted behavior by the policy makers. A second category, *societal explanations*, emphasizes the role of non-state actors that push for growth-impeding policies once they see their bargaining power increased by a context of abundance. This is the well-known literature about resource-induced protectionism. Finally, there is a third, separate though quite hybrid, group: *state-centered explanations*. The main assertion here is that a state relying on transfer revenues (natural resource rents, for example) becomes less accountable to their constituencies. This could happen because rents allow a general decrease in taxation (Morrison 2007) or increased possibilities to buy off the opposition (Acemoglu et al. 2004).

A new strand of the NRC literature started to move away from the traditional cross-country approach and started to look into particular institutional settings where the availability of microdata made it possible to test empirically

[2]These include but are not limited to: the 'Dutch disease', the volatility of growth because of commodity prices fluctuations, and the few linkages of extractive sectors with the rest of the economy.

the mechanisms outlined above. These contributions revealed that the effects of abundant natural resources on economic outcomes depended critically on institutional factors such as the rule of law, checks and balances mechanisms, and electoral competition. The present study focuses on *state-centered explanations* of the resource curse, following this microeconomic approach.

1.2 Empirical Political Economy: State-Centered Explanations Put to Test

The insight that an increase in rents may exacerbate agency problems and facilitate corruption receives support from the findings of Brollo et al. (2013), and Casselli and Michaels (2013). Exploiting a discontinuity generated by the rule for assigning federal transfers in Brazil, the first study finds that a 10% increase in transfers raises the incidence of corruption by 7.3 percentage points. They also find that the mechanism behind this is a decreased electoral punishment for corruption (again, exploiting the discontinuity). At the same time, windfalls abundance attract a relatively worse pool of candidates; thus enhancing the agency problem since the incumbent faces less "dangerous" competitors. In particular, this increase in transfers causes a reduction of 2.7 percentage points in the fraction of opponents to the incumbent that hold at least a college degree.

Casselli and Michaels (2013) find that oil windfalls contribute little to improving living standards of the citizens and data from Brazilian media and federal police indicates that this might be explained by a waste of resources induced by the mayors' increasing association with illegal activities. Carreri and Dube (2016) find that a rise in oil windfalls, during Colombia's internal conflict, induced an increase in the use of force to gain power, in particular, an increase in right-wing paramilitary violence that reduces electoral competition.

In the case of Peru we find empirical studies about the effects of fiscal windfalls on outcomes such as public good provision (Maldonado 2014), efficiency of local government expenditures (Ardanaz and Maldonado 2016), investments in the formation of human capital (Ñopo et al. 2016, Calle 2017) and

citizens' support of democracy (Maldonado 2015).

Maldonado (2011) studies the effects of increased windfalls on corruption and is therefore the empirical contribution that is more closely related to the present work. There, corruption is measured using the Module on Governance of the National Household Survey, where citizens are asked whether they have been required to pay bribes by municipal authorities or employees. One limitation of this source of data is that it relies on self reported information from citizens, who don't have incentives to reveal the truth about their involvement in illegal activities. However, the main concern about using such measure of corruption incidence is that it tends to capture only one kind of corruption and arguably not the most harmful to local institutions.

Indeed, recent reports by the Public Prosecutors' Office Specialized in Corruption Crimes (PPEDC hereafter, for its acronym in Spanish) complain that prosecuting this petty corruption (in which the amount of civil damages are not so high and the public officials involved are low-rank) leaves them with little time to investigate big corruption, involving top-rank officials (PPEDC, 2012). This paper makes a contribution in this sense, by using official data from the PPEDC, which includes small, medium and large corruption cases.

One last observation about previous NRC-related studies for Peru has to do with how they have dealt with electoral competition. In most political economy models theoretical predictions regarding final outcomes (such as public good provision, efficiency in expenditure, composition of expenditure, among others) depend critically on an intermediate outcome which is electoral competition.

Both Maldonado (2014, 2015, 2016) and Calle (2017) account for this by considering a Herfindahl-Hirschman Index meant to capture the degree of competition in the election. It is constructed as a standard HHI, summing over the squares of the candidates' vote shares in the district. The limitation of this indicator of political competition is that it is blind to who is entering the pool of competitors. Moreover, what is interpreted as "enhanced political competition" (which has a positive connotation for economists) is not always seen in such an

optimistic light by political scientists who often consider this a symptom of the pulverization of political power (Levitsky 2001, Vergara 2011). The theoretical framework adopted in the present work takes into account that the composition of the pool of opponents is likely to be endogenously determined as natural resource rents abundance changes the incentives to hold public office. This is the second contribution of the paper to the study of the Resource Curse in Peru.

2 Theory: Agency Problems and Selection into Politics

Political economy models attribute three main roles to elections (Persson and Tabellini, 2000). First, if electoral promises can be enforced, elections serve mainly as a means to select among policy platforms. If, on the other hand, the performance of the mayor is not observable to its constituents, then we are in the setting of a standard agency problem[3]. In this context, and under the assumption that politicians value their political survival, elections can serve as a disciplining mechanism in two ways. The most direct way is that citizens can punish an underperforming incumbent by not reelecting her. The second is more subtle and it has to do with the fact that economic performance of the district serves as a signal of the mayor's competence, which is in turn rewarded with reelection[4]. This restrains the mayor's rent-seeking behavior, because appropriation of rents could compromise the district's economic performance.

Brollo et al. (2013) developed an extension of the *career concerns* model in which the agency problem can be further enhanced by a negative selection effect that worsens the pool of opponents to incumbent politicians. The present work draws from this recent contribution.

[3]It is an agency problem assuming there is a dilemma for the elected official between acting upon her best interest or her constituents'. An example is the dilemma between appropriating resources illegally or using the resources to provide public goods. The dilemma could be more simple, such as choosing the level of effort she will exert in order to provide good public services, knowing that the principal (electors) are not able to observe what she is actually doing.

[4]This is usually referred to as the *career concerns model*

The model's predictions can be summarized as follows. First, the agency problem is enhanced by the availability of an exogenous increase of non-tax revenue for the district. This is so because in a context of abundance the mayor has leeway to appropriate more rents for herself without compromising her capacity to attend her constituents' demands (i.e. electoral punishment decreases as windfalls increase). Second, if rents are more valuable to the relatively less skilled, then a *selection effect* worsens the pool of candidates attracted to public office. This, in turn, magnifies the agency problem because the incumbent faces a weaker competition and her reappointment chances are unchanged or even improved, even if she is appropriating more illegal rents. The remainder of this section presents the set up of the formal model, as developed in Brollo et al. (2013) and discusses its main theoretical predictions. Every hypothesis' plausibility is assessed in the light of the institutional context we are analyzing.

2.1 The Model

Consider two periods; one $(t = 1)$ in which the incumbent sets policy and elections are held, and another $(t = 2)$ in which the elected mayor sets policy once more. In each period the district receives a budget of size τ which can be allocated, alternatively, to r_t (rents for the mayor) or g_t (public good for the citizens). Competent mayors can provide the same level of public good provision (in terms of voters' utility) at a lower resource cost. In particular:

$$g_t = \theta(\tau - r_t) \tag{1}$$

where θ reflects individual competence in providing the public good. A higher θ is equivalent to a lower cost of providing any given level of public good and therefore a high level of θ characterizes a more competent mayor. Voters can not observe the politicians' competence but they can observe their type J, which is an observable characteristic that serves as a predictor of competence. The realization of θ is drawn from two alternative distributions which depend on

J. Both have the same (uniform) density, but different means as shown in the following equation:

$$Mean\ of\ \theta = 1 + \sigma^J, \quad J = H, L \tag{2}$$

$$\sigma^H = \sigma = -\sigma^L$$

with σ a known parameter. Thus, the expected value of competence of an individual when she is of type $H, E[\theta|J = H]$ is higher than that of an individual whose type is $L, [\theta|J = L]$. In Brollo et.al (2013) the type is approximated using the years of schooling of the candidate and whether or not she has a college degree. The "type" should satisfy two conditions. First, it should be highly correlated with individual competence; and, second, it should be actually *used* by the voters as a shortcut to infer it. In Peruvian subnational elections, it is quite uncommon that a candidate appeals to voters by advertising their academic credentials. A much more valued characteristic seems to be experience. This is why in the empirical analysis we will have two alternative measures of the candidate's "type", the standard one related to education, and a second one related to work experience.

Returning to the set up of the model, the incumbent's and the opponents' types are known to everyone before the elections but the actual competence of any candidate is known only if elected. Also, voters observe their own utility in terms of the provision of the public good g_1 but don't observe rents r_1.

There is a risk for the incumbent of being caught appropriating rents illegally, which would cause her a utility loss of λ^J. An important assumption is that the utility loss is higher for a high-quality (type H) mayor (i.e. $\lambda^H > \lambda^L > 0$). This is meant to capture the fact that a high-quality mayor has a more valuable *outside option* in the private sector.

Let $d(r_t) = qr_t$ be the probability that a corrupt mayor is caught by an official audit. Then the expected utility of a mayor of type J who is in office in periods 2 and 1 would be:

$$V_2^J = r_2 - \lambda^J q r_2 + R \tag{3}$$

$$V_1^J = r_1 - \lambda^J q r_1 + R + P^J V_2^J \tag{4}$$

where R are exogenous "ego rents" that enter the utility function of politicians in any standard model of political economy and P^J is the probability of reelection (as perceived by the incumbent in period 1, when she decides how much r_1 is optimal). Rearranging the first two terms in equations 3 and 4 (specifically, factoring out r_2), yields $(1 - \lambda^J q)r_2$. Therefore, $(1 - \lambda^J q)$ captures the value of rents for a mayor of type J. Because of the previous assumption about the differential utility loss depending on type J ($\lambda^H > \lambda^L$), the expected value of rents is higher for low quality mayors. The utility of voters is simply: $W_t = g_t$.

I outline here the timing of events just as they are presented in the model developed by Brollo et al. 2013. At the beginning of period 1, the mayor sets r_1 (the amount of rents to appropriate). At this point, she knows her own type ($HorL$) but not yet her own competence (θ). She does not know her future opponent's type either. Consistent with previous notation, the expected quality of the opponent will be: $\hat{\sigma} = \pi\sigma^H + (1-\pi)\sigma^L$. An important contribution of the cited work is that the model allows for π (the probability of an opponent being of type H, or, more simply, the fraction of opponents of type H) to be endogenous.

Next, the type of the opponent (but not her competence) becomes known to all and elections are held. Voters observe: g_1, the incumbent's type J and the opponent's type J. They don't observe r_1. Then, audits happen and corrupt mayors may get caught. In period 2 the elected mayor sets r_2 and a final audit takes place[5].

[5]It is important to note that the model set up is consistent with a particular institutional setting where random audits take place. Because I focus on a different institutional setting, there are some mechanisms that I am not able to test meticulously, like the effect of the disclosure of corruption, for example. This will be explained in more detail in the following sections.

2.2 Theoretical predictions

Throughout this section, by rents I refer to public resources illegally appropriated by the mayor. In the empirical section, however, the outcome variable will be dichotomous (whether the mayor is involved in corruption or not). 'Rents' serves just as a synonym of corruption in this section. The predictions are obtained when solving for the equilibrium of the model and all derivations can be found in the Online Appendix of Brollo et al. 2013[6].

Prediction 1. The electoral punishment for corruption ($\frac{\partial P^J}{\partial r_1}$) becomes smaller in absolute value as the level of transfers rises (Part 1). Because of this, rents are an increasing function of transfers: $\frac{\partial r_1^J}{\partial \tau} > 0$ (Part 2).

Voters base their inferences of the mayor's ability on the observed outcomes (how the incumbent satisfies certain expectations of public good provision, in this model). Increased windfalls would leave room to appropriate part of them and still satisfy such expectations. The inference is therefore distorted and voters will not (effectively) discipline mayors' behavior through electoral choices.

Prediction 2. Rents are a decreasing function of the quality of the incumbent (Part 1) and of the expected quality of the opponent (Part 2).

The intuition behind this is that a high quality incumbent faces a larger penalty for being discovered involved in corruption scandals because she would lose relatively more profitable opportunities in the private sector. The second part of this prediction is related to the fact that a tougher competition for the incumbent increases her incentives to please voters (by increasing the allocation of resources to public goods, thus reducing the rents she grabs).

Prediction 3. The effect of budget size on rents (positive, according to *Prediction 1*) is smaller the higher is the expected quality of the pool of opponents.

In other words, the effect stated in prediction 1 is attenuated if the incumbent expects an upgraded pool of opponents. This, however, is usually not the case. Because of other considerations, the model predicts that the quality of the pool tends to deteriorate simultaneously, resulting in an even worse outcome. So

[6] Stable link here.

far we have described the aggravated moral hazard effect, taking the composition of the pool of candidates as given. Allowing for endogenous entry of candidates (i.e. π is endogenously determined), two additional predictions hold.

Prediction 4: The quality of the pool of candidates is a decreasing function of transfers.

In terms of the model set up: $\frac{\partial \pi}{\partial \tau} > 0$ (the share of low quality candidates increases when transfers increase). This is explained because the value of rents is higher for low quality mayors as opposed to high quality mayors (see previous subsection). This is connected to the following prediction.

Prediction 5: The incumbent is more likely to be reelected when transfers are higher, $\frac{\partial P^J}{\partial \tau} > 0$.

The rationale for this is that as the pool of challengers becomes worse, voters have a lower bar to reappoint an incumbent mayor.

Finally, taking into account all the channels of impact (moral hazard effect plus the aggravating selection effect):

Prediction 6. The overall effect of transfers on rents is positive.

There is one caveat to point out about prediction 1, and consequently, prediction 6. Anectodal evidence by Arellano (2011), reported that in mining areas citizens are not necessarily lethargic in the presence of abundant resources. In contrast, they sometimes push for a quick (though not necessarily efficient) expenditure of resources, fearing that authorities may benefit from them before citizens can reap the benefits. I consider this is a relevant observation and to keep the adaptation of the model in line with our present setting, I will also consider in the empirical section an alternative estimation where the effect of windfalls can be non-monotonic. In other words, it could be the case that up to some point of windfalls the effect on corruption is positive, but such effect decays and could even become negative past certain threshold where windfalls are *extremely* high.

2.3 Competing theoretical frameworks

There are some challenges to this theoretical approach that are worth addressing before continuing to the empirical section.

The first one is that this model abstracts from the first stated role of elections, namely, aggregating heterogeneous preferences. In that sense, it leaves out, for example, redistribution and other aspects in which conflicting interests among voters arise, and focuses exclusively on the accountability role of elections, which is in the common interest of voters.

This is questionable but plausible in a context in which party labels are not strong [7]. Also, it could be argued that, if this role is important (aggregating preferences), then it is likely to weaken the other role (accountability) because voters would face a trade off between favoring their preferences and effectively disciplining politicians (Persson et al. 1997).

There is another challenge, much more fundamental: it could be the case that voters are not even concerned about holding politicians accountable, but are rather interested in benefiting from their illegal activities [8]. In this case, a more approriate framework would be one of patronage.

Another important aspect that could be questioned is the nature of the disciplining mechanism. In the adopted model, rent-seeking by incumbents is restricted by the need to signal competence to voters. A plausible deviation from this would be that reelection does not depend simply on incumbents pleasing voters but rather on pleasing them to the point in which they can deter challengers.

The work by Caselli (2006) is an example of this approach. The incumbent politician, in the context of an increased budget, must decide how much will be assigned to public goods and how much to appropriated rents. In order to avoid competition, the incumbent provides more g to create an environment favorable

[7] In 2010, for example, out of 982 authorities running for reelection, only 375 (38%) were doing so under the same party label with which they were elected in 2006.

[8] This could happen, for example, because after repeated elections, voters come to the conclusion that politics are intrinsically corrupt, and therefore what they maximize is their probability of benefiting from it. An anecdotal evidence favoring this view would be the former governor of Moquegua, who admitted in front of a public audience "I indeed stole, but I shared with all of you"

to investment, so that the opportunity cost (for entrepreneurs) of running is higher. However, if windfalls continue to increase, the political value of holding office may be so high that it is not possible to deter entry, and the mayor will dedicate to appropriate as much as she can during the current period.

This approach is interesting, but it demands finding a good measure of how the incumbent tries to make the private sector more attractive. Using just the level of investment on public goods might be a very loose definition. This falls outside the scope of this work. Something interesting about the cited work, however, is that it introduces the notion that effects of windfalls on corruption may be non-monotonic. This was already mentioned and will be revisited in following sections.

3 Institutional Background and Data

Having completed the detailed description of the theoretical framework, I proceed to describe the Institutional Background in which I intend to put the relevant hypotheses to test. As pointed out in the introduction, Peru experienced an accelerated increase in its mining-related revenue because of the rise in the prices of gold, copper, zinc, nickel and tin. Figure (3) in the Appendix shows the evolution of prices. A relevant feature of this commodity boom that distinguishes it from previous ones is the degree of redistribution that took place. As stated by the *Mining Canon Law* of 2001, after corporate tax is collected from mining companies, 50% of this revenue has to return to producing areas. The distribution rule is as follows:

- 10% is transferred to the Municipality of the producing district
- 25% is distributed among the districts in the producing province
- 40% is distributed among the districts in the producing department
- 25% is transferred to the Regional Government, and out of this 20% (i.e. 5% of total) is earmarked for Regional Universities

Table 1: Per capita *canon* by type of district and electoral cycle

	2002		2006		2010	
	Mean	N	Mean	N	Mean	N
Producing district	11.48	67	204.58	89	959.41	107
Non-producer, in producing prov.	8.48	454	106.57	546	461.38	596
Non-producer, in producing dept.	12.24	941	47.79	829	198.13	672
Districts in non-producing dept.	0.24	350	0.46	342	10.19	421

Source: Own based on SIAF (Ministry of Economy and Finance). Mean for the last row should be zero, but is different from zero because districts in Apurimac which do receive canon are coded as being in a non-producer department. Further checks must be made into this but this does not compromise the empirical analysis because data on transfers was retrieved from SIAF and is complete for every district (including Apurimac).

The most important mineral deposits are located in the highlands of Peru; but with redistribution happening as explained before, most districts in the coast received important resources too (because many departments lie on both geographical regions at the same time). The easternmost part of Peru, however, has been largely unaffected in terms of mining transfers. Table 1 presents the mean of canon per capita received by each group of districts: producing districts, non-producing districts in producing provinces, non-producing districts in producing departments and districts in non-producing departments. We can see that, on average, canon transfers to municipalities increased in more than 3400% from 2002 to 2010.

Figures (6) through (8) in the Appendix present another depiction of the spectacular increase in transfers. This is, furthermore, relevant for the validity of my identification strategy because it shows the geographical variation in the amount of canon received. Two contiguous districts, similar in almost every observable aspect, can receive significantly different amounts of transfers, just because of the assignment rule.

Another salient but not so positive feature of municipalities in Peru is that they tend to be the setting of most corruption crimes. According to a recent report by the PPEDC, one in every four Peruvian districts has a former mayor being investigated for collusion, illicit enrichment, embezzlement, or related charges

(PPEDC, 2017). In 2012, they were the second most frequent setting for corruption crimes (13%), only after ministries of the executive (PPEDC 2012). In 2014, the share of cases prosecuted originating from district municipalities had increased from 13% to 38% (Arbizu, 2014). As noted by the former head of the PPEDC, the combination of administrative autonomy and significant mining-related fiscal transfers generates a context in which it is highly likely that authorities use public resources for their personal interest (idem).

These observations and the recent studies for our neighboring countries in part motivate this paper. The necessary data to carry out the analysis came from three main sources: the National Jury of Elections, the Ministry of Economy and Finance, and the Public Prosecutors' Office Specialized in Corruption Crimes.

The data on election outcomes was provided by the National Jury of Elections (JNE). This includes the list of candidates and the electoral results of every district for years 2002, 2006, 2010, and 2014[9]. Using this source of data, I constructed a district-level panel of four periods, and I was able to create indicators of 1) whether the incumbent was running in the election, and 2) whether the incumbent won the election. The incumbency indicators were constructed only for 2006, 2010 and 2014. Doing so for 2002 would have required access to data from 1998, which is not publicly available. An extensive description of this and other electoral outcomes can be found in tables (8) through (10) in the Appendix.

The variables used to measure the quality of opponents to the incumbent are: the average years of work experience of the pool of opponents, the average years of education of the pool of opponents, and the fraction of opponents with higher education. All of them were calculated using the resumes that candidates presented in order to run in the municipal elections of 2006, 2010 and 2014. Prior to this, it was not mandatory to present a resume. The data were also provided by the National Jury of Elections (JNE). Each resume includes personal information (age, place of birth, time of residence in the district), educational information, work experience records, history of party militancy, and (only for 2010) a self

[9]For every election, around 2-3% of districts have missing data. Details can be found in the Appendix.

reported approximation of annual income and assets' value. Although there were some changes in the resume template that candidates filled out every election, the information is still comparable.

Calculating the average years of work experience of the pool of opponents was relatively straightforward. Calculating the fraction of opponents with higher education, on the other hand, posed a challenge. Many candidates report in the section of "Higher education" any educational activity after high school, many of which don't strictly qualify as higher education. For this reason, a set of rules was used to define what higher education was. They are summarized here:

- Type of education: University & "Titulacion/grado/egresado" & Length of activity $\geq$ 4 years
- Type of education: Technical & "Titulación/grado/egresado" & Length of activity $\geq$ 2.5 years
- Type of education: Masters & Length of activity $\geq$ 9 months

Finally, the total years of education were calculated as the sum of years reported under basic education and higher education (having filtered out those activities that did not qualify). It is important to note that, in line with the theoretical model, the three variables refer to the pool of *opponents to the incumbent* and therefore, are only meaningful in districts where there is an incumbent running[10]. The descriptives in the Appendix show that around 60% of incumbents run for reelection every period. Therefore, restricting the sample to those districts still leaves us with an important part of the universe[11].

The district level data on *canon* transfers comes from SIAF, the Ministry of Economy's repository of public finances data. Specifically, the information gathered is the amount listed under "*Monto acreditado*" for item "*Canon Minero*", every

[10] Also, by definition, they are calculated excluding the incumbent.

[11] Some additional estimations that do not conform to the model but can provide suggestive additional evidence could be performed using all districts. In that case, the variables used would be the average years of work experience of the pool of *candidates*, and the fraction of *candidates* with higher education. Emphasis is on candidates to remind that these variables are meaningful for all districts, regardless of an incumbent deciding to run. This falls outside the scope of the present work, but is part of a future researach agenda.

year from 1998 to 2016. These amounts are converted to constant *soles* of 2009 using the official Central Bank Price Index. Per capita measures are constructed using population estimates from INEI (Peru's National Institute of Statistics). Using data from Peru's Ministry of Energy and Mines I am able to identify which districts have active producing locations in each electoral period. As expected, these districts received amounts of per capita *canon* transfers that were, on average, four times those received by the rest of districts, at the peak of the commodity boom.

In the model discussed, there is a moral hazard effect of higher contemporaneous windfalls and a selection effect that aggravates the moral hazard problem (because mayors face weaker competition). In that sense, and considering that there is a time dimension in the data, corruption is affected by contemporaneous windfalls and by past windfalls that have deteriorated the current pool of politicians. The empirical tests that aim at disentangling moral hazard from the selection effect require a careful consideration of the timing of events.

For this reason, I construct two measures of per capita *canon*, and I use the one that is relevant in each estimation. The first measure is the average canon transfers received by the district in the four years following the election. For 2002, it would be an average of 2003, 2004, 2005 and 2006 transfers, for example. Elected officials assume their post in January of the year following the election, so this would be a measure of the windfalls available during their time as mayors. I use this measure of canon to test part 2 of hypothesis 1 (that windfalls increase the incidence of corruption), and hypothesis 3 (that this effect is attenuated if mayors face tougher competition), because they both have to do with the moral hazard effect.

The second measure considered is the average canon transfers received in the four year period leading to the election. For example, in 2010, it is an average of transfers received in 2007, 2008, 2009 and 2010. This measure is useful when testing hypothesis 4, which has to do with a worsening pool of opponents. The reason is that the quality of the pool of opponents that are drawn to challenge

the incumbent in year t (or, more generally, the quality of candidates attracted to compete in electoral year t) is likely influenced by the expected windfalls to be received during the years following t. Our assumption is that the best prediction that potential candidates can make about those future transfers is based on the transfers that were received by the district in the most recent years.

The test of hypothesis 5, related to the probability of reelection of the incumbent, also makes use of this lagged measure of canon transfers because —just like the quality of opponents—reelection is an outcome that happens in year t (unlike corruption, which happens during the mayoral term starting in $t+1$).

Hypothesis 6 is the final prediction relating windfalls and rents, once all channels of impact (moral hazard, selection effects) have been taken into account. A tentative specification of this hypothesis includes both the contemporaneous and the lagged measure of per capita *canon*. This is detailed further in Section 5.

The data on corruption comes from the PPEDC. The category corruption charges includes crimes committed against public administration such as collusion, embezzlement, abuse of authority, among others. There is an important caveat to point out about this source. The PPEDC only discloses information about processes that already have a definitive sentence. Therefore, we only have information about mayors/candidates with a sentence for corruption, but not about mayors/candidates with corruption charges more generally. This is a limitation of our corruption measure because given the sluggishness of the Peruvian judicial system, only a minority of cases get a definitive sentence (condemnatory or absolutory) in a reasonable time.

This potentially generates a bias in our corruption measure because, given how our judicial system works, it is possible that the most corrupt individuals manage to get away without a sentence. The future research agenda includes 1) complementing this information with news scraped from regional sources, 2) changing the indicator of corruption to be 1 not only if the mayor is corrupt but also if her "front men" are.

4 Methodology

For estimating the effect of canon transfers on the aforementioned political outcomes, I use a difference-in-differences approach. Mining transfers from the central government to the local municipalities would constitute the "treatment". The source of exogenous time variation in these transfers, I argue, is the spike in international prices of minerals that started in 2003. Given that Peru, in spite of being an important producer, is a price-taker in the international market of mineral commodities (see arrow 1 in Figure (1)), it is plausible to treat the price boom as a valid source of exogeneity explaining the large increase of fiscal windfalls. Figure (3) in the Appendix depicts the evolution of international prices of the most important minerals in Peru's export bundle.

A potential endogeneity concern is that the rise in prices eventually affects production decisions, which in turn affect local economic activity and can directly cause corruption to increase. Then, we would have both transfers and corruption jointly determined by a "production effect" (see arrows 2 and 3 in Figure (1)) and we would not be able to isolate the "rentist effect".

Figure 1: Identification Strategy

However, as argued by Ardanaz and Maldonado (2016), changes in pro-

duction take a relatively long time. The expansion of installed capacity in response to international prices occurs with a lag of about 7 years, while the effect in transfers is almost immediate. Our outcomes are measured four years apart, and not so long after the commodity boom began. For this reason, I argue that in the short term the causal effect is correctly identified, once we control for year fixed effects (time trends) and district fixed effects (unobservable but constant heterogeneity). However, the potential of production as a confounding variable is not negligible and therefore we replicate every estimation excluding producing districts, since they are relatively few (around 5% of districts) and their exclusion from the sample does not compromise statistical power. It is important to mention that the redistribution of canon resources usually happens one-and-a-half years after revenues of a given fiscal period are collected. Therefore, if the international boom of mineral prices occurred between 2003 and 2012, we witness the associated increase in transfers approximately between 2005 and 2014. It is also noteworthy that even though the boom started in 2003, the prices began to skyrocket in 2005, and there was a corresponding steep increase in transfers in 2007. For these reasons, 2006 was chosen as the appropriate baseline year.

Geographical variation in the amount of mining transfers received comes from the redistribution rule that was detailed in the preceding section. An additional aspect to comment about this pattern is that, as shown in figures (6) through (8), there are some departments that are almost entirely unaffected by the increase in mining transfers: Callao, Loreto, Tumbes, and Ucayali. This means that there is very little spatial variation within these departments, which is important for the validity of the identification strategy. Therefore, every estimation will be replicated excluding them.

Now I turn to the specific steps followed to test the predictions of the theoretical section. I first restate the predictions of the model in terms of our institutional background. Then, I describe the estimations that are feasible with the available data.

The first hypothesis is that there is a positive effect of mining windfalls on

the probability that a mayor is involved in corruption (part 2 of *Prediction 1*, and *Prediction 6*). The specification to test this assertion is as follows:

$$y_{it} = \beta_0 + \beta_1 Canon_{it} + \gamma_i + \delta_t + \epsilon_{it} \tag{5}$$

where y_{it} is an indicator of whether the mayor was sentenced for corruption, $Canon_{it}$ is the logarithm of canon per capita received by district i in the electoral cycle starting after year t [12], γ_i is a district fixed effect, δ_t is a year fixed effect, and ϵ_{it} is an error term. The outcome y_{it} in equation 5 is equal to 1 if three conditions hold: 1) the mayor was sentenced for corruption, 2) the aggravated entity was the municipality where she served, and 3) the date in which her first municipality-related judicial file was opened is posterior to election year t. This is not trivial because 1) many mayors are sued for corruption but few of these cases actually make it past the investigative stage, let alone the definitive sentence; 2) the theory that is being discussed has to do with a deteriorating local government, therefore, the relevant crimes are not just any kind of crime but rather those that affect the municipality directly, and 3) some mayors may have a past criminal record but this test in particular has to do with corruption deeds once she becomes the mayor.

Ideally, criteria number 3 would be not only that his/her first file was opened after the election, but that the crime itself happened after the election, and during that particular term (so, before the next election year). However, the available data does not allow for this variable to be constructed in such way. The only date that is available for every case in the PPEDC data is the date embedded in the name of the judicial file *(Expediente judicial)*, which coincides with the year in which the case surpasses the investigative stage and the trial begins[13]. The date of the sentence is also available for most cases. The date of the crime itself,

[12] As explained in the previous section, elected officials assume their post in January of the year following the elections and hold office for four years. So, $canon_{it}$, for 2002, is an average of per capita canon transfers received in 2003, 2004, 2005 and 2006; for 2006, it is an average of 2007, 2008, 2009 and 2010; and for 2010, an average of 2011, 2012, and 2013.

[13] In local jargon, it is the year in which a case with a *Carpeta Fiscal* is deemed worthy of an *Expediente judicial*.

however, is never available. This is indeed a limitation of the data, and it required some working assumptions.

Implicitly, I am assuming that if the file was opened during a mayoral term, the crime happened during that mayoral term. This may sound questionable because the corruption deed could be from a previous term in which the mayor had been in office. However, in 73% of cases in which y_{it} equals one, the mayor is in his/her first term ever as mayor. That means that the municipality-related corruption file being opened can not be from a previous term. Another figure to take into account is that in 2017, 28% of current[14] mayors were being prosecuted for corruption (PPEDC, 2018). This suggests that it is rather common that corruption crimes by authorities start to be investigated while they are still in office.

The rest of hypotheses have to do with the mechanisms that would explain the overall effect of canon transfers on corruption; namely: a decreased electoral punishment of corruption (part 1 of *Prediction 1*), a worsening pool of opponents for the incumbents who run for reelection (*Prediction 4*), and a resulting increased probability of reelection as a function of transfers (*Prediction 5*).

An empirical test of the first mechanism (decreased electoral punishment of corruption) is specified in Equation 6. R_{it} is the reelection outcome (0/1), y_{it} is 1 if the incumbent seeking reelection comitted a corruption crime, and $before_{it}$ is 1 if corruption was disclosed before this reelection attempt. If there is a punishment for corruption, β_6 should be negative (indicating that a disclosed crime prior to the election lowers the chances of reelection), and if such punishment decreases when windfalls[15] increase, β_7 should be positive.

[14] Mayors that had been elected for the period 2015-2018

[15] In this case, the canon variable refers to the average of the years leading to the election (i.e. the second measure explained in the preceding section) because of the nature of the outcome that is being considered: it is an outcome whose realization occurs in the year of the election and not during the period of office.

$$
\begin{aligned}
R_{it} = \beta_0 + \beta_1 Canon_{it} + \beta_2 y_{it} + \beta_3 before_{it} \\
+ \beta_4 Canon_{it} \times y_{it} + \beta_5 Canon_{it} \times before_{it} + \beta_6 y_{it} \times before_{it} \\
+ \beta_7 Canon_{it} \times y_{it} \times before_{it} + \gamma_i + \delta_t + \epsilon_{it}
\end{aligned} \tag{6}
$$

With the available data, however, it is not feasible to test this assertion because we do not have precise information about the date of disclosure of the corruption deed. As was explained before, the only date that is available for every case is the judicial file date. Even if the three dates of interest were available simultaneously (crime, file opening, sentence), it is not clear which (if any) of them would represent the date when corruption was disclosed to citizens because they could become aware of it and make their own judgements at any moment between the corruption crime and the sentence. Defining which date is best as a proxy of disclosure date would require further qualitative work.

The empirical test of hypothesis 3 is an extension of the one for hypothesis 1 in the sense that it looks again into the effect of windfalls on corruption but seeks to assess if such effect is attenuated when the incumbent's challengers are of higher quality. See equation 7. As for equation 5, the expected sign of β_1 is positive, reflecting the positive effect of windfalls on corruption. Furthermore, if such effect is attenuated in the presence of higher quality opponents, then the coefficient accompanying the interaction of windfalls $\times$ quality of opponents, β_3, should be negative.

$$
\begin{aligned}
y_{it} = \beta_0 + \beta_1 Canon_{it} + \beta_2 ExpOpponents_{it} \\
+ \beta_3 Canon_{it} \times ExpOpponents_{it} + \gamma_i + \delta_t + \epsilon_{it}
\end{aligned} \tag{7}
$$

Finally, hypotheses 4 and 5 are testable and the specification is the same as in equation (5), except the outcome is a measure of the quality (J in the model) of the pool of opponents for hypothesis 4, and then the probability of reelection of the incumbent for hypothesis 5. These two estimations apply only for the sub-sample of districts where an incumbent mayor actually runs for reelection. Also,

in these two regressions the measure of canon employed is the average of canon transfers received by the district in the four years leading to the election.

To sum up all the hypotheses outlined so far, it is useful to see a timeline of the outcomes that enter the regressions. This is also helpful to explain how I restrict the sample for every estimation. To work within a single institutional framework, I only consider years after 2001, when the latest *Canon* Law was approved. For hypotheses 1 and 3, I include *electoral periods* starting after 2002, 2006, 2010 and 2014. There is one caveat to make about the post 2014 electoral period.

Because of its recency, there are too few sentences for mayors of that period. Table 2 shows the number of mayors sentenced by mayoral term. Unsurprisingly, the number of mayors sentenced is lower for 2010 and 2014 when compared to 2002 and 2006. More recent mayors are less likely to be sentenced not necessarily because incidence of corruption is lower in more recent terms, but rather because there has not been enough time for their cases to receive a sentence[16]. Because I am including year fixed effects, this is not a major concern; however, I replicate all estimations regarding hypotheses 1 and 3 excluding the post-2014 term.

Table 2: Number of mayors sentenced by term

Mayoral term	Not sentenced	Sentenced	Total
2003-2006	1702	114	1816
	(93.72%)	(6.29%)	(100.00%)
2007-2010	1704	106	1810
	(94.14%)	(5.86%)	(100.00%)
2011-2014	1749	51	1800
	(97.17%)	(2.83%)	(100.00%)
2015-2018	1806	5	1811
	(99.72%)	(0.28%)	(100.00%)

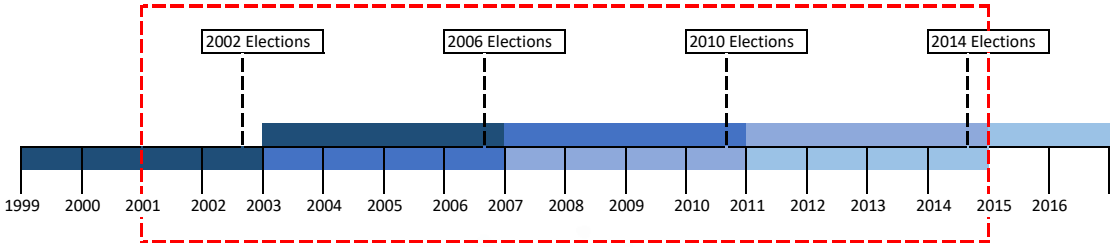
Source: PPEDC.

When testing hypotheses 4 and 5, I include *electoral years* 2006, 2010, and 2014 in the sample. Year 2002 can not be included in neither because of the

[16]This could be confirmed if there were available data on cases at each step of the judicial process, but such data is not public. As explained before, only cases with a definitive sentence are consolidated and delivered upon request.

data restrictions explained in the previous section.

Figure 2: Timeline of events



5 Results

The overall effect of windfalls on corruption was evaluated first, and the results are presented in Table 3. The outcome is a binary variable which equals 1 if the mayor of that term was sentenced for corruption[17].The first column shows results for all districts with available data, including mayoral terms starting after 2002, 2006, 2010 and 2014. The second column excludes districts in departments without significant geographic variation in *canon* transfers. As mentioned before, this is the preferred sample because using it is more consistent with the identification strategy. The third column shows results when I also exclude producing districts. Even though samples differ by column, the specification in the three of them refers to Equation 5, so the regressor of interest is the contemporaneous measure of *canon*.

Columns (4) through (6) in the same table repeat the estimations of (1) - (3) but in this case the regressor is the lagged measure of *canon* transfers. Anytime I use the lagged *canon* measure, I exclude 2002 because lagged *canon* for 2002 would include years before 2001, and that falls outside of the selected timeframe[18]. Finally, columns (7) through (9) present results when I include both the contemporaneous measure and the lagged measure in the same regression. All

[17]For more details on how this outcome was constructed, see section 3.
[18]These results are still positive and significant if I include 2002.

results in this Table (3) support the hypothesis that windfalls increase the incidence of corruption.

Because of the concerns discussed in section 4 regarding the too few sentences for mayors who served during 2015-2018, I repeat these estimations excluding the post 2014 mayoral term. Results are shown in Table 4. The direction and magnitude of effects remain very similar for every specification (with current canon, lagged canon or both) and sample (all districts, excluding those without geographic variation, excluding producing districts), compared to their equivalent in Table 3. However, effects are not significant for regressions in columns (1), (2) and (3) that employ the contemporaneous *canon* measure.

In the model, contemporaneous *canon* is expected to affect corruption through a moral hazard channel. Lagged canon is expected to affect corruption through a negative selection effect that aggravates the moral hazard problem. Because results for contemporaneous *canon* are sensitive to the exclusion of one electoral term, it is not possible to say anything conclusive regarding the moral hazard channel so far. However, there is a robust positive effect of lagged *canon* on corruption. Moreover, when including lagged and contemporaneous *canon* simultaneously in the regression (columns (7) through (9)), marginal effects for lagged canon reamain almost identical (compare (4), (5), (6) to (7), (8), (9)), and estimated marginal effects for contemporaneous canon are larger.

This suggests that the effect of contemporaneous windfalls on corruption might be conditional on the level of transfers received in the preceding electoral term. This could happen because windfalls from the preceding term affect the quality of candidates that are drawn to compete. This is consistent with results for hypothesis 4, which deals with selection effects more specifically.

Before moving on to the next set of results, it is important to emphasize that any time trend that there may be in our outcome of interest (e.g. a widespread increase/decrease of the incidence of corruption from one term to another, or the mechanical “recency bias” —fewer sentences for more recent terms—) is controlled for with the inclusion of year fixed effects. Furthermore, all regressions

include district fixed effects, to control for time-invariant unobservable characteristics of districts that might affect corruption.

Table 3: Hypothesis 1 - Effect of windfalls on corruption

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Canon cycle	0.010***	0.009***	0.009**				0.014***	0.013***	0.013**
	(0.004)	(0.004)	(0.004)				(0.005)	(0.005)	(0.005)
Canon previous cycle				0.017***	0.019***	0.020***	0.018***	0.019***	0.021***
				(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Observations	7,217	6,888	6,492	5,406	5,158	4,829	5,405	5,157	4,828
R-squared	0.026	0.026	0.025	0.033	0.035	0.035	0.036	0.037	0.037
Number of districts	1,839	1,749	1,707	1,835	1,750	1,694	1,834	1,749	1,693
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Exclude NoGeo	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Exclude Producers	No	No	Yes	No	No	Yes	No	No	Yes
Mean dependent variable	0.0382	0.0389	0.0393	0.0300	0.0308	0.0309	0.0300	0.0308	0.0309

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4: Hypothesis 1 - Effect of windfalls on corruption, excluding 2014

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Canon cycle	0.007	0.007	0.006				0.019*	0.019*	0.016
	(0.005)	(0.005)	(0.005)				(0.011)	(0.011)	(0.012)
Canon previous cycle				0.010*	0.012**	0.014**	0.013**	0.015**	0.016***
				(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Observations	5,414	5,168	4,905	3,602	3,437	3,241	3,602	3,437	3,241
R-squared	0.010	0.010	0.008	0.014	0.015	0.014	0.016	0.017	0.016
Number of districts	1,835	1,745	1,698	1,830	1,745	1,680	1,830	1,745	1,680
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Exclude NoGeo	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Exclude Producers	No	No	Yes	No	No	Yes	No	No	Yes
Mean dependent variable	0.0501	0.0509	0.0510	0.0436	0.0448	0.0444	0.0436	0.0448	0.0444

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

As mentioned in section 2, the relationship between windfalls and corruption could be non-monotonic. In particular, it could be the case that up to some level of *canon*, the effect on corruption is positive but after such point (so, when transfers are extremely high), the effect starts to decrease.

This could happen because when transfers reach a certain level that is *too* high, it may become more evident for citizens that the resources received surpass the spending capacity of the district, and they may become more vigilant of authorities.

The rationale for including a quadratic term has to do with the moral hazard

effect. Therefore, the results in Table 5 have linear and squared terms of contemporaneous *canon* as the variables of interest. The linear term has a positive significant effect on corruption, and the squared term has the negative significant coefficient that is consistent with a decreasing effect past some threshold of windfalls [19].

Table 5: Hypothesis 1 - Non-monotonic effects of windfalls on corruption

	(1)	(2)	(3)
VARIABLES			
Canon cycle	0.026***	0.026***	0.029***
	(0.007)	(0.007)	(0.007)
Canon cycle ^{2}	-0.002**	-0.002***	-0.003***
	(0.001)	(0.001)	(0.001)
year = 2006	-0.013	-0.011	-0.011
	(0.008)	(0.009)	(0.009)
year = 2010	-0.043***	-0.042***	-0.039***
	(0.008)	(0.009)	(0.009)
year = 2014	-0.064***	-0.063***	-0.063***
	(0.006)	(0.006)	(0.006)
Observations	7,217	6,888	6,492
R-squared	0.028	0.028	0.027
Number of districts	1,839	1,749	1,707
District FE	Yes	Yes	Yes
Exclude NoGeo	No	Yes	Yes
Exclude Producers	No	No	Yes
Mean dependent variable	0.0382	0.0389	0.0393

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

For testing hypothesis 3, I take the specification of equation 7 and estimate it for all districts where there is an incumbent running for reelection. Results are presented in Table 6 (including electoral periods that start after 2006, 2010 and 2014). In columns (1) through (3), the coefficients of interest are those corresponding to *Canon cycle* and to the interaction of *Canon cycle* with the average experience of the pool of opponents. Both have the expected sign (positive and negative, respectively), but they are not significant[20]. The previous finding

[19] To be consistent, I also replicate these estimations excluding the post 2014 mayoral term. Results are very similar and can be found in Table 11 in the Appendix.

[20] Results are replicated excluding the post 2014 mayoral term and results are presented in Table 12 of the Appendix. Again, coefficients are of the expected sign but not significant.

pointed to a non-monotonic effect of windfalls. Therefore, it is important to say that, building on that result, hypothesis number 3 was also evaluated including the second order polinomial specification of Table 5 plus the corresponding interactions. Again, coefficients had the expected sign, but were not significant. In summary, I do not find evidence supporting the theoretical prediction that a better pool of opponents attenuates the (positive) effect of windfalls on corruption.

Table 6: Hypothesis 3 - Differential effect of windfalls on corruption depending on quality of oppositors

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Canon cycle	0.0100	0.0093	0.0098				0.0126	0.0120	0.0173
	(0.0079)	(0.0084)	(0.0093)				(0.0110)	(0.0112)	(0.0128)
Avg. work exp. opponents	0.0012	0.0013	0.0016	0.0023	0.0025	0.0025	0.0025	0.0028	0.0032*
	(0.0014)	(0.0017)	(0.0017)	(0.0015)	(0.0017)	(0.0017)	(0.0016)	(0.0018)	(0.0018)
Canon cycle × Avg. exp opps	-0.0003	-0.0003	-0.0004				-0.0004	-0.0005	-0.0010
	(0.0004)	(0.0005)	(0.0005)				(0.0007)	(0.0007)	(0.0008)
year = 2010	-0.0329***	-0.0331***	-0.0303***	-0.0552***	-0.0579***	-0.0577***	-0.0560***	-0.0587***	-0.0590***
	(0.0091)	(0.0094)	(0.0095)	(0.0135)	(0.0144)	(0.0149)	(0.0136)	(0.0145)	(0.0150)
year = 2014	-0.0440***	-0.0463***	-0.0431***	-0.0684***	-0.0724***	-0.0703***	-0.0638***	-0.0682***	-0.0672***
	(0.0093)	(0.0099)	(0.0100)	(0.0133)	(0.0142)	(0.0143)	(0.0132)	(0.0143)	(0.0145)
Canon previous cycle				0.0219**	0.0233**	0.0265***	0.0199**	0.0210**	0.0211**
				(0.0091)	(0.0095)	(0.0095)	(0.0094)	(0.0096)	(0.0099)
Canon previous cycle × Avg. exp opps				-0.0006	-0.0006	-0.0007	-0.0002	-0.0002	0.0002
				(0.0004)	(0.0005)	(0.0005)	(0.0007)	(0.0007)	(0.0007)
Observations	3,273	3,109	2,916	3,273	3,109	2,916	3,273	3,109	2,916
R-squared	0.027	0.028	0.025	0.031	0.033	0.031	0.033	0.034	0.033
Number of districts	1,671	1,589	1,519	1,671	1,589	1,519	1,671	1,589	1,519
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Exclude NoGeo	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Exclude Producers	No	No	Yes	No	No	Yes	No	No	Yes
Mean dependent variable	0.0330	0.0338	0.0346	0.0330	0.0338	0.0346	0.0330	0.0338	0.0346

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Results for hypothesis 4; namely, that windfalls negatively affect the quality of the pool of opponents to the incumbent, are presented in Table 7. In each column, quality of opponents is proxied by a different variable: average years of work experience of the pool of opponents, average years of formal education, and fraction of opponents with formal higher education. Using the three different measures, the estimated effect of windfalls on the quality of opponents is consistently negative and significant. Specifically, an increase of per capita *canon* transfers equivalent to one standard deviation (S/713.00 *soles* from 2009[21]) causes a reduction of 1 year in the average work experience of the opponents to the incumbent, a reduction of 0.5 years in the average years of formal education of

[21] This is the standard deviation in the sample that entered this estimation: 3109 observations corresponding to 1589 districts × the number of elections when there was an incumbent running in the district (could be 1, 2 or 3).

opponents to the incumbent, and a reduction of 4.5 percentage points in the fraction of opponents that have completed formal higher education. The reported results are those obtained when excluding the departments without significant geographic variation (Loreto, Ucayali, Callao, and Tumbes). This is the preferred sample to be consistent with the identification strategy as explained before. However, results are extremely similar (and also significant) when considering the full sample and/or excluding producing districts. These results point to a large effect of windfalls on the quality of politicians attracted to compete for public office.

Table 7: Hypothesis 4 - Quality of opponents to the incumbent

	(1)	(2)	(3)
VARIABLES	Work experience	Educ. years	% with Higher Ed
Canon previous cycle	-0.410**	-0.240**	-0.020**
	(0.169)	(0.099)	(0.008)
year = 2010	6.111***	2.885***	0.118***
	(0.304)	(0.192)	(0.016)
year = 2014	5.732***	2.756***	0.185***
	(0.296)	(0.180)	(0.015)
Observations	3,109	3,109	3,109
R-squared	0.317	0.249	0.132
Number of districts	1,589	1,589	1,589
District FE	Yes	Yes	Yes
Exclude NoGeo	Yes	Yes	Yes
Exclude Producers	No	No	No
Mean dependent variable	10.30	12.39	0.406

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Finally, I present results regarding the effect of windfalls on the probability of reelection. I disaggregate this into three estimations. To save on space, Tables summarizing these findings are in the Appendix. In Table 13 the outcome is whether the incumbent mayor decides to run for reelection. In columns (1) through (3), the specification only includes a linear term. In (4) through (6), a quadratic term is also included. All results point to a positive effect of windfalls on the probability that the incumbent runs for reelection. This makes sense because availability of larger resources makes holding public office more attractive. However, I find no significant effect of windfalls on the probability of being reelected, conditional on running, as can be seen in Table 14. Finally, I find an overall (in-

conditional) effect of windfalls on reelection that is positive and significant (not reported here). The previous results suggest that this is driven by more incumbents seeking reelection, rather than by more incumbents winning the reelection.

Before moving on to discussing results in the concluding section, there is one more hypothesis to consider, which is that corruption is a decreasing function of the quality of the incumbent and of the expected quality of the opponent. It is not easy to identify empirically such effect, in a way that could be credibly causal. The reason is that there is no source of exogenous variation in the candidates' education. However, I can provide descriptive evidence that corruption is indeed less prevalent among mayors with higuer levels of education. The probability of being sentenced for corruption is 1 percentage point lower (alternatively, 27% lower) in the group of mayors with formal higher education relative to the group of mayors withou such educational attainment. This difference is significant to the 5%. Furthermore, there is a negative correlation between corruption and years of formal education (significant to the 5% level); and also between corruption and years of work experience (significant to the 10% level). This is important because it is one of the pieces that gives credence to the theoretical model set up.

Conclusions

In this paper I discussed some political channels through which a natural resource curse may opperate. Drawing from the contributions by Brollo et al. 2013, I outline several hypotheses about the plausible effects of windfall resources on corruption and selection into politics.

Abundance of windfalls could increase the incidence of corruption because their presence aggravates the moral hazard problem that is assumed to exist between citizens and their elected authorities in standard political economy models. An increased budget means that officials can appropiate rents illegally without compromising their obligations with the electorate, thus distorting the inferences that citizens make about their authorities' competence. This, in turn, impairs the disciplining role of elections, in which, absent a windfall of money, corrupt mayors would be less likely to be reappointed.

This problem could be aggravated if lower-quality candidates are drawn to compete with the incumbent. The prediction is that this deterioration of the pool of opponents will indeed take place because political rents are more valuable for the relatively less-skilled. This is an assumption of the model presented, but is consistent with high-quality candidates having higher quality outside options in the private sector, that they would lose if they were caught appropriating rents illegally in the public sector.

The commodity boom that took place during the 2000s generated an unprecedented redistribution of fiscal resources to local governments in Peru. In this paper, I argued that the spike in prices, combined with the redistribution *canon* law approved in 2001, generate a suitable setting to test the hypotheses outlined above. With a difference in differences approach, I test empirically most of the implications stemming from the theoretical model.

To sum up, I find significant evidence of a positive overall effect of windfalls on corruption. I also find strong evidence of a sizeable effect of windfalls on the quality of opponents challenging incumbents. Of course, this second result is for the subsample of districts where there is a mayor running for reelection (around

60% of them). However, I find no significant evidence that better opponents attenuate (conversely, that worse opponents aggravate) the effect that windfalls have on corruption. In other words, I do not find evidence that the link between the two phenomena is the one proposed in the model. Considering it carefully, it is not all that surprising because such a link would imply that mayors are very sophisticated agents that are capable of foreseeing the quality of opponents they will face, and factor this into their decision of how much to give in to their rent-seeking behavior. This is not to say that there is not a link between moral hazard problems and selection problems. There could be other ways in which these two are related.

The biggest limitation in terms of data is definitely the one on corruption. One reason is that it does not allow to construct a continuous (let alone a normalized) measure of corruption. That would require having the amount of resources compromised in the illegal activity (or even better, the amount compromised as a fraction of total municipal budget, for example). However, this is not something systematically gathered or reported by the judicial authorities. It would be tempting to use the amount of civil reparation as a proxy, but rules for setting it are explicitly influenced by how mediatic the corruption crime is (in order to somehow monetize the "moral" damage inflicted to the institution). This decision, although understandable, renders the civil reparation unsuitable as a proxy for size of theft.

Another limitation of corruption data is that only sentences are disclosed. It would be ideal to have available information on the number of cases opened, not just those that make it to the final stage. Having such data would allow to confront other possible interpretations of the findings. In particular, it could be the case that the effect of windfalls on corruption is capturing not only a moral hazard effect but a mechanical "prosecution" effect. What I mean by this is that it could be that it is not only (or at all) that windfalls increase corruption, but rather that windfalls make prosecutors focus more heavily on those districts with more resources and therefore, the probability of *detection* goes up. Although this is a relevant concern in general, I think it is not that fundamental in the context of

Peru. The reason is that all the anecdotal evidence gathered by prosecutors and commented in their periodic reports (some of which are cited in this paper) points to a lack of possibilities for the PPEDC to act upon their own initiative. Multiple prosecutors comment that what should be a proactive agency to prosecute corruption strategically, ends up many times being just one more branch of standard judicial prosecution.

This paper leaves open a very interesting research agenda which should incorporate more complete data on corruption. Furthermore, the long-term consequences of the windfalls shock will require careful consideration of the "production effects" discussed as a concern in the empirical section. Also, it is necessary to take into account how corruption may become something that not just comes and goes with the mayor in office, but rather grows some roots into institutions in ways that are not visible by only looking at sentenced top-rank authorities.

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Appendix

Figure 3: Evolution of prices (BCR)

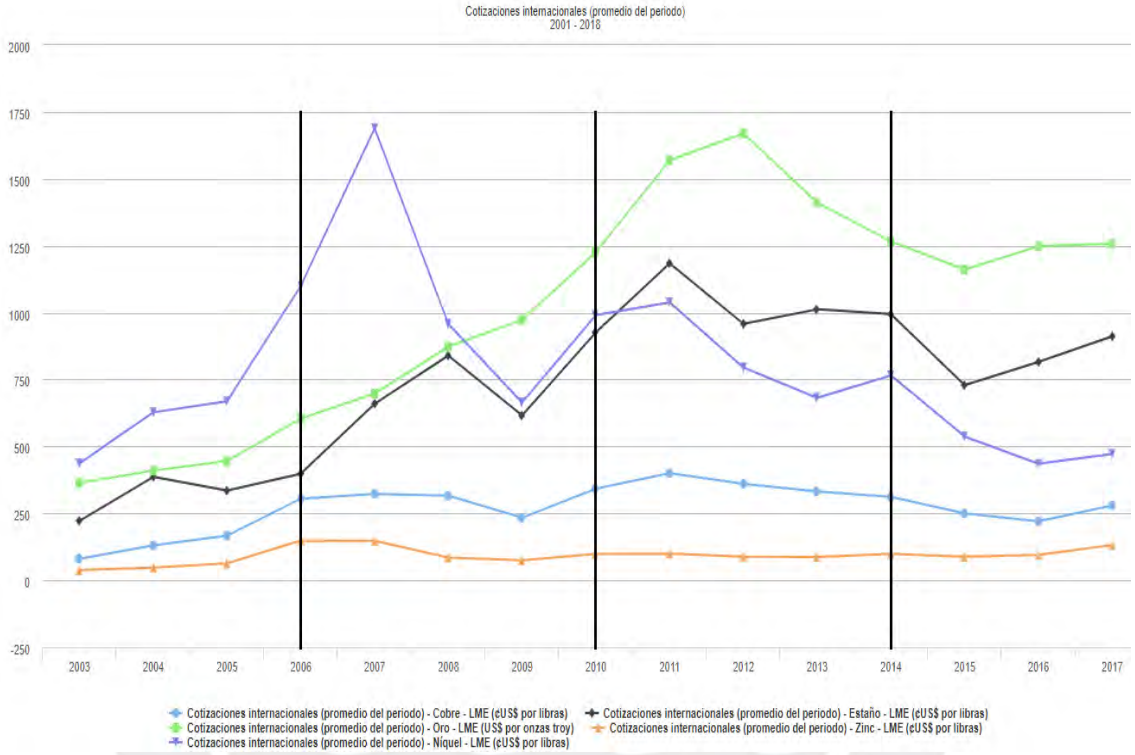

Figure 4: Evolution of canon transfers by district

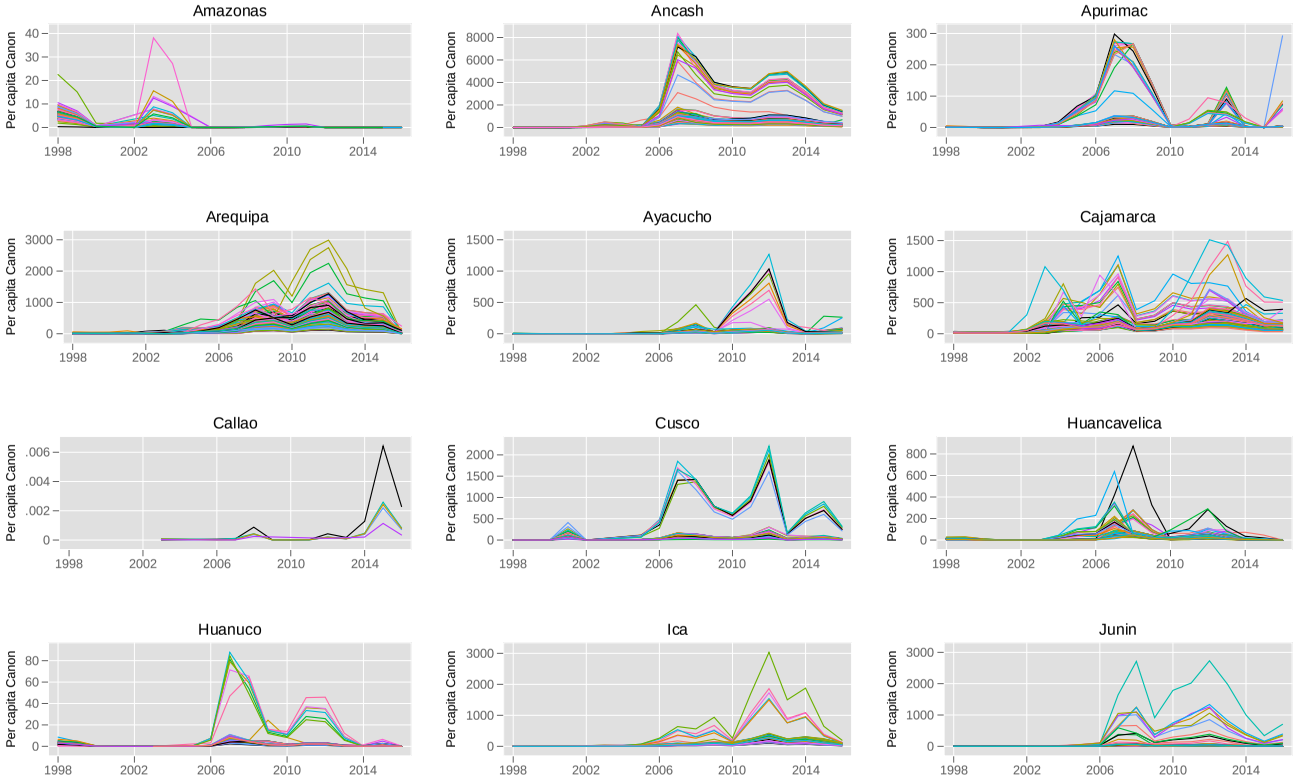

Figure 5: Evolution of canon transfers by district

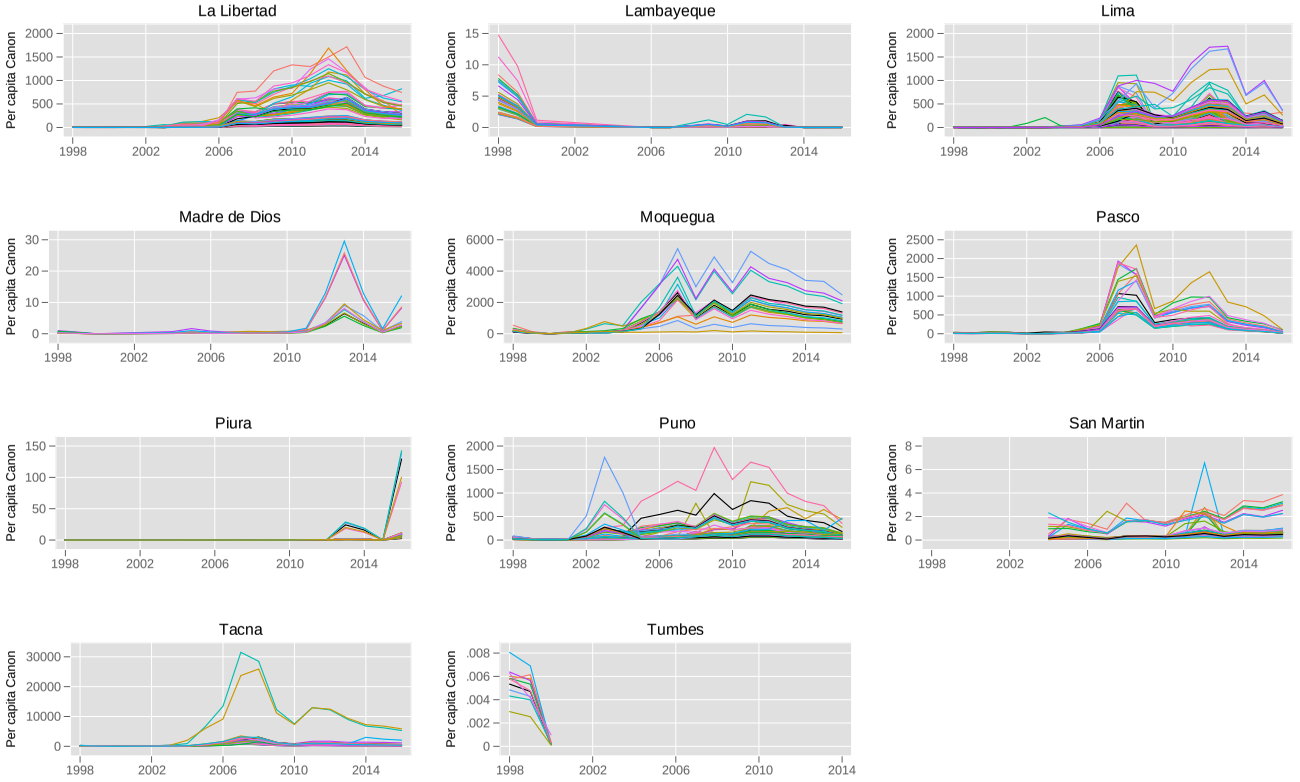

Figure 6: Canon transfers, 2003

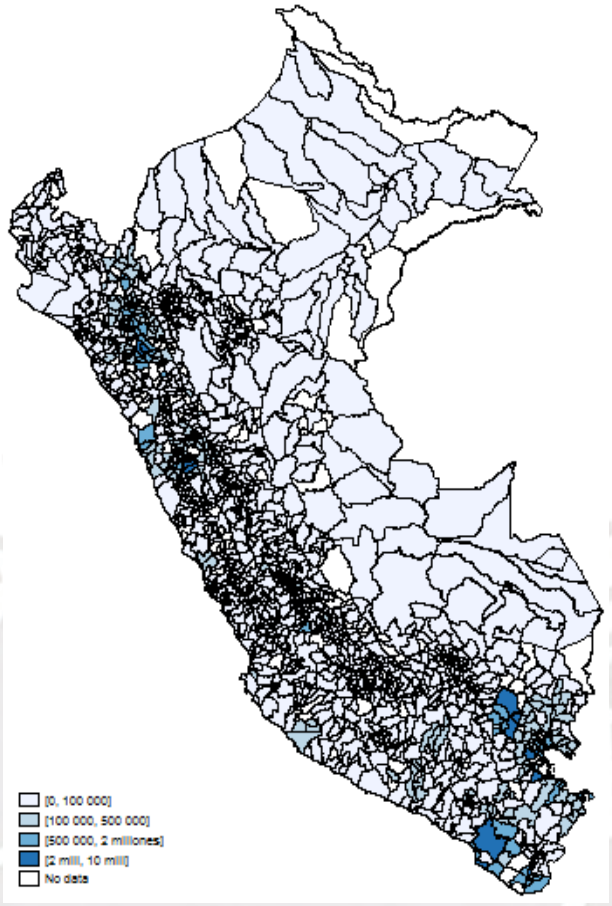

Figure 7: Canon transfers, 2005

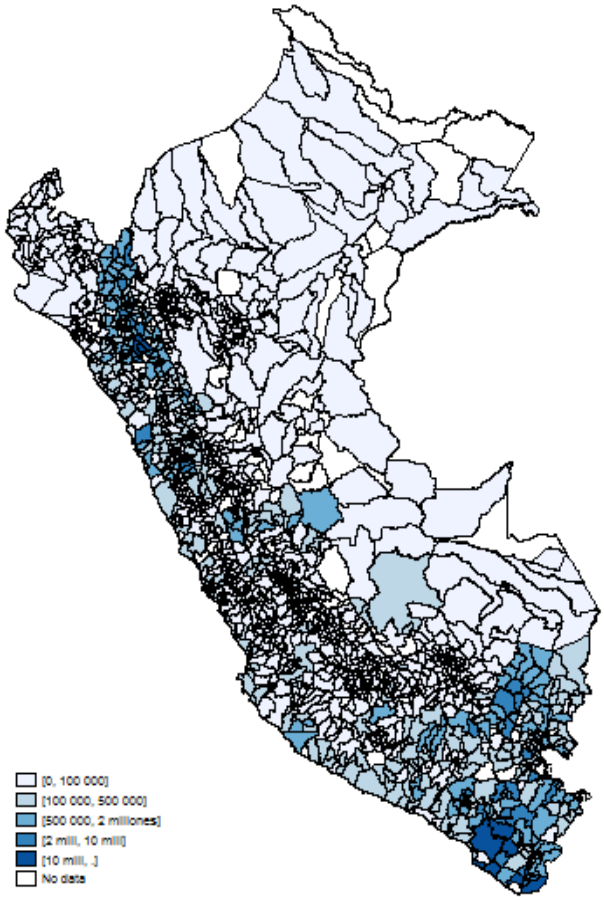

Figure 8: Canon transfers, 2007

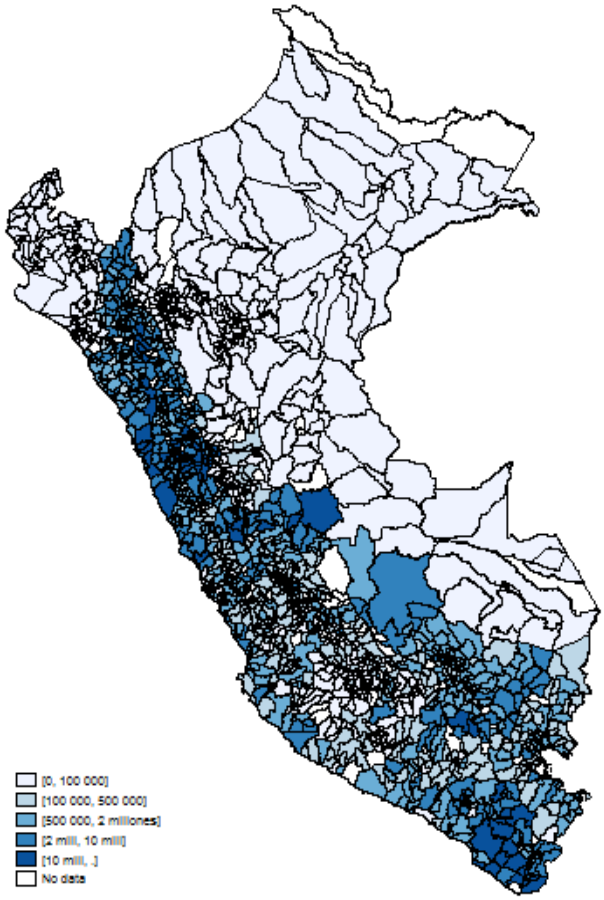

Table 8: Electoral competition 2002

Status	Canon per capita Soles per capita	Participacion electoral %	Margen de victoria %	Porc. Votos %	Movimiento regional %	Org Local (distrital) %	Org Local (provincial) %	Partido político %
Productores	6.4 57	80% 57	9% 57	30% 57	12% 57	7% 57	28% 57	46% 57
Receptores no productores	4.31 1469	81% 1469	8% 1469	31% 1469	13% 1469	7% 1469	19% 1469	52%
No receptores	0 243	82% 290	9% 290	31% 290	13% 290	13% 290	16% 290	52% 290
Total	3.78 1769	81% 1816	8% 1816	31% 1816	13% 1816	8% 1816	19% 1816	52% 1816

Table 9: Electoral competition - 2006

	Canon per capita	Participacion electoral	Margen de victoria	Porc. Votos	Incumbente postula	Reelección	Movimiento regional	Org Local (distrital)	Org Local (provincial)	Partido político
Productores	190.88	86%	8%	32%	57%	43%	24%	2%	10%	58%
	86	86	86	86	77	44	86	86	86	86
Receptores no productores	53.34	88%	9%	34%	64%	35%	29%	3%	7%	51%
	1611	1611	1611	1611	1443	921	1611	1611	1611	1611
No receptores	0	85%	9%	35%	52%	35%	42%	3%	4%	44%
	93	113	113	113	95	49	113	113	113	113
Total	57.18	87%	9%	34%	63%	35%	30%	3%	7%	51%
	1790	1810	1810	1810	1615	1014	1810	1810	1810	1810

Table 10: Electoral competition - 2010

	Canon per capita	Participación electoral	Margen de victoria	Porc. Votos	Incumbente postula	Reelección	Movimiento regional	Org Local (distrital)	Org Local (provincial)	Partido político
Productores	1026.71	85%	11%	36%	53%	48%	48%	2%	6%	38%
	90	90	90	90	90	48	90	90	90	90
Receptores no productores	252.07	86%	9%	35%	60%	33%	52%	2%	2%	35%
	1621	1621	1621	1621	1441	858	1621	1621	1621	1621
No receptores	0	82%	9%	36%	69%	29%	82%	2%	1%	12%
	76	89	89	89	74	51	89	89	89	89
Total	280.36	86%	9%	35%	60%	33%	53%	2%	2%	34%
	1787	1800	1800	1800	1605	957	1800	1800	1800	1800

Table 11: Hypothesis 1 - Non-monotonic effects - Excluding 2014

VARIABLES	(1)	(2)	(3)
Canon cycle	0.019*	0.018*	0.021**
	(0.010)	(0.010)	(0.010)
c.logcanon_pc_fw#c.logcanon_pc_fw	-0.002	-0.002	-0.002*
	(0.001)	(0.001)	(0.001)
year = 2006	-0.013	-0.010	-0.010
	(0.010)	(0.010)	(0.010)
year = 2010	-0.043***	-0.041***	-0.039***
	(0.010)	(0.010)	(0.010)
Observations	5,414	5,168	4,905
R-squared	0.010	0.010	0.009
Number of districts	1,835	1,745	1,698
District FE	Yes	Yes	Yes
Exclude NoGeo	No	Yes	Yes
Exclude Producers	No	No	Yes
Mean dependent variable	0.0501	0.0509	0.0510

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 12: Hypothesis 3 - Excluding 2014

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Canon PC Promedio ciclo	0.0312**	0.0317**	0.0362**				0.0472**	0.0487**	0.0606***
	(0.0155)	(0.0156)	(0.0166)				(0.0197)	(0.0199)	(0.0223)
Anios de experiencia laboral promedio de oponentes	0.0012	0.0016	0.0015	0.0031	0.0035	0.0038*	0.0040*	0.0047*	0.0052**
	(0.0019)	(0.0022)	(0.0022)	(0.0020)	(0.0022)	(0.0023)	(0.0022)	(0.0025)	(0.0025)
c.logcanon_pc_fw#c.exp_pool_opp	-0.0007	-0.0007	-0.0008				-0.0011	-0.0012	-0.0016
	(0.0005)	(0.0005)	(0.0006)				(0.0010)	(0.0011)	(0.0012)
year = 2010	-0.0245**	-0.0246**	-0.0214*	-0.0532***	-0.0554***	-0.0590***	-0.0615***	-0.0651***	-0.0703***
	(0.0111)	(0.0115)	(0.0117)	(0.0173)	(0.0186)	(0.0191)	(0.0185)	(0.0201)	(0.0204)
Canon PC Promedio ciclo anterior				0.0297**	0.0310**	0.0373***	0.0290**	0.0300**	0.0338**
				(0.0129)	(0.0134)	(0.0134)	(0.0146)	(0.0148)	(0.0154)
c.logcanon_pc#c.exp_pool_opp				-0.0012**	-0.0013**	-0.0014**	-0.0003	-0.0002	0.0001
				(0.0006)	(0.0006)	(0.0007)	(0.0011)	(0.0011)	(0.0013)
Observations	2,218	2,118	2,009	2,218	2,118	2,009	2,218	2,118	2,009
R-squared	0.023	0.024	0.023	0.027	0.028	0.030	0.034	0.035	0.040
Number of idaux	1,528	1,461	1,399	1,528	1,461	1,399	1,528	1,461	1,399

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 13: Hypothesis 5 - Probability of running for reelection

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Canon PC Promedio ciclo anterior	0.028**	0.040***	0.037***	0.081***	0.091***	0.104***
	(0.011)	(0.011)	(0.012)	(0.024)	(0.024)	(0.025)
c.logcanon_pc#c.logcanon_pc				-0.007**	-0.007**	-0.010***
				(0.003)	(0.003)	(0.003)
year = 2010	-0.071***	-0.097***	-0.092***	-0.067***	-0.093***	-0.086***
	(0.021)	(0.022)	(0.022)	(0.021)	(0.022)	(0.023)
year = 2014	-0.072***	-0.102***	-0.107***	-0.069***	-0.098***	-0.102***
	(0.019)	(0.020)	(0.020)	(0.019)	(0.020)	(0.020)
Observations	5,406	5,158	4,829	5,406	5,158	4,829
R-squared	0.004	0.007	0.008	0.006	0.009	0.011
Number of idaux	1,835	1,750	1,694	1,835	1,750	1,694

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 14: Hypothesis 5 - Probability of reelection, conditional on running

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Canon PC Promedio ciclo anterior	0.008	0.014	0.012	0.026	0.030	0.042
	(0.016)	(0.017)	(0.018)	(0.034)	(0.034)	(0.036)
c.logcanon_pc#c.logcanon_pc				-0.002	-0.002	-0.004
				(0.004)	(0.004)	(0.005)
year = 2010	-0.014	-0.024	-0.024	-0.012	-0.022	-0.020
	(0.031)	(0.033)	(0.034)	(0.031)	(0.033)	(0.034)
year = 2014	-0.045	-0.063**	-0.065**	-0.043	-0.061**	-0.062**
	(0.029)	(0.030)	(0.030)	(0.029)	(0.030)	(0.030)
Observations	3,273	3,109	2,916	3,273	3,109	2,916
R-squared	0.002	0.003	0.004	0.002	0.004	0.004
Number of idaux	1,671	1,589	1,519	1,671	1,589	1,519

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1