ABSTRACT

Title of Dissertation: ESSAYS ON OVERSIZED PUBLIC EMPLOYMENT AND RENTIER STATES

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This dissertation studies whether politicians in rentier states are more likely to use public sector employment as a redistribution mechanism than in non-rentier states, in exchange for political support. I find that larger resource rents are associated with more public employment, conditional on being a democracy with low productivity and high inequality. In this context, larger resource rents can also change public employment composition, in favor of blue-collar workers and political appointees. Moreover, being resource-wealthy is associated with contextual factors that discourage public service reforms meant to curve down excessive or non-meritocratic public employment growth.

These results are consistent with theory and existing evidence that public jobs can be used by political elites for clientelistic redistribution - that is, in exchange of political support. When productivity is low and inequality high, it can be convenient for politicians to provide more public employment (versus public goods or other private transfers) because it increases their chances of staying in power. Natural resource rents further enhance this tendency, by raising the stakes of keeping office and granting a larger envelope for redistribution.

My findings are relevant because they call for realism in reforms aimed at making the public services of resource rich countries leaner or more meritocratic, by highlighting the potential incompatibility of these policies with political incentives. Furthermore, my work contributes to the literatures on the political economy of the natural resource curse, clientelism and public employment, and public service reforms.

In the first essay, I test existing theories of redistributive politics to consider whether natural resource rents affect politicians' willingness to redistribute income through public employment. Using panel data for 138 countries over 23 years, I find that the relationship between resource rents and public employment size is contingent on the political regime type. In democracies, resource wealth is generally associated with larger public employment, as this strategy provides electoral advantages. The opposite holds for the average autocracy, since more resource rents increase autocrats' ability to repress (hence reducing the need to please the broader population).

In the second essay, I explore the effect of natural resource revenues on municipal public employment in Peru. I exploit the variation in exogenous mining revenue shocks across municipalities, due to a legal reform in 2004 which sharply increased the mining revenues transferred to mineral-producing municipalities. Post reform, producing municipalities significantly increased public employment, providing mainly temporary contracts to (predominantly) blue collar workers and political appointees. Consistent with theory, this employment growth composition suggests that it was partly driven by redistribution concerns and likely clientelistic.

In the third essay, I survey the (relevant) theoretical and empirical literature to explain why resource rich countries are less likely to implement meritocratic reforms of their public services (and thus reduce clientelistic employment). The review shows that public service reforms, while generally politically unattractive, are particularly challenging in resource-wealthy countries. This

is because resource-wealth is associated with contextual factors that further discourage such reforms, such as lower productivity, being less democratic and more prone to violent conflict, and having less programmatic political parties and deeper political budget cycles.

ESSAYS ON OVERSIZED PUBLIC EMPLOYMENT AND RENTIER STATES

by

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Dedication

To my parents and husband.

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List of Abbreviations

CS	Civil service
D&A	Democracies and anocracies
DID	Difference in differences
DPI	Database of Political Institutions
ECA	Eastern Europe and Central Asia
ESA	Eastern & Southern Asia
GCC	Gulf Cooperation
IDB	Inter-American Development Bank
IE	Islands of Excellence
ILO	International Labor Organization
INEI	National Institute of Statistics and Informatics
KSA	Kingdom of Saudi Arabia
LAC	Latin America and the Caribbean
MCTs	Mining Canon transfers
MEF	Ministry of Economics and Finance
MENA	Middle East and North Africa
MINEM	Ministry of Energy and Mining
MP1	Model prediction 1 (on productivity)
MP2	Model prediction 2 (on inequality)
MP3	Model prediction 3 (on the democracy score)
MP4	Model prediction 4 (on NRR, for democracies)
MP5	Model prediction 5 (on NRR, for autocracies)
NAm	North America
NBI	Unsatisfied Basic Needs (*Necesidades Basicas Insatisfechas*)
NP.NPP	Non-producing municipalities in non producing provinces
NP.PP	Non-producing municipalities in producing provinces

NRR	Natural resource rents
NSWE	North, South and West Europe
OECD	Organization for Economic Co-operation and Development
PSM	Public sector management
PS	Public service
RCC	Resource rich countries
RENAMU	National Registry of Municipalities
R&V	Robinson and Verdier
SEAP	South-East Asia and Pacific
SERVIR	National Authority for the Civil Service
SNGs	Subnational governments
TFP	Total factor productivity
UCDP	Uppsala Conflict Data Program
V-Dem	Varieties of Democracy
WAP	Working Age Population
WDI	World Development Indicators

Chapter 1: Introduction

1.1 Natural resource rents and large bureaucracies

I owe the idea for this research to two countries: The Kingdom of Saudi Arabia (KSA) and Venezuela. Between 2011 and 2013, I was part of a World Bank team advising KSA's Ministry of Labor on how the Kingdom could prepare its labor market for transitioning from oil dependency towards a more diversified and productive economy. Much of the answer had to do with reforming the overstaffed, overpaid and largely unproductive public sector, which heavily distorted the labor market. In 2015, 96 percent of Saudi nationals still worked in the public sector. They represented 21 percent of total employment and their wages accounted for 13 percent of the country's GDP [1], more than twice of the average for emerging economies (5 percent in 2005) [2].

I often wondered why the Saudi rulers still relied so heavily on public jobs for oil rents redistribution and as the main social safety net for Saudi nationals. Paying an overstaffed and unproductive public sector was obviously an inefficient policy. Why had the KSA not yet shifted towards other social transfers as alternative redistribution means?

This experience made me see my own home country, Venezuela, in a fresh light. Venezuela, like KSA, is a posterchild of the natural resource curse or "Dutch disease"[3]: Since the 1980s, nat-

[1] Own calculations, based on the General Authority for Statistics of the Kingdom of Saudi Arabia (2015).

[2] See "Time to Sheikh it up" (2016). *The Economist*

[3] This term, coined by *The Economist* in 1977, is used to refer to the economic issues brought about by commodity booms.

ural resource revenues appreciated the exchange rate. This hurt local non-resource industries and private sector employment, and in turn enhanced political pressures to redistribute oil wealth, possibly through public jobs. This narrative had provided me with an easy, ready-made explanation for some of my country's troubles.

Yet, could the resource curse really explain why President Hugo Chavez created 16 extra ministries and 107 vice-ministries in a country of roughly 30 million people (Infobae 2015)? Why did public sector employment need to grow by 108.5 percent between 2000 and 2013 [4], when the average social expenditure in that decade was already high, at about 52 percent of the GDP? [5] While public jobs were not the only means of redistribution, they certainly came in handy when the government needed some extra public support. For example, in 2015, several public employees were forced to vote in the Chavist party primary elections, under threat of being fired (El Nacional, 2015)[6]. This happened in gross violation of Venezuela's legislation: forcing employees to participate in political acts, or to vote for a given party, are electoral crimes. [7]

Why did both the autocratic Saudi monarchy and the initially democratic and popular Chavez government rely so heavily on public jobs as a key redistribution means? How has this policy choice helped ensure the survival of these regimes? The Dutch disease story alone seemed unsatisfactory, since it treats public jobs largely as a gap-filling residual of missing private employment.

In search for answers to this puzzle, I turned to the public choice and political economy

[4] Fernández (2013)

[5] According to De Luca et al. (2013), between years 2000 and 2010, yearly social expenditure ranged between 42 and 59 percent of the GDP (a historical high in Venezuela), standing in sharp contrast with the previous 2 decades average expenditure, at 36% and 39% respectively.

[6] For other recent examples of public workers and employees that were "requested" to vote for the government, not go to opposition rallies or, on the opposite, go to pro-government rallies, see El Nacional (2016), Diaz (2017), Interes (2017), El Impulso (2017), and Martin (2017).

[7] Republica de Venezuela (1998) and Penal (2015).

literature, which explores how political institutions influence size and composition of government *expenditures*. This literature seeks to understand sub-optimal wealth reallocation policy choices (Alesina et al. 2000; Rodrik 2000), explores the influence of electoral rules and regime type on government expenditure (Persson and Tabellini, 1999), and explains under which conditions pre-electoral vote-buying transfers prevail over post-electoral transfers (Hanusch and Keefer, 2013).

I found few recent attempts in the public choice literature to formally model, or empirically test, public employment size determinants (rather than overall government expenditures), with one exception: Robinson and Verdier (2013) provide a first such theoretical model. They explain that public employment can be a preferred redistribution mechanism, *because it solves a two-sided political commitment problem between politicians and their voters*.

Unlike for other types of transfers, politicians have incentives to keep their *pre-election* promises of providing public sector jobs (when productivity is low). They will benefit *post-election*, since they can extract more rents from hiring public employees than they could from private sector taxation. Analogously, voters who want the public job offered by political patrons must indeed support them, since they can only get that job (or keep it) if the patron is in office. Furthermore, the model predicts that *clientelistic* public employment[8] is higher in settings where politicians' rents from being in power are high, and where inequality is high, with public jobs predominantly offered to the patron's poorer clients.

Here then, was a compelling theoretical attempt to answer my puzzle. In KSA's *autocratic setting*, redistributing the country's oil wealth through public jobs provided its leaders with a means of securing the continued loyalty of a large share of Saudis, most of whom still lack the

[8]Clientelism is a phenomenon by which politicians distribute private goods in exchange for votes (or political support in general). In this paper, I focus on the political monopoly of public employment, whereas patrons can use it to reward or punish voters if they win elections (Stokes, 2009).

skills to find jobs in a highly competitive, mostly foreign, private sector. In *more democratic* and highly unequal Venezuela, the Chavez government could use oil wealth to employ its poorer constituents in relatively highly paid public jobs, in exchange for their votes. In both countries, public employees became increasingly dependent on government jobs, thus being tied to their political leaders' willingness to uphold such clientelistic redistribution. Yet, is what's plausible for my two-country sample also true more generally? Answering this question became the objective of my doctoral research.

KSA and Venezuela are certainly not the only countries with resource wealth and large public employment. For instance, a quick look at Latin America showed me that, in at least six countries, recent resource rent expansions were associated with public employment growth (figure 1.1).[9]

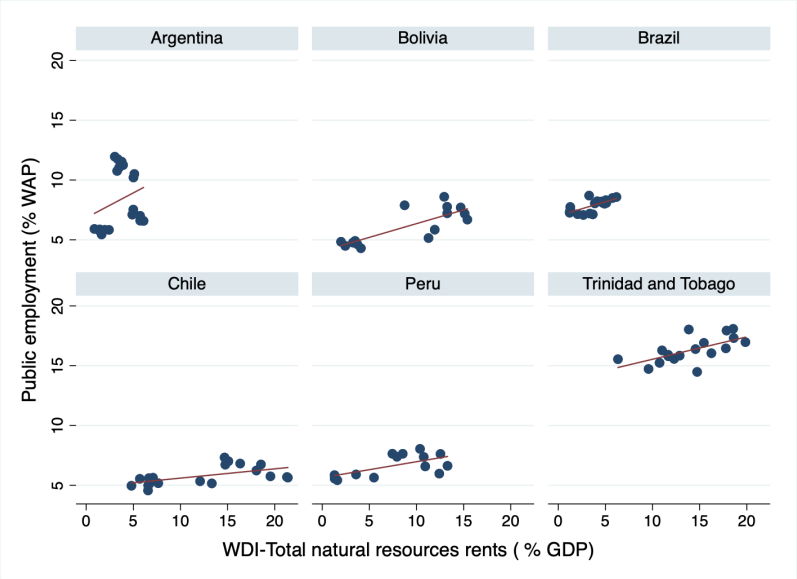
These examples from Latin America, however, also highlighted an important caveat for my research: While *clientelistic redistribution is one important reason for public sector employment growth, it not the only one*. Even though the six countries shared somewhat similar historical backgrounds and political institutions, it seemed implausible that their public employment growth was driven by the same factors. Was the nature of public employment growth in Brazil and Chile, the countries with the two highest civil service development indexes[10] in the region, really comparable to that in Peru and Bolivia, which rank low on the same index[11] and are far more

[9]This region is traditionally a commodities exporter and greatly benefited from the early 2000s commodity prices boom. After the Great Commodities Depression of the 1980s and 1990s, commodity prices nearly doubled in the early 2000s, becoming one of the three major commodity booms since the Second World War. For a compelling illustration of the magnitude of this boom, please refer to LePan (2019).

[10]The Inter-American Development Bank (IDB) developed a Civil Service Development Index, which averages 5 civil service quality indexes: i) efficiency, ii) merit, iii) structural consistency, iv) functional capacity, and v) integrating capacity. Each of these indexes is calculated for 8 civil service subsystems: human resources, work and HR function organization, and management of employment, performance, compensation development management and human and social relations (Inter-American Development Bank, 2022).

[11]See Iacoviello and Strazza (2014)

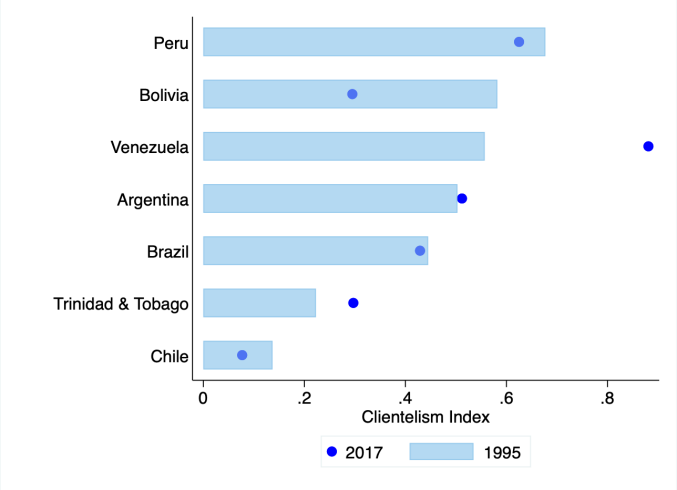
Figure 1.1: Public employment and natural resource rents correlation in selected Latin America & Caribbean countries. 1995-2017 period.



Source: own elaboration, based on ILO (2020) and WDI (2020)

clientelistic (figure 1.2)?

Figure 1.2: Clientelism index in selected Latin America & Caribbean countries. 1995 and 2017 period.



Source: own elaboration, based on the V-Dem Dataset (2021a)

This contrast illustrates a challenge for my research: *relatively large public sector employment need not be harmful*. The size of public employment (relative to the population) varies largely across countries, even within the OECD[12]. There is no formula to determine an "optimal" level of public employment, and differences in the size of public employment hence may simply reflect that some countries chose to produce more public goods or other goods that the market could under-supply.

Larger, public goods producing, bureaucracies are distinct from what I refer to as *inefficient* public employment, where extra units of labor do not necessarily correspond to extra production of such goods (Algan et al., 2002). This "inefficient" public employment occurs when politicians use public jobs strategically in order to: i) redistribute income to a group of people (programmatically or not); or ii) explicitly exchange them for votes and political support (clientelism). *Inefficient public employment is hence not indicated by its mere size*, but rather by the reasons for its growth.

In my first essay, I cannot directly distinguish to what extent public employment increases are attributable to income redistribution purposes. My theory suggests they are, but I cannot directly prove it. Nonetheless, my second essay takes a step forward in support of the hypothesis on resource rents driven public employment increases, due to my findings on new recruits skills composition and lackluster public services provision (to be discussed below).

[12]In Scandinavian countries (Sweden, Denmark, and Norway), for instance, public employment represented about 30 percent of total employment in 2015. By contrast, in Latin American OECD countries (Chile and Mexico), the equivalent share was only 10 percent (Organisation for Economic Co-operation and Development, 2015).

1.2 Moving forward

My first essay (chapter 2) provides the theoretical foundation for this dissertation, as well as a large N empirical test of the following question: are politicians in rentier states more likely to use public sector employment as a preferred redistribution mechanism than in non-rentier states, in exchange for political support? The above mentioned theoretical model, by Robinson and Verdier (2013), predicts that politicians may prefer to redistribute income through public employment, rather than via private income transfers or public goods, in order to solve the commitment problem between politicians and voters - despite inefficiencies. This tendency accentuates as the rents from being in power increase and should hence be more pronounced in resource rich countries.

To my knowledge, this is the first cross-country empirical test of the aforementioned model. Furthermore, I extend this model to explore the predicted link between natural resource rents and public sector employment size, while also considering the role of political regimes. Using panel data for 138 countries over 23 years, I find that the relationship between public sector employment size and natural resource rents largely depends on the political regime type.

In democracies that are highly unequal, resource riches are consistently associated with *larger* public employment. In autocracies, by contrast, the association between resource riches and public employment appears U-shaped. As autocracies move from *low levels* of resource wealth to resource rents levels around 9 percent of GDP (low to moderate resource rents), the size of their public employment *declines*. When resource riches increase beyond 9 percent of GDP, however, this association turns positive and public employment *grows* with increasing resource wealth.

For my second essay (chapter 3), I shift from cross-country to within-country observations. Here, I explore the effect of extra resource revenues on municipal public employment in Peru. I exploit the variation in exogenous mining revenue shocks across municipalities, due to a Mining Canon law reform in year 2004, which sharply increased the mining revenues transferred to mineral-producing municipalities. Post reform, producing municipalities increased the number of public employees by roughly 15 percent. The new recruits had mainly temporary contracts, and were predominantly blue collar workers (security guards and cleaning staff) or directors and officers (usually political appointees). These results hold after a variety of robustness tests, and are consistent with theoretical predictions suggesting that this public employment growth was partly driven by redistribution concerns and hence potentially clientelistic.

The new recruits skills composition, together with a lackluster improvement in some key municipal goods and services, suggest that this extra labor had low productivity. Were they hired for clientelism, to reward their political support? While I cannot directly prove this, it would be unsurprising, since even before the legal reform under study (year 2004) Peru's civil service was already vastly politicized, with high discretion in HR decisions and hardly any merit considerations in hiring (Iacoviello and Strazza, 2014).

A key lesson from this essay is that, in the absence of strong and meritocratic civil service rules, higher resource rents could be redistributed through unproductive public employment, likely with no proportional increase in public goods and services delivery. Furthermore, I add to the decentralization literature by highlighting the need to be cautious when designing sharing rules for natural resource revenues across levels of government.

Finally, in my third essay (chapter 4), I ask why meritocratic reforms are particularly rate in resource rich countries. To address this question, I review the theoretical and empirical lit-

erature on public service reform, to identify which contextual factors generally help explain public service reform trajectories. Then, I analyze to which extent these factors are prevalent in resource-rich countries and can thus help explain their clientelistic public employment. I find that resource-richness is, on average, associated with factors that increase clientelistic public employment attractiveness to political actors, such as lower productivity, less democratic institutions, higher violent conflict probability, prevalence of non-programmatic political parties and more pronounced political budget cycles. This, in turn, makes meritocratic public service reform less likely in resource rich countries than in resource poorer ones.

This last essay makes a small contribution to the literature on public service reform by assessing it in view of its salience for resource-rich countries. While there is a well-developed body of knowledge on public service reform trajectories in developed countries, evidence is less consolidated for developing countries and fairly limited for resource rich ones.

This dissertation's findings call for pragmatism and politically viable policy choices: Is it possible to have lean, meritocratic civil services in resource rich countries? Probably not. Attempts at "right-sizing" public employment are unlikely to succeed, as are any meritocratic reforms. These reforms are unlikely to succeed, or be sustainable in the long run, because they are generally incompatible with politicians incentives. Hence, policy makers and development partners seeking to improve bureaucracies in resource rich countries, may need to adopt modest selective reforms, rather than impose public service reform agendas that run against politicians' clientelistic redistribution incentives.

Chapter 2: Oversized Public Employment and Rentier States. Worldwide Empirical Findings

2.1 Overview

There is plentiful anecdotal evidence of large public employment in countries with natural resource rents abundance. In Saudi Arabia, one of the world's largest oil producers, 96 percent of Saudi nationals worked in the public sector by year 2015, representing 21 percent of the country's total employment [1]. In resource rich Indonesia, the large influx of fiscal resources over the last 15 years drastically raised the number of teachers, creating one of the lowest student-teacher ratios in the developing world (Pierskalla and Sacks, 2020); sadly, despite it, Indonesia still fares rather poorly in terms of educational outcomes[2]. In Venezuela, during a historical oil income boom, President Hugo Chavez created 16 extra ministries and 107 vice-ministries (Infobae, 2015), increasing public employment by 108.5 percent between 2000 and 2013 (Fernández, 2013). Noticeably, these 3 countries are not the only ones with similar trends.

Differences aside, income redistribution could be one of the key reasons behind such public employment expansions. In fact, the use of public employment as a redistribution mechanism is

[1] Own calculations, based on statistics by the General Authority for Statistics of the Kingdom of Saudi Arabia (2015).

[2] Indonesia ranked 64 of the 65 countries that participated in the 2012 Program for International Student Assessment's internationally standardized tests of math and science (Chang et al., 2013).

a widespread phenomenon: it occurs across an ample range of political institutions, economic development levels and cultural contexts (Hicken, 2011). This, nonetheless, begs a question: under which conditions do politicians use public sector employment as a common redistribution mechanism? Do natural resource rents affect such process? In this essay, I provide the first large scale cross-national analysis (covering 23 years and 138 countries) of the relationship between natural resource rents and public employment size. Furthermore, I explore if this relationship differs between democracies and autocracies.

I part from Robinson and Verdier (2013) efforts, who developed the first comprehensive theoretical model that predicts under which circumstances public sector employment is used for redistribution in exchange for political support, a phenomenon known as clientelism. One of their central claims is that (inefficient) redistribution through public jobs will grow as the potential rents from being in office expand, conditional on high inequality and low productivity. In this essay, I expand such theory by i) focusing on the relationship between public employment changes and natural resource rents, ii) permitting heterogeneous effects in different political regime types, and iii) empirically testing these relationships for the first time.

Overall, I find that *in the average country*, resource riches (by themselves) are not associated with higher public employment. In the average country, productivity and the degree of democracy are the two most consistent correlates of public sector employment size. *In democracies*, however, resource riches are consistently associated with larger public employment, possibly due to higher redistribution pressures. This is particularly the case in democracies that are highly unequal, such as in Venezuela and Indonesia.

In autocracies, by contrast, the association between resource riches and public employment appears U-shaped. As autocracies move from low levels of resource wealth to resource rents

levels around 9 percent of GDP, the size of their public employment declines. When resource riches increase beyond 9 percent of GDP, however, this association turns positive and public employment grows with increasing resource wealth, such as in Saudi Arabia. These results are generally robust to alternative definitions of public employment and to using different samples, but they are only partially robust to changes in the inequality definition.

These findings are policy relevant, as they enable us to predict under which circumstances politicians are likely to use public employment for clientelistic income redistribution, in return for political support. This is important, because it helps us be realistic about politically viable policy choices: is it possible to have lean, meritocratic civil services in resource rich countries? Probably not. Public employment capping and downsizing attempts are unlikely to succeed, as these are generally incompatible with politicians incentives. Hence, those hoping to improve public services provision will have to accept somewhat oversized and non-meritocratic bureaucracies, and consider alternative reform paths.

The remainder of the paper proceeds as follows. First, I position the paper within the literature and explain how it contributes to it (section 2.2). Second, I discuss this paper's theoretical baseline and its implications in terms of public employment and natural resource rents (section 2.3), to then propose an initial empirical test for such relationship (section 2.4). Third, I describe the data and evaluate some of the model empirical predictions (section 2.5.2), present the regression analysis (section 2.6), robustness tests (section 3.7.2) and then further discuss the results (section 2.7).

2.2 Place in the literature

This essay contributes primarily to four literatures: first to the inefficient redistribution literature, by providing an initial empirical test on whether public employment is higher in contexts with high natural resource rents, contingent on clientelistic politics. Second, it adds to a closely related literature on clientelism and public employment, by providing a large scale quantitative cross-national analysis in a field that is predominantly qualitative and often case study based. Third, given its focus on natural recourse rents, this study builds into the literature on the political economy of the natural resource curse, by testing one of its key mechanisms. Lastly, it contributes to a small emerging empirical literature on the impact of natural resource on the size o public employment.

2.2.1 Inefficient redistribution literature

The use of public employment as a redistribution mechanism is a widespread phenomenon. It occurs across an ample range of political institutions, economic development levels and cultural contexts (Hicken, 2011). For instance, Alesina et al. (2001) showed that at least one third of Italy's central government wages spent in southern regions were redistributive flows from the north. Alesina et al. (2000) also found evidence that in the U.S. public employment significantly rises in cities where income inequality and ethnic fragmentation are higher, thus used as a a redistributive device. Developing or developed, in a non-trivial number of countries, politicians favor inefficiently large public employment to redistribute wealth, provide social safety nets and/or potentially win votes.

In economics, the *inefficient redistribution* literature provides a useful framework for ex-

plaining sub-optimal policy choices that inefficiently reallocate wealth. Persson and Tabellini (1999) and Lizzeri and Persico (2001) explain that politicians may chose to underinvest in public goods because they can better target supporters with private goods. On a different note, Coate and Morris (1995) argue that inefficiencient forms of redistribution can arise because politicians' may need to *hide* the fact that they were redistributing.

Inspired by this literature, Alesina et al. (2000) extend traditional voting models [3] by incorporating both policy choices (Persson and Tabellini, 1999) and choices over public employment. The authors find that public employment is significantly higher in U.S. cities where income inequality and ethnic fragmentation are higher, discretely redistributing wealth when more direct mechanisms have no political support from the middle class. Rodrik (2000), on the other hand, used a similar framework to show that government employment can also be used as a form of insurance for external economic risk. Rodrik finds no argument for a pure redistribution approach, but claims that developing countries with large exposure to external risk have higher levels of government employment and faster rates of growth of government consumption.

Robinson and Verdier (2013) add to this literature by developing the first comprehensive theoretical model that predicts under which circumstances public sector employment is used for inefficient redistribution in exchange for political support, a phenomenon known as clientelism. One of their central claims is that inefficient redistribution through public jobs will grow as the potential rents from being in office expand, conditional on inequality and low productivity. This essay expands this theoretical effort by i) focusing on the relationship between public employment changes and natural resource rents, ii) permitting heterogeneous effects in democracies and

[3] Probabilistic voting models explain citizens voting behavior as a function of both idiosyncratic preferences and the utility derived from the policy options proposed by politicians. Lindbeck and Weibull (1987) pioneered this approach, predicting balanced-budged redistribution in the presence of political competition.

non-democracies, and iii) empirically testing this relationship for the first time.

2.2.2 Clientelism and public employment literature

An important body of literature points to rent generation ("rents from public employment") and redistribution as a reward for political support ("clientelistic rents") [4] as the main culprits for bloated public sector employment in developing countries. As Jaimovich and Rud (2014) put it, "... bureaucrats may bloat the public sector with excessive workers so as to extract different kinds of perks from some of them. Alternatively, overemployment may be the result of the creation of (unnecessary) jobs as a mean to directly appropriate income from it or to channel transfers to certain desired groups of people. Indirect sources of rents may also lead to an oversized public sector: for example, overmanning may be the result of clientelistic practices by state bureaucrats, as public jobs are somehow exchanged for political support." (Jaimovich and Rud, 2014, p. 145)

Studies on clientelism and size of public employment corroborate the idea public sector employment (or perhaps wage bills), is larger when there is more clientelism. Wantchekon (2003), among others, claim that promising public sector jobs is *the* "common currency" of clientelistic regimes, given its particular ease to be exchanged, privately, for political support. This quid-pro quo can be achieved even in coexistence with highly professional civil services, as Grindle (2012) thoroughly documented, since political actors can use temporary contracts and job categories that do not require formal selection rules in order to retain discretion in new hirings.

For instance, Teso et al. (2018) studied patronage in Brazilian local governments[5], and found that being a political supporter of the ruling party increases the probability of having a

[4] See section 2.4.2 for a detailed discussion on different types of rents.

[5] As previously mentioned, Brazil has highly professional civil service, with the highest civil service development index in Latin America.

public sector job by 10.5 percentage points (a 47 percent increase). Similarly, Calvo and Murillo (2004) and Nazareno et al. (2006) study Argentina's case, and report that in provinces and municipalities where the Peronist party governed, there was a direct link between the number of public employees per thousand and the Peronist vote share in subsequent elections.

This is not only a Latin American phenomenon. Gimpelson and Treisman (2002) report that in the 1990's Russia, public employment often fell after electing new governors (to then rise), presumably due to the skimming of the predecessor's patronage appointments. Similarly, Chandra (2004) finds that incumbent governors in India often enlarge the number of state officials, since they consider dependence on the party for state employment a good strategy to guarantee loyalty. More broadly, Grzymala-Busse (2008) found "extensive targeted and contingent rent distribution" in Italy, Japan, Mexico, Russia, Singapore, and (previously communist) ECA countries, all of which exhibited consistently larger shares of public employment (among other clientelistic handouts).

While a substantial body of literature studies the relationship between clientelism and patronage-based public sector employment, such research is often of limited generalizability, as it is usually of qualitative nature or case study based (Cruz and Keefer, 2015). Furthermore, this research does not delve into the issue of clientelistic public employment and *natural resource* rents, as I plan to do here.

2.2.3 Clientelism in an institutional context

Is it more likely for politicians in certain regimes to give public jobs in exchange for political support? Both empirics and theory suggest that this phenomenon can happen all along the

politico-economic spectrum, from resource poor autocracies to the wealthiest democracies. [6]

By definition, clientelism ought to arise in democracies, were politicians depend on electoral support and often have incentives to tie goods provision to votes. However, evidence shows democracies will rely less in clientelism when there is more political competition and participation [7], inclusive institutions such as greater access to information [8], and in general just more years under democracy, encouraging trust in the state, politicians and parties. [9] In real democracies, where accountability and popular support truly matter, governments must cater to larger population shares to remain in power. Targeted transfers (such as public employment) are relatively unattractive, because of the large size of the group whose support must be won; thus, rational politicians take advantage of the economies of scale of large public goods provision (Bueno De Mesquita et al. 2005, Deacon 2009).

Accordingly, clientelism should be more common the further away we are from democracy, when political influence is more concentrated and leaders need to please mainly some smaller influential groups. In such cases, income redistribution via large targeted transfers becomes preferred to public goods provision, as the latter also spreads out towards non-influential outsiders. (Bueno De Mesquita et al. 2005, Deacon 2009) Furthermore, private transfers are channeled via "patrons", (often) elected public officers that act as intermediaries between the state and the citizens, while also benefiting by the control of such resources (Gandhi and Lust-Okar, 2009). The patrons' "clients" belong to his/her social networks, which is what makes promises of future

[6] Hicken's 2011 review provides ample support for this statement.

[7] For instance, Rodrik (1999) shows that such democratic features are associated with higher manufacturing wages.

[8] As an example, Keefer and Khemani (2014) Benin experiment shows that access to mass media that broadcasts public interest messages can diminish citizen's preference towards clientelistic benefits.

[9] See Bustikova and Corduneanu-Huci (2017), Keefer (2005), Keefer and Vlaicu (2005), Keefer and Khemani (2005), Keefer and Vlaicu (2008) and Hanusch and Keefer (2013).

redistribution more credible.

But, is it then appropriate to talk about clientelism in autocracies? Despite the commonplace assumption that elections are a synonym of democracy, authoritarian regime elections are not rare and serve many purposes, income redistribution included. [10] From the dictators point of view, electoral authoritarianism [11] is convenient to avoid being violently overthrown, helping to identify support and opposition to then reward loyalties and punish dissidence. (Gandhi and Lust-Okar, 2009) In fact, Miller (2015) shows that multiparty autocratic elections can even allow citizens signal dissatisfaction, to which leaders often respond by increasing education and social spending post elections. Ultimately, in non-democracies elections can also be means for rent redistribution [12], although to a much smaller critical mass than in democracies, thus favoring private rewards over public goods.

2.2.4 Resource rents and public employment literature

Economic models of the natural resource curse suggest that resource rents should affect public employment through economic channels. Large influxes of foreign currency (natural resource revenues) appreciate the local currency, increasing export prices and decreasing those of imports. In the long run, the now less competitive local non-resource industries will get hurt, leading to an overall smaller private sector and higher private unemployment. At the same time,

[10] After World War II most dictatorships hold quinquennial multiparty elections, ranging from the "relatively" free to those with very limited and staged choices (Knutsen et al., 2017; Puddington, 2017). Gandhi and Lust-Okar (2009) explains that scholars recognition of the matter lead to the creation of typologies that distinguish "hybrid," "competitive authoritarian," "electoral authoritarian," and other types of non-democratic regimes.

[11] Schedler (2015) indicates that this term is used to define modern authoritarian regimes' trend to hold national multiparty elections that they make sure to win, while at the same time violating basic liberal−democratic minimum standards such as freedom of press, allowing dissidence and political competition, keeping the rule of law, and so on. Naturally, as stressed by Guriev and Treisman (2015), this blurs the line between soft authoritarian regimes and illiberal democracies.

[12] See O'Dwyer (2006), Grzymala-Busse (2007), Trantidis (2015) and Kao (2015).

bureaucracy increases in order to be able to redistribute the natural resource wealth, via a combination of public goods, subsidies and other "favors" to its citizens. Furthermore, governments become a major employer (often trying to compensate for the lack of private jobs), increasing wages for lower skilled workers and creating expectations among certain groups to become government employees and catch a slice of the government's rents.

There is a vast and thoroughly developed literature on the natural resource curse and the economic channels described above. However, recent empirical literature does not directly measure the impact of resource rents on the *size* of public employment (like we attempt to), rather focusing on discussing the diverse mechanisms through which the resource curse impacts other areas of the economy. Example of this would be the link between natural resource and economic development / growth[13], how rent redistribution could create labor market rigidities [14], and how oil riches may affect the allocation of talent in poor institutional setups [15].

To my knowledge, this study provides the first systematic cross-country regression analysis on the issue. The only similar study we could find is that of Ali and Elbadawi (2016), who explain how natural resource wealth *per capita* affect labor market institutions in the Gulf Cooperation Council (GCC) and other Middle East and North Africa (MENA) economies. The authors develop and test a model which predicts that: i) in high resource per capita countries the elites are likely to rely more on expanding public employment and less on political repression; ii) while the opposite holds in moderately endowed but populous countries.[16] This paper, although close

[13]Recent work includes that of Aragon and Rud (2013), Caselli and Michaels (2013), Cavalcanti et al. (2016), Cavalcanti et al. (2015), Kim and Lin (2017) and Toscani (2017).

[14]For instance, see Gatti et al. (2013) for empirical work in the MENA region.

[15]See Ebeke et al. (2015).

[16]Along the same lines, a largely empirical study by Gatti et al. (2013) claims that in MENA governments are also said to redistribute wealth largely via public sector employment, "buying" their citizens political allegiance by these means, together with the use of repression when needed.

in spirit to my model, differentiates in that here the rents are a fundamental factor in explaining the state's capacity to either buy consent *or repress opposition*.[17]

2.2.5 Political economy of the natural resource curse literature

The literature on the political economy natural resource curse focuses on the political incentives and capabilities associated with rents from natural resources (Dunning, 2008). Here, the primary concern is explaining under which conditions natural resources promotes violent conflict [18], authoritarianism or democracy [19]. Although the literature is divided in terms of how/whether natural resource wealth affect institutional development, evidence *predominantly* suggests *negative effects* with a variety of institutional quality measures [20].

Importantly, this literature defines the notion of rentier states: those with reliance on substantial *external* rent (of which the government is the principal recipient), that can sustain the economy without a strong productive domestic sector. Furthermore, a small fraction of society creates wealth, while the rest engages only in its distribution and utilization. The government, accordingly, will play a central role in distributing this wealth to the population; furthermore, it is also the major and employer of last resort (Beblawi, 1987).

The size of public employment in itself, however, is not the dependent variable of interest in this literature. However, some of the mechanisms through which resource rents effects on regime

[17] According to Anthonsen et al. (2012), the rents relative to the population size are the appropriate metric (as opposed to non-per capita rents measures) when interest lays in the role of rents on regime stability or authoritarism.

[18] Most studies, with the exception of Cotet and Tsui (2013), support a causal link from natural resources to conflict. For further details, see Chapter 3's section 4.4.2.7 discussion.

[19] Most recent literature is divided between those advocating a link between resource wealth and autocracy (Andersen and Aslaksen (2013); Arezki and Bruckner (2009), Ross (2001a); Tsui (2011); Wright et al. (2015)); and those supporting a causal link towards democracy instead (Haber and Menaldo, 2011; Liou and Musgrave, 2013). Caselli and Tesei (2015), on the other hand, argue that natural resource windfalls have no effect on democracies, but political consequences are rather heterogeneous in autocracies.

[20] See Bulte et al. (2005), Isham et al. (2005), Knack (2008), Anthonsen et al. (2012).

choice implied in it operate through targeted income redistribution, including public employment. These models, among others, imply that the more resource rents politicians dispose of:

1. The larger the "exogenous" revenues they can use for income redistribution in order to buy votes or buy off critics;

2. The weaker their incentives for investing into growth-promoting public goods for extracting taxes from the economy.

This baseline thus supports the claim that resource rents affect public employment size. Furthermore, the view that public employment is a means of clientelistic rent redistribution is consistent with empirical observations of larger public-sector employment expansions in Africa and in Latin America, regions known for the highest rent-seeking behavior (Rodrik, 2000).

2.3 Theory and empirical propositions

In this section I shortly describe a baseline theoretical model and propose some implications in terms of public employment and natural resource rents. For a formal and detailed discussion of the model, please refer to appendix 2.D.

2.3.1 Theory baseline: a modified standard probabilistic voting model

Robinson and Verdier (2013) (R&V) developed the first theoretical model to explain why income redistribution is inefficient *and* takes the particular form of public employment, contingent on clientelistic politics. In short, this model predicts that clientelistic public employment is likely to emerge in countries with low productivity and high inequality, and that it will further

increase as politicians' rents from being in power (with natural resource rents being a subset) also grow.

The model's key prediction is that low public goods investment (bellow socially efficient levels) helps politicians' seeking to maximize expected returns from being in office, because it maximizes their expected rents from public employment. While this is not the unique factor explaining public goods underinvestment, less public goods (and less aggregate efficiency) make public sector employment offers more attractive to voters. This in turn increases voters dependency on a paternalistic state, making it easier to continue buying their support with job offers.

Redistributive politics are a relationship of exchange, in which *both* involved parties - politicians and voters - face credibility issues. Politicians propose policies to induce certain groups of citizens to vote for them; some of these promises, however, lack credibility, as it might not be in politicians' best interest to implement them once in power. Similarly, citizens might openly voice their support for a party but vote for another. In order to be credible, these two-sided electoral promises must be incentive compatible, since they cannot be enforced legally. Politicians who want to ensure votes must propose policies that provide utility to citizens, *contingent* on them achieving/staying in power. At the same time, if citizens expect politicians to deliver, they should seek for electoral promises that are *ex post* rational for politicians to implement.

Public sector employment is an optimal solution to this political commitment problem, because it is a credible, selective and reversible redistribution mechanism. Employment offers can be more credible than other types of transfers because they are *ex post* rational for both sides, provided we are in a low productivity environment. Politicians can gain more "rents from public employment" than they would from private employment[21], whereas voters also gain "clientelistic

[21] As it will be explained later, politicians can only retain taxes from private employment (which in low produc-

rents" when publicly employed [22]. Politicians also have the added benefit of being able to claim "favors" from hired workers, or could directly transfer the resources managed by such workers to a given group of people. Public jobs, in turn, incentivize voters to keep their electoral promises, as they would lose this selective benefit if their patron lost office. Furthermore, if voting behavior is observable at least at the group level (or can be proxied for), public jobs have the advantage that they can easily be withdrawn as a punishment.

Importantly, in order to make public employment offers more attractive, politicians have incentives to make voters more dependent on them and not contribute to general economic growth and efficiency (that is, under-providing public goods). In low-productivity setups, it is cheaper to buy loyalties by offering safe and relatively well-paid public employment than with public goods or other transfers. Likewise, in low income *and* unequal countries, higher inequality increases the share of potential low-income voters who could earn more in a public rather than private-sector job. In these conditions clientelism is more likely to arise, and the model predicts that as the rents from being in power increase, so will the use of public employment for inefficient redistribution.

2.3.1.1 Basic set-up

The game-theoretical model focuses on four main agents: the incumbent political decision-maker (P) and the challenging political decision-maker (N), plus the two groups of voters each politician caters to, g_P and g_N, which represent a given share of the population λ_g and λ_N. Both groups of agents will have a set of preferences and budgets.

tivity setups can be a small amount), while they can extract rents from public sector employees, who belong to their same social network, deriving from the returns to their work.

[22] They will only take a public sector job if the offered wages are higher than the private sector earnings.

$$U_i^P = c_i - \psi(e_i) + \delta_i + \theta \tag{2.1}$$

Voters maximize their utilities (equation 2.1), where a representative agent i from group g will have a linear utility function U_i^P which depends on i's consumption level c_i, the effort exerted in production e_i and ideological proclivities δ_i and θ [23] . Workers consumption depends on their income, which primarily derives from two employment options:

- The private sector, where pre-tax income of $Ay(I)$ depends on the public investment I chosen by the incumbent politician, as well as on total factor productivity (A). Informal private sector workers earn $(1-\alpha)$ less than their formal counterparts, due to the inherently lower productivity of the informal sector.

- The public sector, where wages W_g^j increases with effort and decreases with the probability of this effort being observed.

In terms of employment, a worker will find in his best interest to accept public employment if the wages derived from it are higher than his potential income in the private informal sector.

When deciding how to vote, workers will weight their intrinsic utility of supporting a politician and the policies offered by each candidate.

$$max_I V_c^P(I, A) = P^P(I)\{(1-\alpha)\tau Ay(I)\lambda_2 + [R(e) - w_1^P]\lambda_1\} - I \tag{2.2}$$

On the other hand, **patrons want to maximize expected consumption**, (equation 2.2) via a combination of tax revenues ($(1-\alpha)\tau Ay(I)\lambda_2$) and profits from public employment ($\Pi =$

[23] Ideology is both individual specific δ_i , as well as an aggregate shock common to all agents θ.

$[R(e) - w_1^P]\lambda_1)$, minus transfers (I). To reap such benefits patrons must win elections (with a probability of P^P), for which they offer a combination of direct rents to individuals (in the form of public employment or income transfers) and/or - productivity enhancing - public goods.

Importantly, equation 2.2 *includes no reference to politicians ideological preferences* (in contrast to those of voters, δ_i and θ), which points to the assumption that *politicians care only about maximizing their own welfare*. This can only be achieved by offering a policy bundle that: i) serves the group of voters who identify with them (so as to win elections); and ii) maximizes the level of public investment I that will allow them to retain the most taxes from those that are not his clients, while extracting the most rents they can out of the clients they employ in the public sector.

2.3.1.2 Critical assumptions

For the model to hold, two critical assumptions are required:

A.1. The probability for policy makers to observe public sector employees efforts increases when they belong to the same social network.

Patron's can earn rents (that is, "rents from public employment") due by workers effort itself: the more effort the higher returns to their work, given their wages W_g^j. If an employer is in my network (one of my voters), effort will likely be higher, and so will rents for the politician. This rather standard assumption is important, because it guarantees that patrons can only make rents on public sector employment by hiring their supporters, as they are the ones in their social network. [24]

[24] Alternatively, the probability of observing effort and behavior might not directly affect rents (the politician has another mechanism to produce rents), but it makes employment offers more credible, since it implies politicians can also observe voting behavior of those in their group and give or take away jobs based on such outcome.

A2. On the credibility of public employment offers

Once the winner politician takes power, he will optimally set the policy bundle $\tilde{B}(\tilde{T_g^j}, \tilde{m_g^j}, \tilde{n_g^j})$ that maximizes his consumption, by choosing a combination of income transfers ($\tilde{m_g^j}$), taxes ($\tilde{T_g^j}$) and public sector employment ($\tilde{n_g^j}$). This policy bundle $\tilde{B}$ need not correspond to the pre-election promised bundle B, as in some cases the politician derives no benefit of providing a given transfer/good.

Critically, public employment is different to a pure transfer of income because it generates rents for the patron; furthermore because of moral hazard, some of these rents can be transferred to employees in the form of efficiency wages. In consequence, consumption maximizing elected officers will:

- Set income transfers to zero ($\tilde{m_g^j} = 0$), since post-election the politician derives no benefit from providing them;
- Raise tax levels ($\tilde{T_g^j}$) so as to maximize revenues *without* making informal sector more attractive than formal one [25]; and
- Employ clients in the public sector ($\tilde{n_g^j} > 0$) when the benefits of providing public sector jobs to the client (rents minus wages) are higher than the revenues to be collected if such client was a private sector worker.

Taken together, these constraints imply that the public sector wage offer of the incumbent patron is also tied up to public investment levels I, as well as to his possibilities to extract taxes from private sector workers. Consequently, as summarized by equation 2.3, it will only be optimal

[25] This assumption is at the core of why taxation policy, as a redistribution device, is sub- obtimal from the politician's perspective. R&V note that "because of the cost of raising taxes on the private sector, employment in the bureaucracy is a relatively attractive way for politicians to generate rents". (p. 262)

for the incumbent to employ clients *ex-post* if the benefits that he derives from providing public sector jobs to the client ($R(\epsilon) - W_P^j$) are no less than the revenues to be collected if such client was in the private sector $(1-\alpha)\tau Ay(I)$, with τ being the "cost" of tax collection. That is, public employment should be a relatively more effective method of extracting rents to citizens.

$$R(\epsilon) - W_P^j \geq (1-\alpha)\tau Ay(I) \tag{2.3}$$

Similarly, equation 2.4 indicates that voters will support the patron if the combination of the job wages and net utility ($W_P^1 - \psi(\epsilon)$) plus the utility of voting for the patron ($\delta_i + \theta$) is larger than the payoff of the potential patron - the after tax private sector job income of $(1-\alpha)Ay(I)$.

$$W_P^1 - \psi(\epsilon) + \delta_i + \theta \geq (1-\alpha)Ay(I) \tag{2.4}$$

2.3.1.3 The role of inequality

We can extend the previous analysis to study the role of income inequality in clientelism [26]. Importantly, *this model explicitly distinguishes between generalized poverty levels (in the form of low productivity A) and inequality*, and each of these will play a distinct role in terms of predicting whether clientelism will arise or not. As noted by Alesina et al. (2000), redistribution is only a concern when in presence of income inequality between distinct groups. In societies with evenly distributed income (regardless of generalized wealth level), there is little political demand for redistribution between citizens. On the contrary, high inequality calls for income redistribution between groups, particularly (in principle) to those that are the least well off.

[26] For formal derivation please refer to Appendix 2.D.5

To allow for inequality, the incumbent P can now make credible job offers to both groups 1 and 2 (g_{P1} & g_{P2}) at the same efficiency wage W_g^j [27], since he belongs to the same social network. There is a third group of voters who are not in the same network as P, so he cannot make credible job offers to any g_{N3} members. Both groups 1 and 2 are size $\lambda/2$, while group 3 is size $1-\lambda$.

Members of g_{P1} and g_{P2} differ only in their riches, with associated incomes of $\sigma^1 Ay(I)$ $> \sigma^2 Ay(I)$, where $\sigma^1 = 1+x$ and $\sigma^2 = 1-x$. x measures income inequality between them. As before, and reflected by equation 2.4, voter's of g_{P1} and g_{P2} will support the incumbent if the offered job and associated net utility is larger than the pay-off from the potential patron. Also as before, public-sector employment for each group must be ex post credible, as per equation 2.3.[28]

Solving equation 2.3 for $y(I)$ yields the levels of investment over which the incumbent patron can make credible offers to the agents of $g = 1, 2$:

$$y[I_1(x)] = \frac{R(\epsilon) - W_P}{A(1-\alpha)\tau} \frac{1}{1+x} \tag{2.5}$$

$$y[I_2(x)] = \frac{R(\epsilon) - W_P}{A(1-\alpha)\tau} \frac{1}{1-x} \tag{2.6}$$

From these equations it reads that when income inequality is null $I_1(0) = I_2(0)$. However, $I_1(x) < I_2(x)$ for any other level of x, so higher levels of public investment always make public sector employment *less* credible for the rich, because of the increasing opportunity cost of not withdrawing taxes from them. As a result, the model predicts that there will be 3 possible regimes,

[27] Voters of g_{P1} and g_{P2} have the same q_g^j, which reflects the probability that their effort level as public sector employees ($e \in \{0, \epsilon\}$) is observed. As a result, each voter will earn a wage of W_g^j.

[28] Appendix 2.D.5 equations 2.23 and 2.24 show the slightly modified version of equations 2.3 and 2.4, respectively, which differ only by the inclusion of σ^1 and σ^2.

for any given value of inequality:

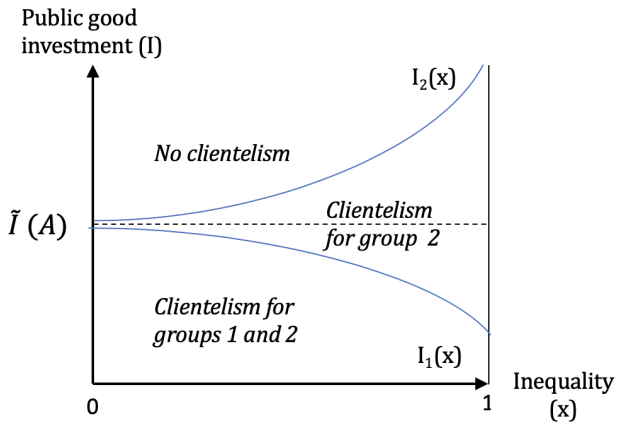
1. Complete clientelism (for g_1 and g_2) : when investment I is so low that $I \leq I_1(x)$, the patron can make credible employment offers to both rich and poor agents;

2. Incomplete clientelism (g_2 only): when investment I ranges between $I_1(x) < I \leq I_2(x)$, the patron can only make offers to the poorer agents; and

3. Non-clientelistic regime: for $I_2(x) < I$, productivity is high enough that the patron prefers all his clients to work in the private sector; public sector employment is no longer attractive.

These results are summarized in figure 2.1, where the regime type is a function of distinct levels of inequality x and the level of provision of I. Importantly, we can see that the more inequality increases, the larger the region of incomplete clientelism is, since it becomes relatively cheaper to provide public jobs to the poor but relative more expensive to do so to the rich.

Figure 2.1: The impact of inequality on clientelism



Source: adapted from Robinson and Verdier (2013)

2.3.1.4 Model predictions

I present the model's basic predictions below. Importantly, as I will further discuss in section 2.4, *I can only empirically test two out of these predictions, namely model prediction MP1 (on productivity) and MP2 (on inequality).*[29]

***Prediction A*: $I^c < I^e$. In the clientelistic regime, the equilibrium level of public investment I^c is smaller than the socially efficient level of I^e.** Inefficiencies in provision arises mainly because:

1. The existence of an informal sector prevents patrons from taxing those citizens and thus provide the socially efficient I^e;

2. When voters are employed in the public sector as a mean to influence their political/voting behavior, there will be an under-supply of public goods that only increase private sector productivity;

3. Under-investment increases the incumbent's probability of reelection ($dP^P/dI < 0$), because:

    - Less public goods keep employment offers credible, by making the private sector less productive and (hence) comparatively less attractive; and
    - More public goods make the alternative patrons' utility increasing offers more credible.

***Prediction B*: $I_u^* < I^e$, but maybe $I_u^* > I^c$.** In the non-clientelistic regime, the equilibrium

[29] These testable predictions are numbered separately, for ease of reference.

level of public investment I_u^* is smaller than the socially efficient provision I^e, and *can* be greater than that of the clientelistic regime, I_c^*.

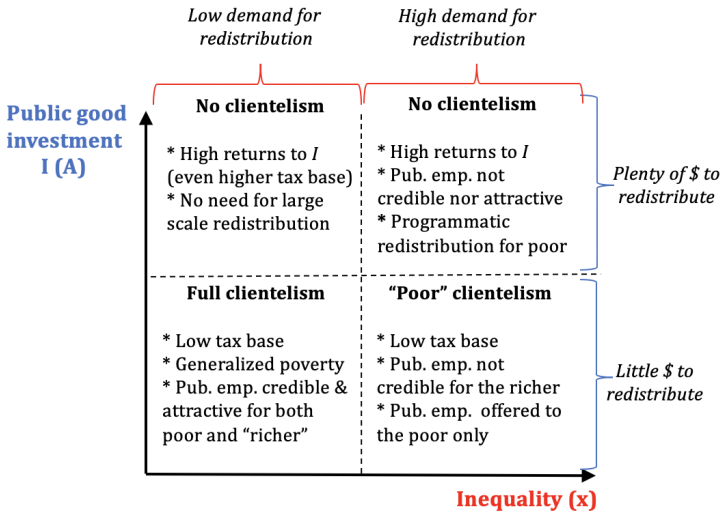
Prediction C: $dI/dR(e) < 0$. The patron will reduce I supply as the rents from being in power $R(e)$ increase, and will increase investment I as the population is less subject to ideological bias.

Model prediction 1 **(MP1): Public employment decreases with higher productivity.** If total factor productivity is low, the incumbent inefficiently employs his clients to bias election results, as a public sector job will be more desirable than a private sector one for the most unproductive subset of the population. Increases in productivity, by contrast, lead to abandoning clientelism, because it increases the opportunity cost of not taxing the (now more productive) private sector workers.

Model prediction 2 **(MP2): Public employment increases with inequality**. An increase in inequality among patron's clients rises the likelihood of clientelism, which will more likely be geared towards the poorest clients. Moreover, when productivity is low (so we are already prone to clientelism), the optimal level of government provided bundle good B decreases with inequality.

Parting from the above mentioned predictions, figure 2.2 explains possible redistribution scenarios, as well as the ultimate reasons behind it, for different productivity and inequality levels.

Figure 2.2: Predicted redistribution pattern, by productivity and inequality levels



Source: own elaboration.

2.3.2 Model extensions

For the purposes of this research, I extend R&V's model to derive predictions for two key research questions: First, can R&V's model be extended to predict the influence of political institutions on clientelistic public employment? Second, what does the model imply for the impact of natural resource rents on clientelistic public employment? R&V's baseline model does not explicitly include natural resource rents as a specific form of rents. It does not contemplate the roll of political institutions either. Should we expect the influence of natural resource rents on clientelistic public employment to depend on political institutions, that is, to differ between democracies and autocracies?

Table 2.1 provides an overview of the (empirically testable) model predictions I derive in this section, by extending R&V's model, in addition to MP1 and MP2 from the original model (see the previous section). With regards to the first question posed above, the extended model

predicts that clientelistic public employment is lower in democracies than in autocracies (MP3). On the second question, the extended model predicts that the impact of natural resource rents differs by political regime type. In democracies, clientelistic public employment *increases* with natural resource rents (MP4). In autocracies, by contrast, total clientelistic public employment *decreases* with natural resource rents (MP5). As a corollary of MP3, MP4 and MP5, the prediction that clientelistic public employment is lower in democracies than in autocracies (MP3) holds particularly in countries with low natural resource rents. It only holds to a lesser extent in countries with high resource rents.

Table 2.1: Extended model predictions for the relationship of natural resource rents and political institutions with public employment.

Number	**Prediction**	**Details**
Model prediction 3 (MP3)	$dPE/Democracy < 0$	Public employment *decreases* with more democratic political institutions.
Model prediction 4 (MP4)	$dPE/d\rho \vert Democracy > 0$	In democracies, public employment *increases* with natural resource rents.
Model prediction 5 (MP5)	$dPE/d\rho \vert Autocracy < 0$	In autocracies, public employment *decreases* with natural resource rents.
Corollary of M3, MP4 and MP5 (MP6)	$dPE/Democracy \vert \rho = poor < dPE/Democracy \vert \rho = rich$	MP3 holds particularly in *resource-poor** countries. It only holds to a lesser extent in *resource-rich** countries.

Note: $\rho =$ Natural resource rents. For empirical definitions of resource-poor and rich (*), please see section 2.5.3.

The following sections will derive each of these predictions in detail.

2.3.2.1 Public employment decreases with more democratic political institutions (MP3)

It seems intuitive that the extent of clientelist public employment depends on the political institutions that constrain elected leaders' choices (Robinson et al., 2006). R & V's initial model

(my baseline), however, does not contemplate the role of political institutions in clientelistic public employment. How then can I extend a probabilistic *voting* model, that is prima facie applicable to *democratic settings with strong electoral accountability*, to autocracies?

This question calls for briefly clarifying how the concept of *elections* - which is at the center of probabilistic voting models — relates to the concepts of *democracy* and *autocracy*. A standard definition by Robert Dahl defines *democracies* as regimes with i) elected officials in control of the government, ii) free, fair, and frequent elections; iii) freedom of expression, iv) access to alternative sources of information, v) associational autonomy, and vi) inclusive citizenship. (Dahl and Shapiro, 2015). *Autocracies*, by contrast, are are systems of government that strongly restrict or suppress competitive political participation. There, chief executives are chosen within the political elite, and when in power they face few institutional constraints (Center for Systemic Peace, 2018).

Overall, these definitions highlight that *competitive elections are necessary characteristic of democracies*; but that *autocracies are not necessarily marked by the absence of elections*. The latter holds because (i) autocracies may well recruit their executives through elections *de jure* – but seek to repress/restrict electoral competition *de facto*; and (ii) autocracies may even allow some de facto political competition, but remain classified as autocracies, since executive power is largely unconstrained.

Empirically, the aggregation methodology used by the Polity IV project for constructing its composite polity score underlines that autocracies are not necessarily marked by the absence of elections, although these rarely are competitive. The polity score, which I adapt and use in my regression analysis, is consistent with the above definitions (see section 2.4.2.4 and appendix 4.B.2 for details). The composite polity score comprises the following major dimensions: (i)

the competitiveness[30] and (ii) openness[31] of executive recruitment; (iii) the competitiveness of political participation[32] and; (iv) constraints on the chief executive. [33].

Broadly speaking, the presence of de jure electoral institutions is indicated by dimensions (i) and (ii) of the polity score, the extend of de facto political competition by dimension (iii) and, the strength of constraints to executive power by dimension (iv). This decomposition of the polity score enables us to show that, indeed, about half of the countries classified as autocracies do have *some* de jure electoral institutions in the form of open executive recruitment (Figure 2.3, panel C), and mild political competition (Figure 2.3, panel B).[34]

Conceptually, and consistent with the argument above, a small strain of the literature suggest that probabilistic voting models can be used to study clientelism in non-democracies (either autocracies or anocracies). While the literature on probabilistic voting models has predominantly focused on democracies, this need not mean they are not applicable to non-democratic settings. As Golden and Min (2013) note, this focus might be opportunistic, motivated by the (only recently remediated) lack of systematic data on the allocation of government goods and services in non-democracies.

Robinson et al. (2006) and Deacon (2009) adapted probabilistic voting models to explain

[30]Competitiveness of the executive recruitment (XRCOMP) reflects to what extent lower rank officials can advance to higher positions. For instance, chief executives selection is considered competitive if done via popular elections, with 2 or more viable parties or candidates (Center for Systemic Peace, 2018).

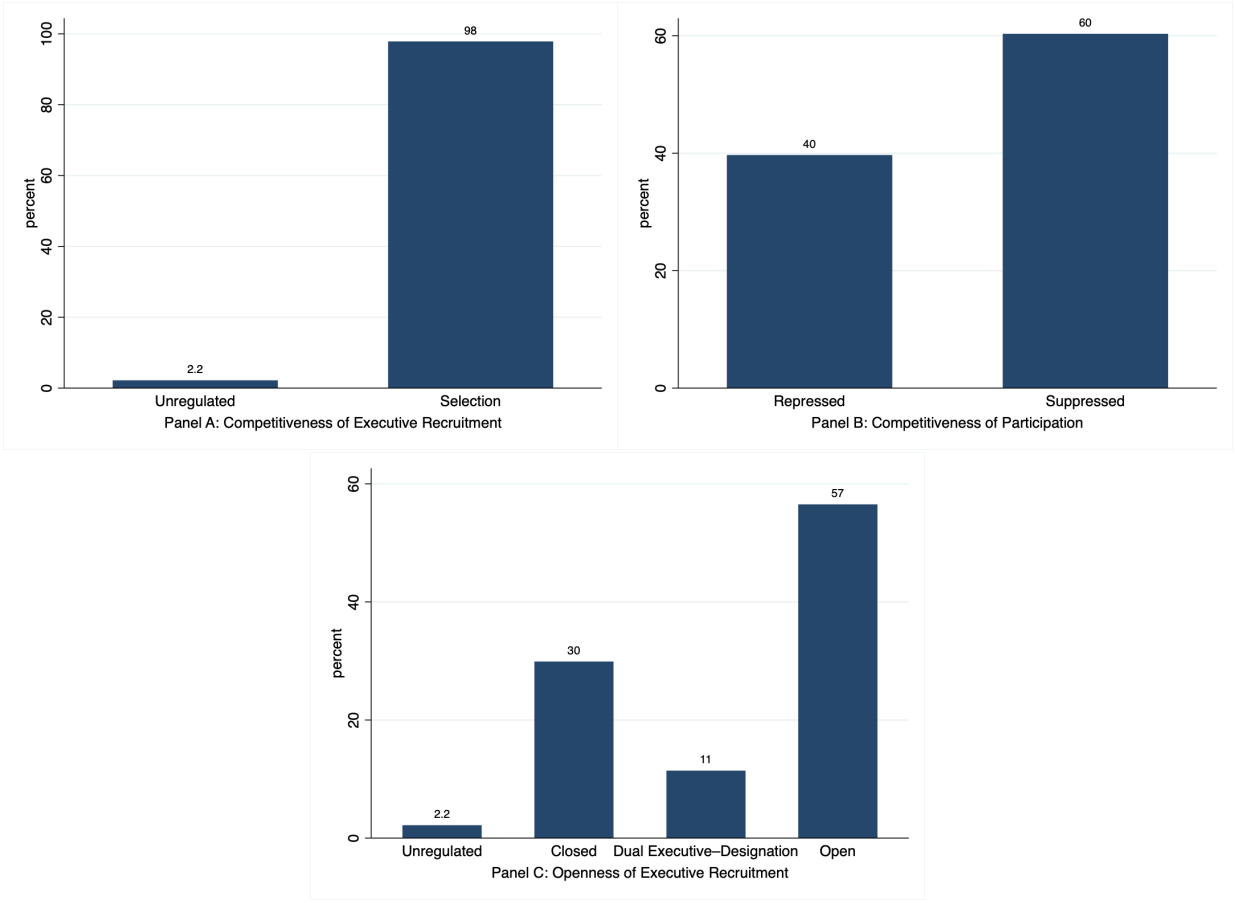
[31]Recruitment of the chief executive is "open" (XROPEN) if there is in place a regular process that potentially allows anyone in the politically active population to attain a position (Center for Systemic Peace, 2018).

[32]The competitiveness of participation (PARCOMP) measures the extent to which alternative preferences for policy and leadership are allowed. This, naturally, implies a significant degree of civil interaction (Center for Systemic Peace, 2018).

[33]Consistent with these dimensions, Center for Systemic Peace (2018) argues that "A mature and internally coherent democracy, for example, might be operationally defined as one in which (a) political participation is unrestricted, open, and fully competitive; (b) executive recruitment is elective, and (c) constraints on the chief executive are substantial."

[34]60 percent of autocracies in the sample have suppressed competitiveness of participation. As per Center for Systemic Peace (2018), this implies that some organized political competition occurs outside government; however, the regime sharply limits it, excluding substantial groups from participation.

Figure 2.3: Dimensions of the Polity IV score, for autocracies. 1995-2017 period.



Note: Author's elaboration, based on the Polity IV Project (2018) database. These statistics come from the 20 countries in my sample that have been autocracies at any point in the period under study.

private and public goods provision patterns, conditional on political institutions. They suggest treating political influence analogous to income inequality — that is, as a factor that assigns uneven weights to different groups of voters in a probabilistic voting model. [35] In democracies, where (i) voting age citizens are deemed to have fair access to elections and (ii) elections are the primary means of selecting political leaders, these weights are *relatively evenly distributed across citizens*. The political influence of different groups hence matches the size of their membership. This is much like a situation with no inequality in my current baseline model, and thus one were

[35] As noted by Deacon (2009), political influence could be distributed due to wealth, command of a military force, religious authority, membership in a royal family, among others.

clientelism is less likely (MP2). Consequently, the models predict larger spending on public goods.

In autocracies, by contrast, (i) access to elections may be limited and (ii) voters' preferences expressed in elections may not determine the selection of leaders. Rather, the political influence of some powerful groups may outweigh the size of their membership. Robinson et al. (2006) and Deacon (2009) adapted models thus predict that dictatorial regimes will privilege transfers of private goods, targeted to smaller powerful groups.

The basic intuition behind these adaptations lies in Bueno De Mesquita et al. (2005) selectorate model, which explains how policy choices, under all forms of government, are made to promote the interests of an elite group (the "selectorate"). Accordingly, political leaders seek above all things to survive in office. They will hence choose the public and private goods provision that maximizes their chances for staying in power. The main distinction between democracies and autocracies is the size of the elites relative to the population, with democracies having much larger elites or influence groups. Consequently, it is incentive-compatible for political leaders in democracies to focus on providing public goods, which can effectively reach a large "elite". In autocracies by contrast, these smaller elites can more efficiently be targeted with private good transfers, such as public jobs.

The literature on political institutions and clientelism I have discussed here and above (see section 2.2.3) hence permits two main conclusions. First, it is possible to extend R&V's probabilistic voting model to autocratic settings; it can be achieved by assigning higher weights to those groups of voters considered part of the elite. Second, the above "selectorate" argument yields a clear prediction for the relationship between regime type and clientelistic public employment:

Model prediction 3 (MP3): Public employment decreases with more democratic political

institutions. Formally, $dPE/Democracy < 0$. As argued above, the size of the "selectorate", the politically influential elites, is larger in democracies than in autocracies. In autocracies, it is hence less costly for political leaders to "buy" the selectorate's support through *private* targeted transfers, like clientelistic public employment. In democracies, by contrast, it would be more efficient to reach a larger selectorate through broad public goods (service delivery), rather than relying on private transfers.

2.3.2.2 In democracies, public employment increases with natural resource rents (MP4)

I here first introduce natural resource rents into the original model, conditional on being in a *democracy* (see the following section for autocracies). I do so by treating *natural resource rents as part of politicians' returns from being in office.*

The original model distinguishes between two key types of rents linked to public employment[36]: i) those accruing to workers, in the form of above-productivity wages ("clientelistic rents")[37], and ii) those that political patrons will receive ("rents from public employment"). The second type of rent plays a key role in the model's dynamic, with a particular focus on patrons' returns from employing their voters, in excess of the salary paid. Patrons rents from public employment, plus tax revenues minus transfers, constitute politicians' consumption if in power. As

[36] R&V's use of the term rents suggests that it is consistent with the common definition of economic rents, described by Tollison (1982) as "a return (for a factor of production) in excess of the resource owner's opportunity cost". Generally speaking, rental income enables us to earn money through the possession of scarce assets, either as "the income derived from the gift of nature" (Marshall, 1920, p. 350), or the price paid for all natural and differential endowments, such as land, location, climate, etc. (Beblawi, 1987) We owe the modern, negative, usage of the term to Gordon Tullock (Tullock, 1967, p. 43), who defines it as "...the use of resources for the purpose of obtaining rents for people where the rents themselves come from some activity that has negative social value". (Tullock et al., 2002, p. 43)

[37] According to R&V, "... because of moral hazard, optimal employment contracts concede rents to workers." (Robinson and Verdier, 2013, p. 262).

R&V themselves note, this notion of rents can easily be broadened to more generally "... capture the extra benefits from being in office"; these "*...could be natural resource rents* accruing to government. In this case, the model explains how a *political economy resource curse operates*, biasing down public investment and inducing inefficient redistribution"(Robinson and Verdier, 2013, p. 278).[38]

Building on R&V's terminology, we can hence distinguish three types of rents:

1. *Clientelistic rents* (W_g^j, wages above productivity), earned by workers through public employment. This term follows from the clientelism literature, portraying public jobs as an income transfer mechanisms to explicitly reward political support to the incumbent politician;

2. *Rents from public employment* ($R(e) - w^P$), received by political patrons who employ their voters. These can take different forms, but essentially imply the extraction of resources/favors from employees in the patrons network;

3. *Natural resource rents* (ρ), or government natural resource revenues.

The empirical model in my paper focuses on the third type of rents: the effect of natural resource rents on the size of public employment. Introducing natural resource rents into R&V's model in democratic settings is straightforward. In contrast to rents from public employment, as "manna from heaven" (Dunning, 2010), these rents do not depend on the extent of public goods investment, productive activity in the economy, nor on the size of public employment (see detailed argument below). However, they increase the benefits from being in office and, potentially, the envelope for redistribution (e.g. through clientelistic rents). Natural resource rents (ρ) can hence

[38] Italics are not included in the original text.

simply be introduced in R&V's original politicians' consumption function as part of politicians' returns from being in office (equation 2.7).

$$max_I V_c^P(I, A) = P^P(I)\{(1-\alpha)\tau A y(I)\lambda_3 + [R(e) + \rho - w^P](\lambda_1 + \lambda_2)\} - I \tag{2.7}$$

Importantly, these natural resource rents (ρ) are distinct from the rents from public employment ($R[e] - w^P$) and affect equilibrium outcomes through distinct channels.

Unlike $R(e)$, the level of ρ does *not* depend on the level of public goods investment I. The rents from public employment depend directly on I primarily because reducing I increases the relative attractiveness of public jobs. This, in turn, increases the size of public employment and hence the total rents to be extracted from it.

By contrast, intuitively, the *level* of ρ is independent of the incumbents choice of I. Natural resource rents accrue to whomever wins the election, regardless of the prior choice of I or the policy bundle $B(T_g^j, m_g^j, n_g^j)$. Why then, one might wonder, should natural resources influence the incumbent's choice of I, and thus the level of inefficient public employment?

More natural resource rents (ρ) increase the stakes of winning the election for the incumbent. The incumbent will then attribute higher weight to his probability of winning in his overall utility maximization ($dP^P/d\rho > 0$). We know that an incumbent's probability of winning office (dP^P) increases with declining public investment I ($dP^P/dI < 0$). In the presence of natural resource rents (ρ), he will hence reduce investment ($dI/d\rho < 0$). More formally:

$dI/d\rho = dI/dP^P * dP^P/d\rho$, with $dI/dP^P < 0$ and $dP^P/d\rho > 0$, such that $dI/d\rho < 0$.

Reduced investment, on the other hand, entails larger inefficient public employment (pre-

diction 1). Consequently:

***Model prediction 4 (MP4)*: In democracies, public employment increases with natural resource rents.** Formally, $dPE/d\rho|Democracy > 0$. *MP4 is the main focus of my dissertation*. The original comparative static (prediction C) focuses on the impact of rents on investment $dI/dR(e)$, and does not explicitly include the comparative static of interest to me: $dI/d\rho$.

However, as I will argue in the following section, this prediction only holds unambiguously in democracies. For autocracies, I develop an alternative prediction (MP5, below).

2.3.2.3 In autocracies, public employment decreases with natural resource rents (MP5)

Given the discussion in section 2.3.2.1, it is more than reasonable to assume that natural resource rents could affect public employment differently, conditional on political institutions. In this section I propose that the relationship between natural resource rents and public employment is the entire opposite in autocracies (MP5) than in democracies (MP4).

***Model prediction 5* (MP5): In autocracies, public employment decreases with natural resource rents.** Formally, $dPE/d\rho|Autocracy < 0$.

Why should the relationship between natural resource rents and clientelistic public employment differ between autocracies and democracies? I argue that there are *two countervailing channels* at work.

I. The *"stakes of winning the election"* channel (= MP4). In autocracies, as in democracies, higher natural resource rents increase the stakes of winning the elections (or remaining in power) for the incumbent. The channel underpinning MP4 hence should also hold in autocracies: Fac-

ing higher stakes of winning the election ($dP^P/d\rho > 0$), the incumbent will reduce investment ($dI/d\rho < 0$). Hence, $dI/\rho < 0$, also in autocracies. Reduced investment, in turn, entails larger public employment. This channel hence predicts a positive impact of natural resource rents on public employment, also in autocracies: $dPE_{Ch.Winning}/d\rho|Autocracy > 0$

II. The *"ability to repress"* channel. In autocracies, however, a countervailing channel is at play. Higher natural resource rents can also serve autocratic incumbents as a means for financing *repression*. As argued above (see MP3), the literature suggests that autocracies are characterized by smaller selectorates or elite groups than democracies. This, in the first place, makes clientelistic public employment more attractive to incumbents in autocracies. In autocracies, however, higher natural resource rents may allow leaders to further shrink the size of the selectorate or elite group they depend on for staying in power[39].

Natural resource rents provide leaders with "unconditional" (not originated from taxes) revenues. Autocratic leaders – in contrast to democratic ones - can use these revenues to invest in the national security apparatus, to repress the populations' aspirations for change (either by threat or actual action) and hence to further shrink the selectorate. Consequently, *resource rich autocrats can afford to redistribute less than resource poorer ones*, and share those riches predominantly with the elites[40]. The "ability to repress channel" hence predicts a negative impact of natural resource rents on public employment, in autocracies: $dPE_{Ch.Repress}/d\rho|Autocracy < 0$

Formally, the politician's optimization problem can be expanded to include the costs of setting up a security apparatus (Rep, in equation 2.8). Repression arises as a factor that affects the probability of remaining in power [41], conditional on the current political institutions.

[39] See Robinson et al. (2006), Deacon (2009) and Miller (2015).

[40] See Ali and Elbadawi (2016), Anthonsen et al. (2012), Busse and Groning (2013),Bueno De Mesquita and Smith (2010), Knutsen et al. (2017) and Robinson et al. (2014)

[41] Either by wining elections, in democracies, or by eliminating the change of leadership in autocracies.

In democracies, repression is to be avoided because it diminishes the probability of reelection ($dP^P/dRep < 0$), while in autocracies repression *could* efficiently suppress leadership change when natural resource rents are high, relative to population size. [42]

$$max_I V_c^P(I, A|Regime) = P^P(I)\{(1-\alpha)\tau Ay(I)\lambda_3+[R(e)+\rho-w^P](\lambda_1+\lambda_2)-Rep\}-I \quad (2.8)$$

The available theory does not yield a clear prediction as to which of these two countervailing channels dominates in autocracies. However, in contrast with democracies, I assume at the moment that the "ability to repress" channel dominates. Consequently, in autocracies, on balance, public employment decreases with natural resource rents (MP5).

2.3.2.4 Corollary (MP6): Model prediction 3 holds particularly in countries with low natural resource rents.

If the relationship between natural resource rents and clientelistic public employment differs between democracies and autocracies, a related question naturally arises: Conversely, does the difference in the size of clientelistic public employment between democracies and autocracies depend on the level of natural resource rents? MP3, MP4 and MP5 jointly yield a useful, empirically testable corollary with regards to this question.

Intuitively, prediction MP3 should hold for natural resource-rich settings. Accordingly, clientelistic public employment should be *lower in resource-rich democracies than in resource-rich autocracies* (MP3). What does this imply for the analogous comparison between *resource-*

[42] This argument has been fully developed by Ali and Elbadawi (2016).

poor democracies with resource-poor autocracies? The difference in clientelistic public employment should be *enlarged* by two factors. Relative to *resource-rich settings*: (i) MP4 implies that *resource-poor democracies* are predicted to have lower clientelistic public employment than *resource-rich democracies*; and (ii) MP5 implies that *resource-poor autocracies* are predicted to have higher clientelistic public employment than *resource rich autocracies*.

Formally, a simple way of showing this is as follows:

(1) $dPE/d\rho|Democracy > 0$ (MP5) implies that $PE|Democracy[\rho = High] > PE|Democracy[\rho = Low]$

(2) $dPE/d\rho|Autocracy < 0$ (MP4) implies that $PE|Autocracy[\rho = Low] > PE|Autocracy[\rho = High]$

(3) $dPE/Democracy < 0$ (MP3) implies that $PE|Autocracy[\rho = High] > PE|Democracy[\rho = High]$

Combining (2), (3) and (1) yields the following chain of inequalities:

$$\text{(4)} \quad \underset{(2)}{PE|Autoc.[\rho = Low] > PE|Autoc.[\rho = High]} \quad \underset{(3)}{> PE|Democ.[\rho = High]} \quad \underset{(1)}{> PE|Democ.[\rho = Low]}$$

Hence, if the prediction MP3 that clientelistic public employment is lower in democracies than in autocracies (MP3) holds for high natural resource settings ($\rho = High$), it must also hold for low natural resource settings ($\rho = Low$). Further, (4) implies that this *negative difference must be larger in low natural resource settings*.

Model prediction 6 **(MP6, corollary of MP3, MP4 and MP5)**: The prediction that public employment is lower in democracies than in autocracies (MP3) holds particularly in *resource-poor*[43] countries. It only holds to a lesser extent in *resource-rich* countries.

[43] For empirical definitions of resource-poor and rich, please refer to section 2.5.3.

2.3.2.5 Clarification: on politicians' preferences

My stance so far has been that, unlike voters, politicians have *no clear ideological preferences and care only about winning the elections* in order to maximize their own welfare. This is a standard and frequently used assumption in probabilistic voting models, and in all likeliness not distant (on average) from reality, even the more so in less democratic nations. In Bueno de Mesquita and Smith's words: "survival is the primary objective of political leaders", who choose policies that shape public and private goods provision as the means to guarantee their political power. (Bueno De Mesquita and Smith, 2010, p.1)

Should one wish to be less cynical about human nature, a small stream of literature suggests my model is also compatible with setups in which politicians have particular ideological preferences. Importantly, Dixit and Londregan (1996) model the electoral politics of redistribution when voters *and parties* care also about inequality. In their analysis, redistribution will change with the parties effectiveness (or lack of) to deliver transfers to specific groups. When parties are:

1. Equally good at delivering transfers to *any* group, both parties will focus on swing voter groups which are, more likely than not, the poorer voters[44] ; or

2. More effective in delivering transfers to their *own* voters, "machine politics" are bound to happen, and parties will favor their particular group of supporters.

Dixit and Londregan (1996) first scenario is fully consistent with the prediction of public

[44] In the authors own words, "... we should expect the poor to do well in tactical redistributive politics. However, the reason is not that any ethical considerations enter the parties' calculations. Instead, it is a cold calculation of votes - the poor voters switch more readily in response to economic benefits because the incremental dollar matters more to them". (Dixit and Londregan, 1996, p. 1143)

sector employment being preferentially offered to the poor, which is in line with my model's logic. Moreover, their second scenario is also consistent with a clientelistic politics framework, particularly so given the key assumption that it is easier to observe effort within your own network of voters. Thus, *in settings where elections matter, politicians' redistribution preferences are not likely to affect the key results.*

2.4 Empirical model

In this section I propose an empirical adaptation of the model, to test whether politicians in rentier states are more likely to provide public sector employment as a device to redistribute income, in exchange for political support. Equation 2.9 summarizes the ideally required information to empirically test the sign of the change in public sector employees when natural resource rents increase, $dn/d\rho$.

$$Y_{igc} = F[X_{igc}, T_{igc}, TFP_c, Ineq_c, PG_c, \rho R_c, Inst_c] \tag{2.9}$$

Here, public sector employment for individual *i* of group *g* in country *c* (Y_{igc}) depends on:

- X_{igc} - Vector of individual characteristics, meant to characterize different groups which politicians cater to. This includes information about *i*'s poverty and informality status, labor force status (if not employed) and other general demographic control variables;

- T_{igc} - Vector of government transfers for *i*, capturing the interception between politicians redistribution preferences and their budget and political constraints. This includes tax rates t_{igc} and income transfers m_{igc}, as an equivalent of the policy bundle variable B;

- $Ineq_c$ - Inequality in country c;
- PG_c - Vector of public goods expenditure in c, as an equivalent of public investment I;
- TFP_c - Total factor productivity in c, as an equivalent for A;
- R_c - Rents extraction potential in c, as the rents variable;
- $Inst_c$ - Institutional makeup in c.

All of equation 2.9 variables have empirical equivalents readily available for the general public; however, there are two limitations to such data: i) it is not equally available for all countries, and ii) it can be found for the largest number of observations only at an aggregate country level. In terms of the first problem, I can include only those variables which sample size approximates as closely as possible to that of the dependent variable (the one with the largest sample size in the dataset). With regards to the second problem, I do a first modeling attempt using aggregate data; if the model proves useful, then future research will include more disaggregate data.

Consequently, and given the current limitations, I will adapt equation 2.9 to test the following hypotheses: a larger public sector is associated with decreases in productivity (MP1), more inequality (MP2) and less democratic institutions (MP3). The correlation between public sector employment and natural resource rents will depend on whether we are in a democracy (positive, MP4) or a non-democracy (negative, MP5). Due to data constraints, I cannot evaluate its correlation with labor force characteristics, poverty level, government transfers and more generally public good investments.

2.4.1 Model specification - fixed effects regression

2.4.1.1 Basic model

My model specification is a fixed effects regression, using country-year data for the entire period under study (as per equation 2.10). In the current analysis, country fixed effects help reducing the bias caused by unobserved time-invariant factors; as a downside, they absorb most inter-country variance and only allow to identify intra-country one. Importantly, this empirical strategy does not permit to attribute causation; at best, we can see some general associations / correlations between variables. Thus, through my analysis, I will concentrate on the sign and significance of the coefficients, and will only discuss the magnitudes for descriptive purposes.

$$\begin{aligned} Y_{ct} = \alpha_0 + (\alpha_1 + \delta_1 Inequality_{ct}) * Rents_{ct} + \alpha_2 Productivity_{ct} \\ + \alpha_3 Inequality_{ct} + \alpha_4 DemocracyScore_{ct} + \beta X_{ct} + Country_c + \epsilon_{ct} \end{aligned} \tag{2.10}$$

Given the data constraints and considering the relevant literature [45], public sector employment (as a share of the working age population, *WAP*) in country *c*, in year *t*, depends on:

- $Productivity_{ct}$ - in the form of output per worker (to test MP1);
- $Inequality_{ct}$ - Uneven access to high quality basic education (to test MP2);
- $Rents_{ct}$- Natural resource rent dependence, measured as natural resource rents as a share of GDP (to test MP4 and MP5);
- $DemocracyScore_{ct}$ - is a rescaled version of the polity IV index. It ranges from 0 to 10,

[45] My choice of variables and econometric specification is informed mainly by Alesina et al. (2000), Ali and Elbadawi (2016), Algan et al. (2002), Arezki and Bruckner (2009), Brunnschweiler and Bulte (2008), Cruz and Keefer (2015), Dunning (2008), Ebeke et al. (2015), Persson and Tabellini (1999) and Rodrik (2000).

and increases as countries have more competitive political participation and recruitment of the executive, and less constraints on the chief executive (to test MP3).

In addition, I add a vector of control variables X_{ct} which include:

- $Dependency_{ct}$ - Ratio of persons below 14 and above 64 years over the working age population;
- $Duration_{ct}$ - Number of years under the current polity, as a measure of regime stability;
- $Population_{ct}$ - To determine whether population size affects policy choices; [46];
- $Trade_{ct}$ - Trade openness (trade over GDP), the external risk faced by the domestic economy;
- Urb_{ct} - Share of urban population, as more densely populated areas might require more government services;
- $Country_c$ - Country fixed effects, to control for unobservable time-invariant characteristics.

Appendix 4.B.2 contains detailed definitions and data sources for all the above mentioned variables, while the next section includes an in depth discussion on the most critical variables.

The main coefficients of interest are those for natural resource rents dependency (α_1), and the interaction term between resource dependence and inequality (δ_1). Positive values for α_1 and δ_1 would confirm the hypothesis of natural resource driven clientelistic public employment.

In terms of the control variables, positive coefficients for dependency ratio and negative for trade openness would favor the alternative hypothesis of public sector employment as social

[46] I will use a population control, as opposed to population weights, to generalize results at the country level, not at the population level. See Solon et al. (2013) for a detailed discussion on the use of weights.

insurance. A negative coefficient for regime duration variable would also support the hypothesis of clientelistic public sector employment, as presumably the more stable the regime the larger the trust citizens have in the state, so politicians will need to rely less on clientelistic practices.

Importantly, equation (2.10) reflects only some basic model predictions (MP1, MP2 and MP3), but does not properly distinguish whether resource riches affect public employment similarly in different political regimes (model extensions, MP4 and MP5). One possibility to test this is adding an interaction between natural resource rents and the democracy score.

$$\begin{aligned} Y_{ct} = \alpha_0 + (\alpha_1 + \delta_1 Inequality_{ct} + \delta_2 DemocracyScore_{ct}) * Rents_{ct} + \alpha_2 Productivity_{ct} \\ + \alpha_3 Inequality_{ct} + \alpha_4 DemocracyScore_{ct} + \beta X_{ct} + Country_c + \epsilon_{ct} \end{aligned} \quad (2.11)$$

Equation 2.11 only adds the term $\delta_2 DemocracyScore_{ct} * Rents_{ct}$ to equation 2.10. As per prediction MP4, α_1 and δ_2 should be positive in democracies, reflecting that, when political institutions improve, higher resource rents will partially be spent in more public employment (possibly due to redistribution pressures). On the opposite, α_1 (and possibly δ_2) should be negative in autocracies (prediction MP5), indicating that resource riches allow autocrats to depend less on redistributive public employment for their survival.

2.4.1.2 Alternative identification strategy: using two samples

While a model with interactions does say if political institutions mediate the effect of natural resources on public employment, it does not really capture capture wether α_1 and δ_2 sharply differ by political regime types (MP4 and MP5). A simple way to test this is to estimate my basic model (equation 2.10) with 2 distinct samples, defined in virtue of whether countries begin the

period under study as autocracies (17 countries[47]), or as democracies or anocracies (121 countries). The two key reasons to pursue this strategy are that i) the distribution of natural resource rents differs substantially by regime type; and that ii) some literature suggests a pivotal role of ex-ante political institutions, as opposed to current ones.

The main argument for using separate samples is that the distribution of natural resource rents differs substantially by regime type.[48]. *Natural resource rents dependence is considerably higher in autocracies* than in their more democratic counterparts: being in the top quintile of resource riches in autocracies means that natural resource rents constitute about 46 percent of the GDP, while for anocracies and democracies this figure barely reaches 14 percent (equivalent to the resource riches Q3 for autocracies).[49] Thus, the effect of being a resource rich autocracy might be "diluted" when bundled up with the (considerably poorer) resource rich non-autocracies.

I split the sample by ex-ante regime type, as oppose to yearly regime, primarily to guarantee consistency: while countries can change regimes at some point in time[50], their ex-ante regime type is always constant. The natural resources curse literature helps me support this choice, since it provides evidence that ex-ante institutions are the ones that mainly affect public sector employment size. For instance, Mehlum et al. (2006) claim that the effect of natural resources on economic performance depends on the quality of *preceding* state institutions. They find that countries with "grabber friendly" institutions see their aggregate income diminish when resource

[47] Azerbaijan, Bahrain, Belarus, China, Cuba, Egypt, Indonesia, Iran, Kuwait, Laos, Morocco, Oman, Qatar, Saudi Arabia, Syria, Viet Nam and Zimbabwe.

[48] I further elaborate on it in section 2.5.3. For a natural resource rents histogram by regime type, please see figure 2.8.

[49] Table 2.7, in the descriptive statistics section, summarizes natural resource rents by ex-ante regime type.

[50] Regime type, nonetheless, is relatively stable in the period I study (see appendix table 2.23). 87 percent of countries that were autocracies in 1995 remain as such all the time, while for democracies this holds in 96 percent of cases. Anocracies are more unstable, becoming democracies in 33 percent of cases and autocracies 6 percent of the times.

riches increase; in countries with "producer friendly" institutions, they see the opposite. Likewise, Robinson et al. (2006) hypothesize that resource booms are dissipated through excessive public employment and patronage if ex-ante institutions are weak. Hence, this literature suggests that my rents coefficients (and the interactions with inequality) could substantially differ depending on a country's ex-ante regime type.

2.4.2 Critical variables

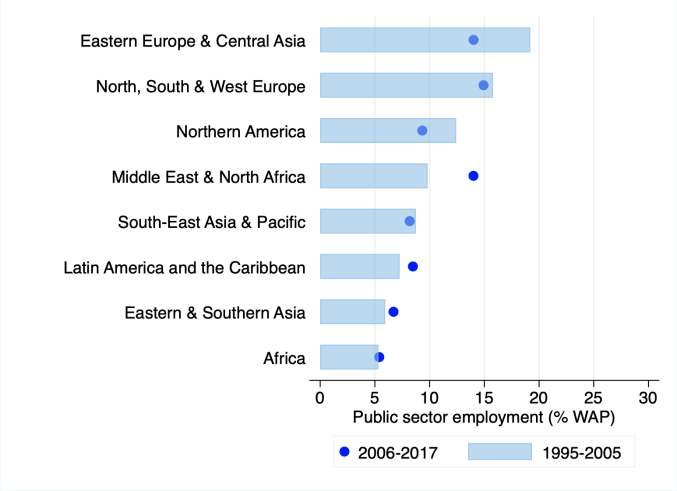
2.4.2.1 Public sector employment as a share of the WAP

My main dependent variable is public sector employment as a share of the WAP. Total public sector employment includes employees from the general government (local, state and central) and public enterprises, covering all persons employed directly by these institutions, regardless of employment contract types. The WAP includes all the individuals in working age, typically between 15 and 64 years old.

In the period under study, there is a large variance of public sector employment as a share of the WAP between regions, but moderate within. As seen in figure 2.4, in the last 23 years public employment shares have: i) declined in Eastern Europe and Central Asia (ECA) and Northern America (NAm); ii) increased in Middle East and North Africa (MENA), Latin America and the Caribbean (LAC), and Easter and Southern Asia (ESA); and iii) remained stable in North, South and West Europe (NSWE), South-East Asia & Pacific (SEAP) and Africa.

Operationally, I have 3 distinct possibilities for public employment size metrics: either measuring *total* public employment, public employment *as a share* (of the employed, total population, or the WAP), or using government *wage bill* data.

Figure 2.4: Public employment as a share of the WAP. Regional averages, 1995-2017 period.



Source: own elaboration, based on ILO (2020)

While government wage expenditure (either per capita, or as a share of total wage expenditure or GDP) was favored by authors such as Rodrik (2000) and Ali and Elbadawi (2016) in their analyzes, I will not consider this metric at the moment. To start with, my main aim is to study public sector **employment size** determinants. While wage bill should correlate with the number of public employees, the relationship is not as simple as expected. For instance, a quick exploration of the data shows no correlation between wage expenditure as percentage of total government expenditure and public sector employment shares (table 2.2), as well as a moderately positive correlation with wage expenditures as a share of GDP. A more detailed exploration (see appendix table 2.14) reveals that both the magnitude and sign of the correlation greatly vary by region and by the type of wage data. This adds an extra layer of complexity that should be addressed in subsequent work.

Of the 2 remaining choices, I prefer to use a scaled version of public sector employment, instead of just total employment, since this eases comparisons across countries and also reflects the

Table 2.2: Correlation matrix for measures of government employment size. 1995-2017 period.

	Public sector employment			Wage expenditure
	% of WAP	% of employed	% of population	% Gov. Exp
Public sector employment (% employment)	0.89			
Public sector employment (% population)	0.98	0.89		
Wage expenditure (% Gov. Exp.)	-0.03	-0.02	-0.04	
Wage expenditure (% GDP)	0.43	0.39	0.42	0.44

Source: Author's calculations from ILO (2020) and IMF GFS (2018)

relative importance of the public sector employment. My main dependent variable will be public sector employment as a share of the WAP, which reflects public employment's magnitude among those that could potentially work. Public sector employment as a share of total employment is also an attractive option, but will be used only for robustness tests purposes, as it is difficult to disentangle whether a change in the public sector employment ratio is due to variations in public sector or in total employment (or both).[51]

Back to a more fundamental discussion, the main aim of this paper is to identify under which conditions we will have increases in clientelistic, vote driven, public sector employment. Nonetheless, one of the main challenges in the empirical study of clientelism is the difficulty in both defining and measuring the concept.[52] In this case, how can we find a measure that distinguishes "service driven" increases in public sector employment from clientelistic ones? There is, the facto, no single measure that permits to do so. Thus, I can only explore if my theoretical model's predictions indeed hold in practice, which in turn would suggest whether higher natural resource rents are associated with increases in clientelistic public sector employment.

[51] This is less of a problem when the denominator is the WAP, as this variable, unlike total employment, changes steadily and predictably over time.

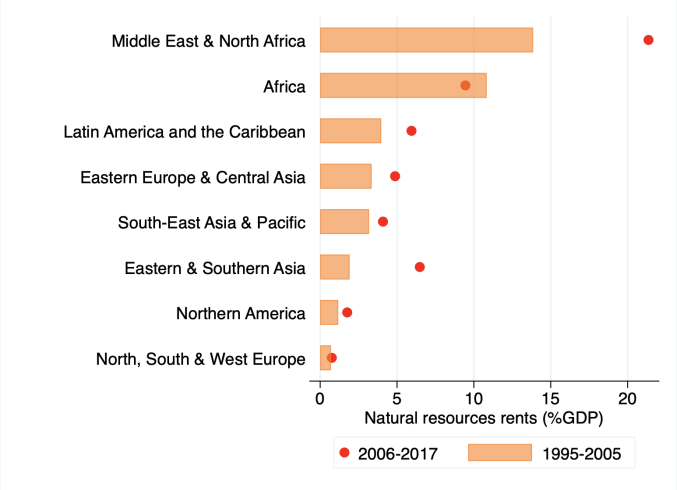
[52] See Kitschelt (2013), pages 323-29, for a thorough summary of such issues.

2.4.2.2 Resource dependence and natural resource rents shares

My main independent variable is natural resource rents shares, following Ross (2006), Collier and Hoeffler (2009), Bhattacharyya and Hodler (2010), Anthonsen et al. (2012), Bhattacharyya and Hodler (2014), Boschini et al. (2013), Kim and Lin (2017), among others. This variable measures the revenues from natural resources (including energy, minerals, and forestry) as a share of GDP. The revenues are captured by unit rents, which quantify the difference between the unit price of a good/commodity and the unit cost of extraction/production, multiplied by total production.

There is large variance of natural resource rents shares both between and within regions. Since 1995 (see figure 2.5) there was an increase in natural resource rents dependence in all regions but the NSWE and Africa. The dependence became all the more pronounced particularly in MENA and ESA.

Figure 2.5: Natural resource rents as a share of GDP. Regional averages, 1995-2017 average



Source: own elaboration, based on WDI (2020)

Natural resource rents over GDP constitutes a good measure for **natural resource dependence**. On the one hand, it monetizes the value of resource production relative to all other production, comparing the magnitude of these resources with the overall economy. On the other hand, it also reveals the size of a **stream of revenues that could be appropriated by political leaders for their purposes**.

Those opposing the use of scaled resource rents (rents divided by national income or total exports) argue that there is strong endogeneity between these and institutional variables, so they recommend substituting them with potentially exogenous measures of resource *abundance* such as stocks of resource reserves (Brunnschweiler and Bulte, 2009; Ebeke et al., 2015), unit export value of oil (Arezki and Bruckner, 2009), and the value of sub-soil assets (Collier and Goderis, 2012). Leaving data quality issues aside, a shortcoming of using resource abundance data is that it is not always readily available, and when available is predominantly about oil and gas.

Nevertheless, measures of reserves and stocks might not be exogenous either, since highly industrialized (and with better institutions) countries are more likely to have explored for longer and thus find larger reserves. [53] Hence, more developed nations *could seem* resource wealthier than less developed ones. Furthermore, as stressed by Kim and Lin (2017), reserves and stocks do not capture the relative importance of such resources for an economy, which in turns determines how such economy is to be affected.

[53] Cavalcanti et al. (2016), Cust and Harding (2019), Kim and Lin (2017), Torvik (2009) and Van Der Ploeg and Poelhekke (2010) are among the most recent contributors to this discussion.

2.4.2.3 Inequality as uneven access to high quality education and health services

For this analysis, I use two measures of **non-income based inequality**: access to high quality education and to high quality health services. I have no a priori reason to prefer one over the other, so I perform my main analysis with education access inequality, and then use healthcare access inequality data for robustness purposes.

Defining and measuring inequality is a complex endeavor, still under debate between both practitioners and academics. The simplest inequality measurement sorts the population from poorest to richest and shows the percentage of "income" [54] attributable to each quintile or decile of the population. Other popular measures of inequality include the Gini coefficient, Theil's T and L, or Atkinson's class of inequality measures. Each of these have advantages and disadvantages, and their usage depends on context and comparability needs. (Cowel, 2009)

Nonetheless, traditional measures of inequality (including the very popular Gini coefficient) might not be appropriate for this paper. Not only is this data scarce, but such measures generally suffer from comparability issues, as the definition of income/consumption/wealth varies by country and survey. Furthermore, the most pressing issue is that *traditional inequality measures also reflect the unequal distribution of rents*, and as such would not allow us to separate the conditional relationship among resource rents, inequality and public sector employment.

In an attempt to remedy this, I use Pemstein et al. (2021) non-income based measure on inequality of access to high quality basic education, as well as their inequality of access to health-

[54] With income used as a broad term that could include either wealth, consumption or income itself.

care.[55] Both of these indicate which percentage (ranges) of the population have access to high quality basic education and healthcare, respectively.

Education access inequality measures to what extent high quality basic education (typically provided between 6 and 16 years of age) is guaranteed to all children, enabling them to exercise their basic rights as adult citizens. Similarly, the health access inequality variable measures to what extent high quality basic healthcare is guaranteed to all citizens, enabling them to exercise their basic rights as adult citizens. Poor-quality healthcare fails to adequately treat preventable and treatable illnesses that hinder people from working, participating in social or political organizations, or voting (if allowed). Both inequality variables are ordinal, ranging from 0 to 4, with 0 corresponding to complete equality and 4 to extreme inequality.[56] Appendix 4.B.2 provides a detailed explanation of what is included in each subcategory.

For my purposes, Pemstein et al. (2021) inequality variables are preferable to traditional income inequality measures. In practical terms, data is less scarce, and conceptually they don't directly include the unequal redistribution of rents. However, there are also downsides to it. First, the data gaps in these variables are filled via a latent variable model, so the measurement method is of course less transparent than that of primary data. Second, as acknowledged by the authors, there might also be cross-national comparability problems, albeit not more than those arising from standard approaches.

[55] Their correlation with the Gini index is 0.60 and 0.69 for health and education access inequality, respectively.

[56] In the original variable, the order of the values was opposite, such that 0 corresponds to extreme inequality and 4 to complete equality. I inverted the ranking to facilitate interpretation of the results, given my framework.

2.4.2.4 Democracy score and regime type

Throughout my paper, I will reference a country's democracy score and regime type. Both are Polity IV based variables[57], a composite index that categorizes countries within a broad spectrum of regime authority, ranging from hereditary monarchies (-10) to consolidated democracies (+10). The index increases as countries become more democratic, achieving higher degrees of i) competitiveness of political participation, ii) openness and iii) competitiveness of executive recruitment, and as there are less iv) constraints on the chief executive and v) regulation of participation. On the opposite, the indicator becomes more negative as countries are more autocratic, exhibiting lower degrees of the aforementioned variables.

The **democracy score** is simply a rescaled version of the Polity IV index, meant to ease interpretation. Following Ali and Elbadawi (2016), I built the democracy score as per this formula:

$$DemocScore_{ct} = \left[\frac{Min(Polity) - Polity_{ct}}{Min(Polity) - Max(Polity)}\right] * 10 \tag{2.12}$$

Where Min(Polity) and Max(Polity) represent the minimum and maximum possible scores of the polity variable (-10 and 10, respectively), while $Polity_{ct}$ shows the score of country c in period t. $DemocScore_{ct}$ then ranges between 0 and 10, increasing as the polity variable becomes more positive.

Regime type is a set of 3 dummies, indicating whether a country is a democracy, an autocracy, or an anocracy (mixed regime with characteristics of both democracies and autocracies). I

[57] Whenever the original Polity IV variable was missing, I replaced it by the latest version of Teorell et al. (2019) imputed polity score. This scale adds imputed values for countries where data on Polity is missing, by regressing Polity on the average Freedom House measure for level of democracy. Hadenius and Teorell (2005) convincingly assert that this averaged index is more valid and reliable than both Polity IV and Freedom House average freedom. Nonetheless, in compliance with my data compilation principles, I will keep polity IV (a primary source) as the main variable and use only the imputed scores whenever there is a missing value.

did this as per the Center for Systemic Peace (2018) suggestion of grouping Polity IV values (in any given year) in 3 broad categories: autocracies (-10 to -6), anocracies (-5 to +5, plus special values -66,-77, and -88) and democracies (+6 to +10). When I use regime types in the empirical analysis (e.g. for subsetting the sample), I will typically group democracies and anocracies, and treat autocracies as a distinct group.

2.5 Descriptive examination

2.5.1 The data

The main dataset includes 138 countries for the period between 1995 and 2017 (23 years). It draws on variables from 11 different sources, covering measures of public employment size, natural resource rents, regime type and democracy scores, productivity, inequality and other socioeconomic covariates (see table 2.3). This is, to my knowledge, the first attempt to gather panel data covering such a large number of countries for analyzing the predictors of public employment size. Appendix 4.B.2 provides further detail on the methodological considerations in creating the dataset, as well the treatment for missing values.

The time span for my analysis is mainly constrained by the availability of public sector employment data. Only since 1995 does the International Labor Organization (ILO 2020) consistently publish public sector employment data for a sufficiently large number of countries. I use the (latest) ILO data, which covers the period until 2017. My analysis hence covers the period from 1995 to 2017, a total of 23 years.

In selecting which countries to include in my analysis, I faced a trade-off between minimizing selection bias and the need to balance the sample. This trade-off arises because, for many

Table 2.3: Variables and datasets

Variables	Main source	Secondary source
Natural resource rents, trade, urbanization and dependency ratio	WDI (2020)	
Education and healthcare access inequality	Pemstein et al. (2021)	
Democracy score & regime type	Polity IV Project (2018)	Teorell et al. (2019)
Output per worker	Based on WDI's (2020) GDP and ILO's (2020) employment	
Public employment	ILO (2020)	
Public employment (GCC countries)	Official sources *(i)*	
Population	WDI (2020)	Feenstra et al. (2019)

(i) Central Statistical Bureau of the State of Kuwait (2019), Labour Market Regulatory Authority of the Kingdom of Bahrain (2019),General Authority for Statistics of the Kingdom of Saudi Arabia (2015), National Center for Statistics and Information of the Sultanate of Oman (2019) and the GLMM Programme Dataset (2019).

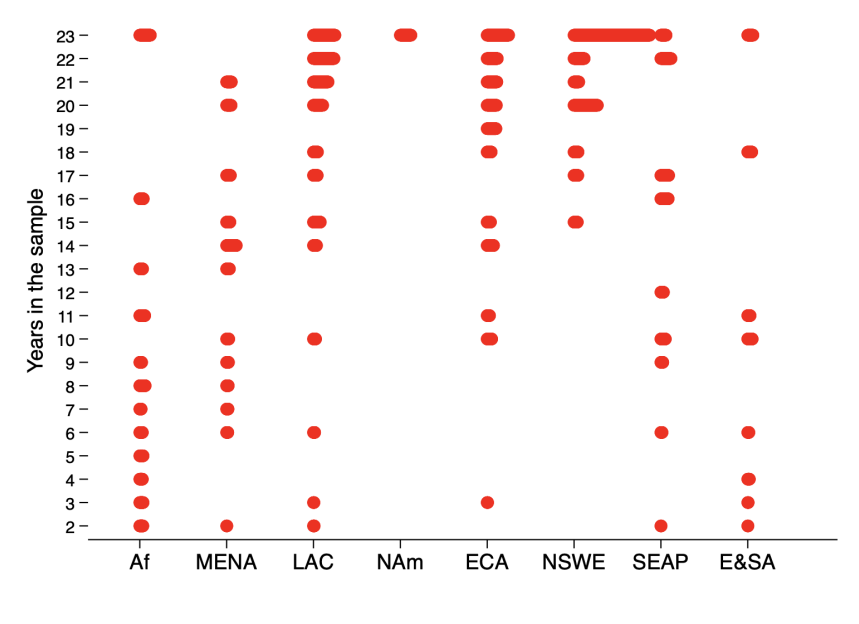
countries, the ILO public employment data has missing observations for a significant number of years between 1995 and 2017 (Figure 2.6). I chose the sample in view of minimizing selection bias and of avoiding arbitrary inclusion criteria, by including all counties which have at least 2 observations in the period under study. The resulting sample consists of 138 countries.

This choice, however, comes at the expense of balance. Of the 138 countries in the sample, only 33 percent (45 countries) have observations for all 23 years. For 41 percent (56 countries) of countries there are observations between 16 and 22 years (2/3 of the possible total observations). For 19 percent of countries (26 countries), there are observations between 8 and 15 years. For 7 percent of countries there are only observations for less than 7 years (1/3 of the possible total observations). A quick glance at the sample size per subregion (Figure 2.6) shows that the number of countries with coverage below 7 years is particularly high in Africa and ESA.[58] Setting more stringent criteria for balancing the panel would hence lead to an underrepresentation in particular of these regions.

Restricting the sample to a shorter period is not an easy fix to the issue of an unbalanced

[58]For a full list of the observations per country and region, please refer to the appendix tables 2.11, 2.12 and 2.13.

Figure 2.6: Total number of years in the sample, per subregion. 1995-2017 period



Note: The acronyms correspond to the following regions: Africa ("Af"), Middle East and North Africa ("MENA"), Latin America & the Caribbean ("LAC"), North America ("NAm"), Eastern Europe & Central Asia ("ECA"), North, South & West Europe ("NSWE"), South-East Asia & Pacific ("SEAP") and Eastern & Southern Asia ("ESA").

panel, since in general the number of observations is relatively similar across quinquennia (see table 2.4)[59]. Furthermore, I shy away from dropping off the countries with fewer observations to avoid arbitrary sample size threshold choices, as well as sampling bias. Importantly, those countries with less observations[60] have on average smaller public sector employment and more natural resource rents, while also being more unequal and considerably less productive and democratic (see table 2.5). Consequently, and despite the econometric inconveniences, I chose to work with an unbalanced panel for the sake of avoiding bias.

[59] But for the period between 2010-2017, which has more observations because it includes 8 years (not 5).

[60] Among those with 7 or less years of data (1/3 of the total sample size) we have Angola, Bangladesh, Benin, Bhutan, Cameroon, Cape Verde, China, Congo, Cote d'Ivoire, Gabon, Ghana, Haiti, Iraq, Laos, Liberia, Madagascar, Malawi, Mali, Myanmar, Namibia, Nicaragua, Niger, Nigeria, Rwanda, South Korea, Suriname, Tajikistan, Timor-Leste, Togo, Yemen, Zambia and Zimbabwe.

Table 2.4: Observations per subregion and quinquennia. Full sample.

Subregion	**Quinquennia**				
	95-99	***2000-2004***	***2005-2009***	***2010-2017***	***Total***
Africa	56 (25.54%)	37 (19.02%)	36 (19.02%)	7 (36.41%)	205 (100 %)
Middle East & North Africa	16 (7.78%)	32 (19.76%)	55 (32.93%)	67 (39.52%)	170 (100%)
Latin America & Caribbean	90 (23.67%)	71 (17.72%)	81 (18.26%)	143 (20.83%)	385 (100%)
Northern America	10 (21.74%)	10 (21.74%)	10 (21.74%)	16 (34.78%)	46 (100%)
Eastern Europe & Central Asia	65 (18.79%)	74 (21.97%)	83 (23.89%)	129 (35.35%)	351 (100%)
North, South & West Europe	103 (19.03%)	123 (22.95%)	125 (23.32%)	186 (34.70%)	537 (100%)
South-East Asia & Pacific	40 (22.81%)	43 (23.98%)	41 (22.22%)	58 (30.00%)	182 (100%)
Eastern & Southern Asia	16 (19.32%)	14 (15.91%)	21 (22.72%)	40 (42.05%)	91 (100%)
Total	396 (19.94%)	404 (20.94%)	452 (23.22%)	715 (35.90%)	1,967 (100%)

Table 2.5: Comparison between countries with small and large number of observations. Key variables.

Variable	**Mean**		**Diff.**	**T. test**	
	$N \geq 8$	***N<8***		***Stat.***	***P.val***
Public employment (% WAP)	11.95	6.75	5.19	7.89	0.0000
Nat. res. rents (% GDP)	4.64	12.77	-8.12	-9.45	0.0000
Unequal access to education	1.18	2.48	-1.30	-11.97	0.0000
Output per worker	48,976	17,924	31,052	9.64	0.0000
Democracy score (0-10)	8.09	5.99	2.10	8.24	0.0000

Note: This table portrays a set of two-sample t tests with unequal variances, where Ho: diff = 0 and Ha: dif!=0. Here we see the differences between the sample of countries with large number of observations ($n >= 8$ per country, with total N=1,841) vs. the one with countries with small number of observations ($n < 8$ per country, with total N=126).

A larger sample size minimizes selection bias. Given the large regional variance of both public sector employment and natural resource rents, it is of essence to guarantee the inclusion of as many countries as possible. With that said, I will also present robustness tests with an alternative version of the sample that excludes countries with a small number of observations. As previously mentioned, the definition of a small sample size threshold is inevitably arbitrary. For

the sake of simplicity, I define it as having 7 or less years of data, or 1/3 of my total sample size. This category includes 35 countries out of 138, with a total of 126 observations (6.41% of the full sample); details can be found at appendix table 2.13.

2.5.2 Descriptive statistics and model predictions

This section provides basic descriptive statistics and evaluates whether these are consistent with the model's main predictions.

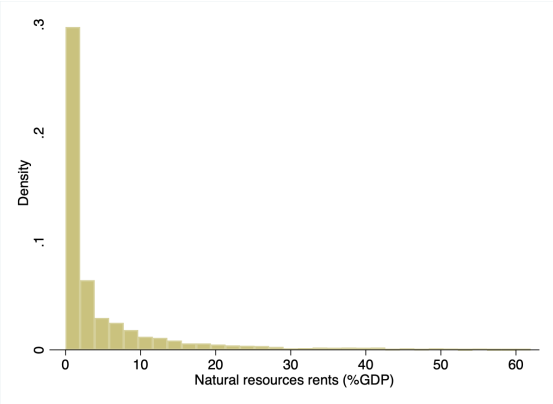
2.5.3 The distribution of natural resource rents

For the subsequent analysis, it is key to highlight that the distribution of the main independent variable of interest —natural resource rents — across country-year observations is highly skewed towards zero.[61] As Figure 2.7 shows, most countries have relatively few natural resource rents as a share of their GDP. For 80 percent of country-year observations (The bottom four quintiles, Q_W1 to Q_W4) natural resource rents are lower than 7.5 percent of GDP. There is, however, a thin upper tail with resource-rich countries (the top quintile, Q_W5), with the sample's maximum being 62 percent.

Moreover, natural resource wealth correlates strongly with a few other covariates. Table 2.6 reports summary statistics for the entire sample of 1,967 country-year observations, covering 138 countries. For each variable, columns 1 to 5 report summary statistics for the overall sample, while columns 6-10 report mean by quintile of natural resource wealth. Table 2.6 suggests that higher resource rents are associated with smaller output per worker, urbanization and trade shares, as well as higher education and health access inequality.

[61] The log version of this variable, nonetheless, is normally distributed.

Figure 2.7: Natural resource rents dependency histogram. Worldwide sample. 1995-2017 period.



Note: For the worldwide sample, the natural resource quintiles have the following intervals. Q_W1: 0.0-0.2; Q_W2: 0.2-0.9; Q_W3: 0.9-2.2; Q_W4: 2.2-7.4; and Q_W5:7.4-62. The index "W" will be used in the following to reference these "worldwide" quintiles.

Table 2.6: Summary statistics. Worldwide sample, 1995-2017.

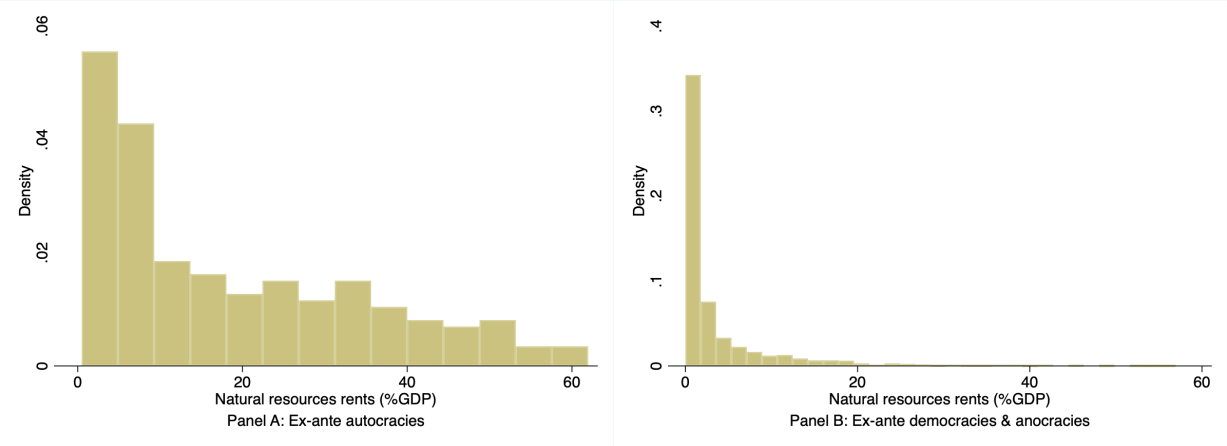
Variable	**Mean**	**SD**	**Min.**	**Max.**	**Corr. P.E.**	**Mean for NRR. Q1**	**Q2**	**Q3**	**Q4**	**Q5**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**
Natural resource rents (% GDP)	5.2	9.29	0	62	0.04	0.1	0.5	1.4	4.2	19.9
Education inequality	1.27	1.22	0	4	-0.55	0.4	0.9	1.2	2	1.9
Health inequality	1.19	1.16	0	4	-0.46	0.3	0.9	1.2	1.8	1.7
Output per worker (in 1,000)	46.9	35.8	1.4	229	0.31	73.4	49.7	36.6	30.7	44.5
Democracy score (0-10)	7.95	2.81	0	10	-0.08	9.2	9	8.7	7,6	5.3
Urban population (% of total)	63.8	19.2	12.9	100	0.31	69	65	64	59	60
Trade (% GDP)	88.3	50	15.6	437	0.20	99	91	81	70	83

Note: The full sample size (pooled data for all period) for all variables corresponds to 1,967 observations. Columns 1 to 5 report summary statistics for the overall sample; while columns 6-10 report mean by quintile of natural resource rents (NRR). Column 5 reflects the correlations to public employment as a share of the WAP.

In particular, Table 2.6 shows that resource-rich countries tend to be much more autocratic than resource-poor countries. Countries in the top quintile of natural resource wealth had an average democracy score of 5.3, compared to an average of 9.2 for the countries in the bottom quintile. Figure 2.8 shows these distinctive distributions of natural resource rents in democracies and anocracies, on the one hand (panel B), and in autocracies, on the other (panel A). Accordingly, for the subsequent analysis, I use different quintile definitions for democracies/anocracies

(Q_D) and for autocracies (Q_A), as per Figure 2.8.

Figure 2.8: Natural resource rents dependency histogram, by regime types. 1995-2017 period.



Note: For the *autocracies sample* (N=199), the natural resource rents quintiles have the following intervals. Q_A1: 0.5-3.6; Q_A2: 3.7-8.5; Q_A3: 8.6-20; Q_A4: 20-35; and Q_A5: 35-62. The index "A" will be used in the following to reference these "autocracies" quintiles. For the *anocracies and democracies sample* (N=1,768), the natural resource rents quintiles have the following intervals. Q_D1: 0.0-0.2; Q_D2: 0.2-0.7; Q_D3: 0.7-1.7; Q_D4: 1.7-5.3; and Q_D5: 5.3-57. The index "D" will be used in the following to reference these "anocracies and democracies" quintiles.

For ease of reference, I will use the following terminology to refer to three distinct absolute levels of natural resource wealth. The term "resource poor" refers to countries with natural resource rents that are lower than 2.2 percent of GDP, in the bottom 3 worldwide quintiles (Q_W1 to Q_W3, see figure 2.7). The term "medium resource wealth" comprises countries with natural resource rents between 2.2 and 7.4 percent of the GDP (Q_W4). Finally, "resource rich" countries are those with natural resource rents above 7.4 percent of GDP, in the top worldwide quintiles (Q_W5). Table 2.7 relates these definitions to the worldwide quintile ranges, and *roughly* maps them to the quintiles of the autocracies and democracies & anocracies subsamples.

Table 2.7: Definitions of natural resource wealth, and observations per category.

Terminology	**NRR / GDP (%) intervals**	**Corresponding quintiles for Autocracies** (Q_A)	**Democracies & Anocracies** (Q_D)
Resource poor	0.00-2.2 (Q_W1 - Q_W3)	Q_A1	Q_D1 - Q_D3
N	*1,160*	*40*	*1041*
Medium resource wealth	2.2-7.4 (Q_W4)	Q_A2	Q_D4
N	*387*	*40*	*347*
Resource rich	7.4-62 (Q_W5)	Q_A3 - Q_A5	Q_D5
N	*387*	*119*	*347*

Note: The natural resource wealth definitions, provided above, are based on the worldwide sample intervals. N corresponds to the number of observations in each cell. The information on the *rough* overlap with the anocracies & democracies and autocracies subsamples, is provided for reference purposes. For precise thresholds of the natural resource rents quintiles by subsample, please see appendix table 2.15.

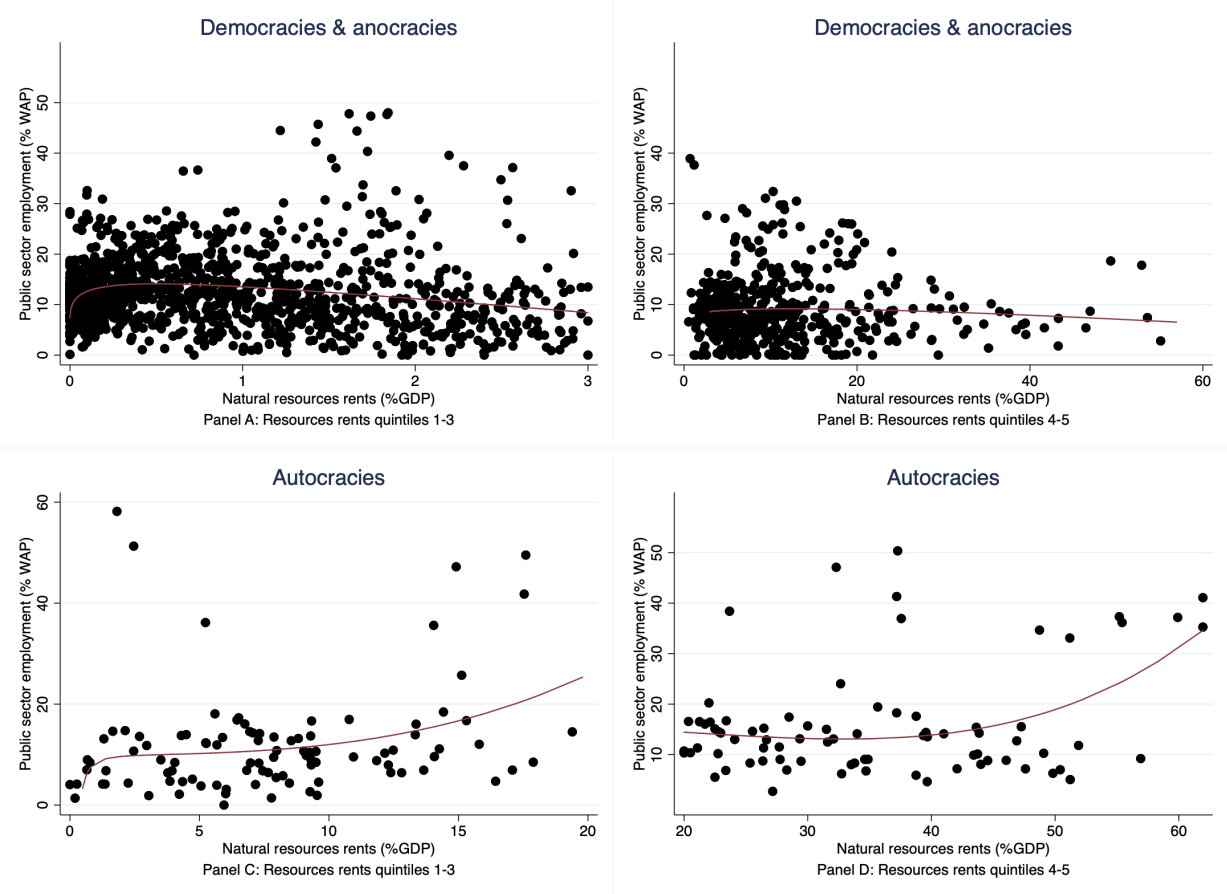
2.5.3.1 MP4 & MP5: The relationship between public employment and natural resource rents differs in democracies and autocracies

A simple, unconditional, exploration of the relationship between total natural resource rents and public employment size suggests no link between total natural resource rents and public employment size. The correlation between these two variables, across all country-year observations, is zero (table 2.6, column 5). There are at least two possible reasons for the apparent lack of relationship. First, my theory predicts the relationship between public employment and natural resource rents is the *opposite* for democracies than for autocracies; thus lumping them up together might net out these effects. Second, it is likely that the relationship under study differs by resource rents levels.

In response to the first point, we can subset the sample by regime type. For *democracies and anocracies*, there is still no apparent correlation between public employment and natural resource

rents (figure 2.9, panels A and B). This is against my prediction of a positive correlation (MP4). However, in *autocracies* public employment and resource rents have a *positive* correlation (figure 2.9, panels C and D); nonetheless, this is also the opposite of my predictions (MP5). Subsetting the sample by natural resource rents wealth levels, in response to the second point, does not affect the findings for either regime type.

Figure 2.9: Public employment shares and natural resource rents correlation. 1995-2017 period



Source: own elaboration, based on ILO (2020) and WDI databases (2020)

Finally, if we subset the sample by region, we find: i) positive correlations for MENA, LAC, NAm, NSWE and SEAP, ii) a negative correlation for Africa, and iii) no relationship for ECA and SEAP (table 2.8). This suggest the importance to thoroughly explore the relationship

under study, as to disentangle the underlying patterns.

Table 2.8: Public sector employment as a share of the WAP correlation with main variables. 1995-2017 period, by region

Region	Nat. res. rents (% GDP)	Inequality		Output per worker (Productivity)
		Health	Education	
Africa (but north)	-0.27	-0.60	-0.41	0.87
Middle East & North Africa	0.46	-0.23	-0.32	0.46
Latin America & the Caribbean	0.19	-0.60	-0.62	0.66
North America	0.23	-0.09	-0.24	-0.17
East Europe & Central Asia	-0.03	-0.13	-0.37	-0.13
North, South & West Europe	0.42	-0.01	-0.08	-0.20
South-East Asia & Pacific	0.08	-0.40	-0.52	0.29
Southern & Eastern Asia	0.43	-0.04	-0.13	-0.17

Source: Author's calculations from ILO (2020), Penn World Tables 9.0 (2019), V-Dem (2021) and WDI (2020).

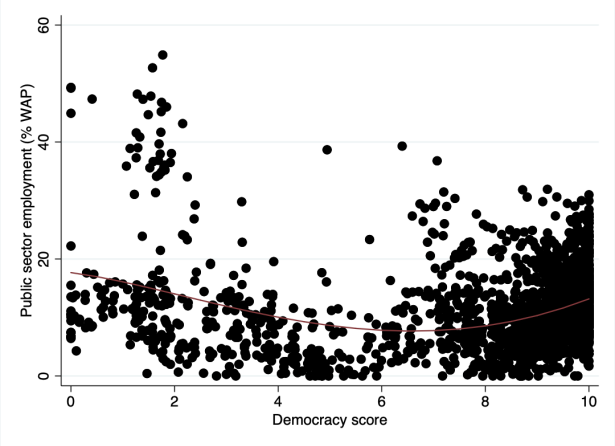
2.5.3.2 MP3: Public employment decreases with more democratic political institutions

Descriptive statistics suggest a U-shaped relationship between regime types and the size of public employment. As per the summary statistics (table 2.6), there is no linear correlation between public employment and the democracy score. The plotted data (figure 2.10), however, reveals a U-shaped relationship. In autocracies or anocracies (democracy scores below 8) more democracy correlates to less public employment. In democracies (democracy scores of 8 +), by contrast, the correlation clearly reverses.

2.5.3.3 MP2: Public employment increases with inequality

Worldwide, there is a strong negative correlation between public employment shares and education inequality (see figure 2.11). This descriptive statistic is inconsistent with model prediction 2 (MP2). The negative correlation consistently holds across regions (table 2.8), except

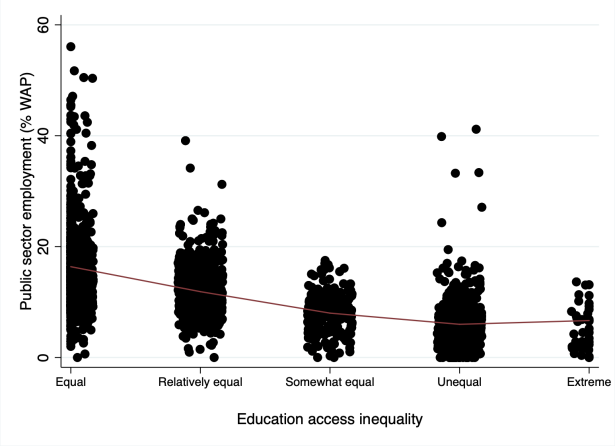
Figure 2.10: Public employment shares and democracy score correlation. 1995-2017 period



Source: own elaboration, based on ILO (2020) and the Polity IV Project (2018) databases.

perhaps for the cases of SEA and NSWE (where it is very close to zero).

Figure 2.11: Public employment shares and inequality correlation. 1995-2017 period

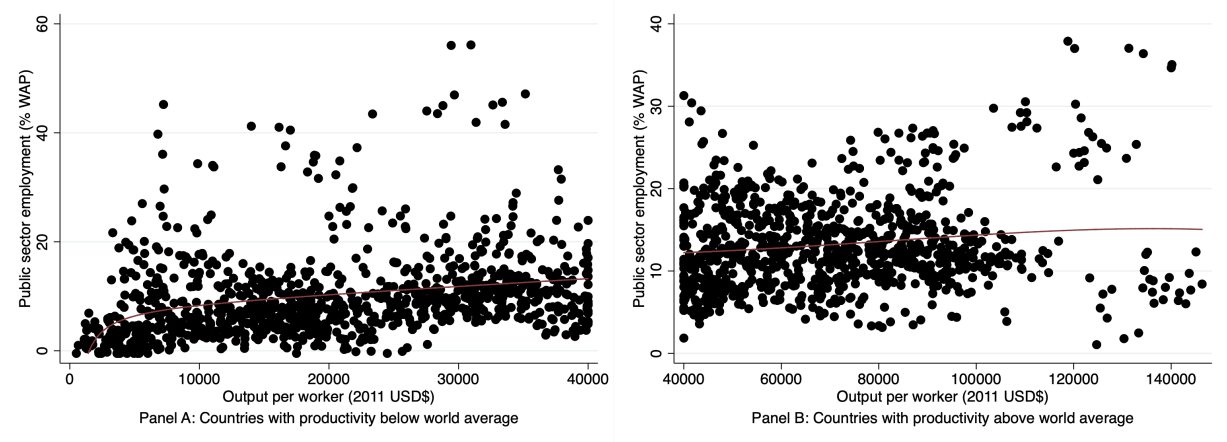


Source: own elaboration, based on ILO (2020) and V-Dem (2021) databases.

2.5.3.4 MP1: Public employment decreases with higher productivity

Worldwide, there is a moderate *positive* relationship between productivity (output per worker) and public employment shares (see figure 2.12). This is inconsistent with the strong negative correlation we would have expected according to theory (MP1). Decomposing the data by region (table 2.8) shows us: i) a negative correlation (albeit not as strong as expected) in NAm, ECA, NSWE and Southern & Eastern Asia (SEA); and ii) a strong *positive* correlation in Africa, MENA, LAC, and SEAP. This positive correlation perhaps indicates that high natural resource rents are "artificially" increasing my productivity measure[62] in these regions.

Figure 2.12: Public employment shares and output per worker correlation. 1995-2017 period



Source: own elaboration, based on ILO (2020) and WDI databases (2020)

[62]Natural resource rents are a component of the GDP. And output per worker is simply the GDP divided by the number of employed people (See appendix 4.B.2).

2.6 Results

Moving forward, I proceed in the following order. First, I start with a simple model that does *not* consider the differential effect of political institutions in the redistribution of natural resource wealth (section 2.6.1). Second, I expand my model and include an interaction between political institutions and natural resource rents (section 2.6.2.1). Next, I estimate my results with two separate samples, depending on whether a country is an autocracy ex-ante or not (section 2.6.2.2). Finally, I perform some robustness tests, and discuss the results and its implications (sections 3.7.2 and 2.7, respectively).

2.6.1 Basic model

I begin my analysis with the basic model (equation 2.10, and the full sample). Table 2.9's column (4) reports the results. Importantly, and unlike my extension proposals, with this model we cannot distinguish if the relationship between natural resource rents and public employment differs in democracies vs. autocracies (MP4 vs. MP5).

As per the basic model, natural resource rents *by themselves* have no effect on public employment, and neither does inequality. However, there is a significant *joint* effect of resource rents and inequality: resource rich[63] countries that also have *highly unequal* access to education have 15 percent higher public employment (as a share of WAP) than equal countries in the lowest quintile of natural resource rents[64]. This relationship is consistent with the predictions for inequality (MP2) and for natural resource rents in democracies (MP4), but not in autocracies

[63] That is, in the 5th quintile of worldwide natural resource rents ($Q_W 5$), as per the above definition.

[64] Since neither "somewhat equal education access" nor its interactions are statistically significant in any specification I tested, I will exclude them in future sections.

(MP5). This seems plausible: democracies and anocracies represent by far the largest share of country-year observations in the sample (90 percent). The prediction for democracies (MP4) will hence dominate that for autocracies (MP5).

Table 2.9: Public employment determinants, basic model. 1995-2017 period.

Ln (Pub. Emp./ WAP)	**(1)**	**(2)**	**(3)**	**(4)**
Nat. res. rents - Q_W5	-0.073	-0.055	-0.073	-0.055
	(0.08)	(0.08)	(0.08)	(0.08)
Nat. res. rents - Q_W4	0.013	0.012	0.013	0.012
	(0.08)	(0.07)	(0.08)	(0.07)
Nat. res. rents - Q_W3	0.013	0.008	0.013	0.008
	(0.05)	(0.05)	(0.05)	(0.05)
Nat. res. rents - Q_W2	0.044	0.033	0.044	0.033
	(0.04)	(0.04)	(0.04)	(0.04)
Ln Output per worker	-0.477***	-0.354***	-0.477***	-0.354***
	(0.11)	(0.13)	(0.11)	(0.13)
Ln Democracy score	-0.104**	-0.075*	-0.104**	-0.075*
	(0.05)	(0.04)	(0.05)	(0.04)
Extreme & unequal educ. access	-0.111	-0.092	-0.111	-0.092
	(0.08)	(0.08)	(0.08)	(0.08)
Somewhat equal educ. access	-0.101*	-0.031	-0.101*	-0.031
	(0.05)	(0.06)	(0.05)	(0.06)
Nat. rents Q_W5 * Extremely unequal	0.197**	0.146*	0.197**	0.146*
	(0.08)	(0.08)	(0.08)	(0.08)
Nat. rents Q_W4 * Extremely unequal	0.052	0.019	0.052	0.019
	(0.07)	(0.07)	(0.07)	(0.07)
Nat. rents Q_W3 * Extremely unequal	0.020	-0.008	0.020	-0.008
	(0.05)	(0.05)	(0.05)	(0.05)
Nat. rents Q_W5 * Somewhat unequal	0.108	0.000	0.108	0.000
	(0.07)	(0.08)	(0.07)	(0.08)
Nat. rents Q_W4 * Somewhat unequal	0.052	-0.013	0.052	-0.013
	(0.09)	(0.09)	(0.09)	(0.09)
Nat. rents Q_W5 * Somewhat unequal	0.046	0.011	0.046	0.011
	(0.04)	(0.04)	(0.04)	(0.04)
Year F.E.	No	No	Yes	Yes
Controls	No	Yes	No	Yes
R^2 (within)	0.158	0.229	0.158	0.229

Note: All regressions have 1,967 observations, country fixed effects and country clustered standard errors (In parenthesis). Included controls are urbanization, population, regime duration, dependency ratio and trade shares. Appendix table 2.18 shows the full results. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Importantly, countries with *more democratic political institutions* have *less* public employment, as predicted (MP3). In the basic model, a 1 percent increase in the democracy score is associated with an average decrease of 0.08 percent in public employment.

The basic model also supports prediction 1 on productivity (MP1). I find that, all else equal,

lower productivity is associated with higher public employment. Countries with 1 percent higher output per worker have on average 0.36 percent *less* public employment.

Finally, I find no evidence of public employment being used as social insurance, since both the dependency ratio and trade shares are statistically non-significant (Appendix table 2.18 shows the coefficients for such variables)[65]. The trade shares variable remains non-significant throughout the paper, regardless of the specification; thus, I will not include it in the following sections. The dependency ratio variable is not consistently significant, it only is in some cases, so it will remain as a control moving forward.

The results from the basic model hold (in sign and magnitude) with and without controls and year fixed effects, when replacing the continuous democracy score by regime type dummies (see appendix table 2.19), and as per alternative definitions of public sector employment[66] (appendix figures 2.32 and 2.33).

2.6.2 The differential joint effect of natural resource rents and political institutions

2.6.2.1 Extended basic model: Natural resource rents and democracy score interaction (MP4 and MP5)

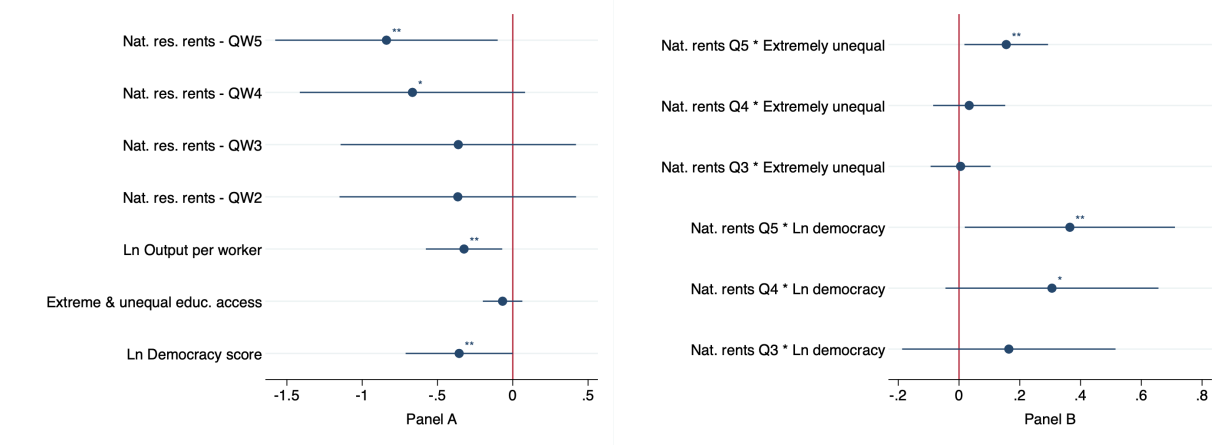
In this section I expand the basic model to include an interaction between political institutions and natural resource rents, as per equation 2.11. This enables me to test whether the

[65] In section 2.4.1.1, I explained the need to include such variables as controls. Positive coefficients for dependency ratio and negative for trade openness would favor the alternative hypothesis of public sector employment as social insurance.

[66] As a share of WAP, of the population, or plainly as total public sector employment. Results are not robust to defining public employment as a share of the employed, despite the similar magnitude of the coefficients, since the standard errors are larger under that specification.

relationship between natural resource rents and public employment differs in democracies (MP4) and autocracies (MP5). Figure 2.13 reports results from the *extended basic model*.

Figure 2.13: Ln public employment as a share of WAP determinants, regressions coefficients. 1995-2017 period.



Note: The regression has 1,967 observations, includes urbanization, population, regime duration and dependency ratio controls, country and year fixed effects and country clustered standard errors. Full regression results available in appendix table 2.20, column 6. $*\ p < 0.10$, $**\ p < 0.05$, and $***\ p < 0.01$

The extended basic model provides support for prediction MP4. The *interaction between democracy scores and natural resource rents is positive* — and it is larger the higher the level of natural resource rents. Consider countries with a democracy score that is 1 percent above the average democracy score for the sample. As figure 2.13 (panel B) shows, the size of public employment in these (slightly) more democratic countries does not statistically differ from their less democratic peers, when they are resource-poor ($Q_W1 - Q_W3$). However, when these slightly more democratic countries have either medium resource wealth (Q_W4) or are resource-rich (Q_W5), their public employment is larger than for their less democratic peers, by approximately 0.37 percent and 0.31 percent, respectively. In more democratic countries, the marginal

effect of higher natural resource rents hence is positive, consistent with prediction MP4.

When evaluating the extended basic model *at the average democracy score for the sample* (that is, if we observe only the rents main effects), *higher* natural resource rents are associated with *lower* public employment. The sample average democracy score is 7.95 and hence corresponds to anocracies. As Figure 2.13 (panel A) shows, public employment in otherwise average resource-rich ($Q_W 5$) and medium resource wealth ($Q_W 4$) countries, is distinctively lower than in otherwise average resource-poor countries. The magnitude of this difference is astonishing: public employment in otherwise average resource-rich or medium resource wealthy countries is 80 percent and 63 percent *lower*, respectively, than for otherwise average resource-poor countries.[67]

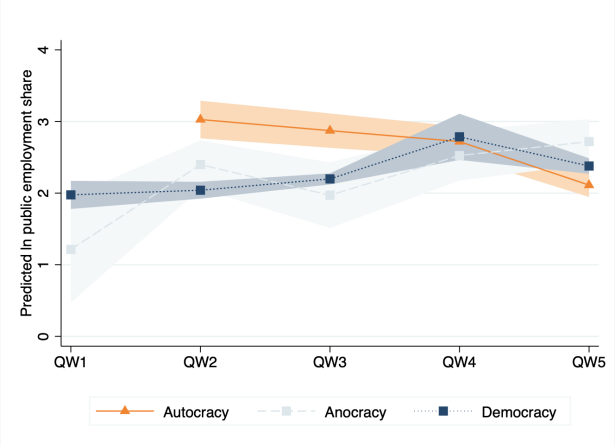
Compared to the base model without the interaction term (see section 2.6.1), this is a sharp change in the coefficients' sign, magnitude and significance. The negative sign is contrary to expectations for democracies (MP4), but fully consistent with my autocracies prediction (MP5). The fact that most medium and resource rich countries in this sample are not democracies (as per the descriptive statistics section 2.5.2) suggest that the coefficients' negative sign is likely due to the effect of autocracies. Consequently, I believe this finding supports MP5.

Figure 2.14 evaluates the marginal effects of natural resource rents for each regime type, at means for other variables. The estimated marginal effects are indeed consistent with MP4 and MP5: In democracies, public employment increases with resource wealth (MP4), whereas for autocracies it decreases with it (MP5). Anocracies, for which I had no clear theoretical priors, behave on average like democracies (albeit not as linearly).

As the basic model, the extended model supports that, on average, *countries with more*

[67]The coefficients size, together with its also very large standard errors, are another reason why I consider next section results, with separate samples, more accurate.

Figure 2.14: Predicted Ln public employment as a share of WAP determinants, by natural resource rents quintiles and regime type. 1995-2017 period.



Note: These figures shows the marginal effects of natural resource rents (at means) by regime type, together with 95% confidence intervals, of figure 2.13's underlying regression. For reversed values (non logs) of the marginal effects, please refer to appendix table 2.22.

democratic political institutions have lower public employment[68], consistent with theoretical priors (MP3). The extended model also provides support for the corollary (MP6), namely that MP3 holds particularly in resource-poor countries, and to a lesser extent in resource-rich countries. As noted and shown in figure 2.13 (panel B), medium resource wealth and resource-rich countries (Q_W4 and Q_W5) see public employment grow when their democracy score increases by 1 percent. This suggests that in these countries, the gap in public employment between democracies and autocracies is narrower than in resource-poor countries.

The estimated marginal effects by regime type (figure 2.14) also support MP6. In resource-poor countries (Q_W2-Q_W3), democracies and anocracies clearly tend to have lower public employment than autocracies. For medium wealth and resource-rich countries (Q_W4 and Q_W5), by contrast, figure 2.14 predicts that democracies and anocracies have equal or higher levels of

[68] A 1 percent increase in the democracy score correlates to 0.36 percent less public employment.

public employment than autocracies.

With regards to inequality (MP2), results are the same as in the basic model. Countries with extreme and unequal access to education *that are also resource rich* (Q_W5) have 15 percent *higher* public employment. As per my theory, the positive correlation between education access inequality and resource rents suggest that, in resource rich setups, more inequality leads to higher public employment, to be used for redistribution purposes (MP2). While, at this point, I cannot rule out that more public employment in fact causes higher education access inequality, the evidence that I will provide in section 2.6.3.3 suggests this is likely not the case.

In sum, the extended basic model yields supportive evidence of my model's main predictions. In democracies, public sector employment increases with natural resource wealth (MP4), whereas it decreases with natural resource wealth in autocracies (MP5). In the average country, it increases with more democratic institutions (MP3). This relationship holds particularly in resource-poor settings, but not in medium resource wealth and resource-rich settings (MP6). The results hold with and without year fixed effects (see annex table 2.20), presenting resource rents either in quintiles or as a continuous variable (see appendix figure 2.34), and per different public employment measures (see appendix figure 2.35).

2.6.2.2 Basic model for separate samples, by ex-ante regime type

In this section I run my basic model[69] on two separate samples, as an alternative means for testing whether resource riches affect public employment differently in democracies and autocracies. The samples are defined based on whether countries were autocracies (17 countries[70]), or

[69] Equation 2.10, with no interactions between natural resource rents and the democracy score.

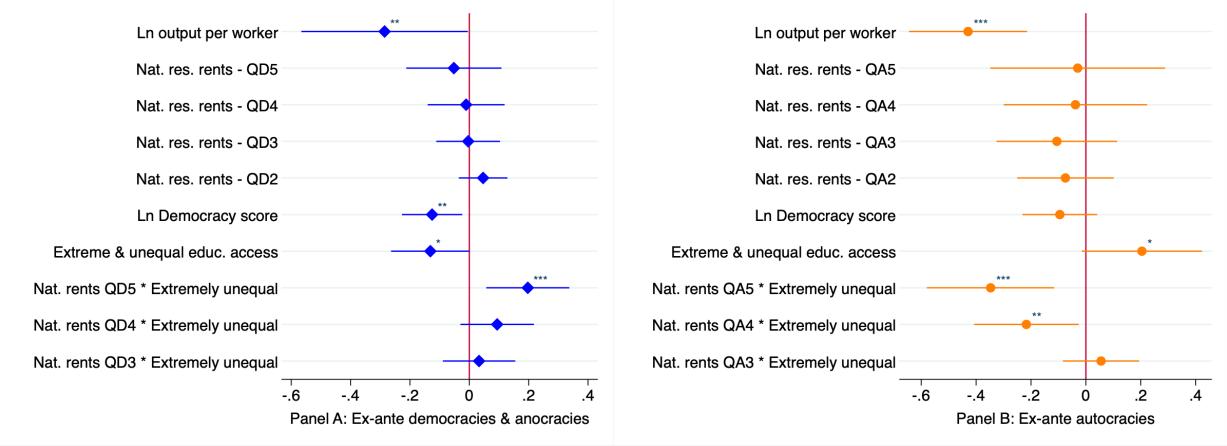
[70] Azerbaijan, Bahrain, Belarus, China, Cuba, Egypt, Indonesia, Iran, Kuwait, Laos, Morocco, Oman, Qatar, Saudi Arabia, Syria, Viet Nam and Zimbabwe. There are in total 199 observations.

democracies and anocracies (121 countries), in year 1995 (beginning of the period under study).

a. Ex-ante democracies and anocracies (D&A)

Figure 2.15, panel A, reports results for ex-ante D&A. Accordingly, natural resource rents only "affect" public employment in countries with high inequality in access to education. Consistent with MP2 and MP4, resource-rich D&A (Q_D5, with resource rents over 5.3% of GDP) that are also highly unequal, have on average 20 percent *higher* public employment than more equal countries in the natural resource rents lowest quintile (Q_D1).

Figure 2.15: Ln public employment over WAP determinants. Separate samples, by countries' ex-ante regime type. 1995-2017 period.



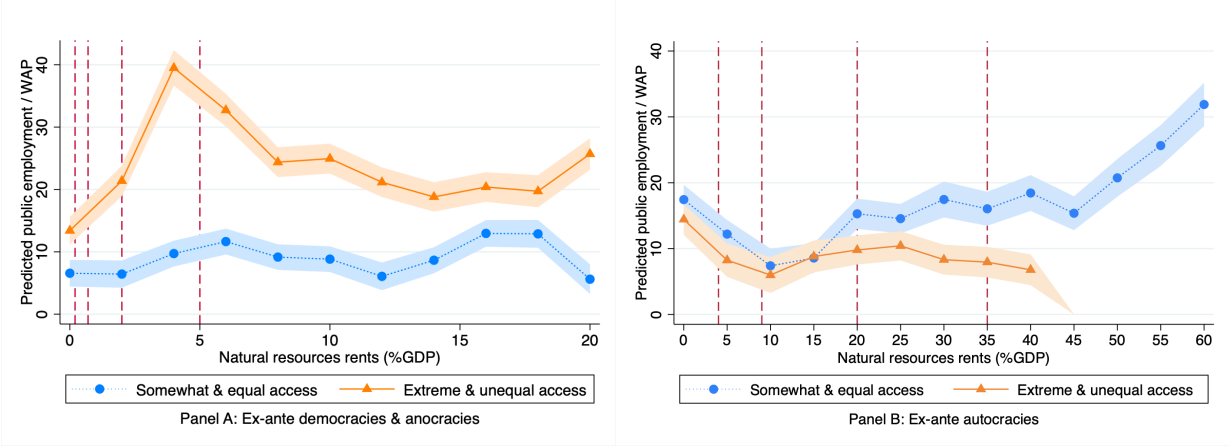
Note: All regressions have urbanization, population, regime duration and dependency ratio controls, country and year fixed effects. Panel A has N=1,768 and country clustered standard errors, while Panel B has N=199 and robust standard errors. Full results available in appendix table 2.24. For precise thresholds of the natural resource rents quintiles by subsample, please see appendix table 2.15.

The role of inequality in ex-ante D&A's requires further discussion. As per figure 2.15's panel A, highly unequal D&A's have 13 percent *smaller* public employment compared to a more equal average D&A, conditional on being resource poor (Q_D1). Importantly, this negative coef-

ficient is *not* the average effect of inequality, but rather simply a discrete change from the base level; it need not go against MP2.

In order to clarify this point, Figure 2.16 (panel A) shows the predicted public employment for D&A's, by natural resource rents and two levels of inequality ("extreme and unequal" versus "somewhat unequal and equal"), at means for other variables. These marginal effects estimates confirm MP2: public employment is higher for unequal ex-ante D&A than for more equal D&A. This figure also confirms MP4: In ex-ante D&A, public employment increases with resource rents, at least for resource poor and middle resource wealth countries (Q_D1 - Q_D4)[71].

Figure 2.16: Predicted public employment as a share of WAP, by educational access inequality and ex-ante regime type. 1995-2017 period.



Note: This figure presents the marginal effects (at means) plus 95 % confidence intervals of figure 2.15's underlying regression (see table 2.24, columns 2 and 5), by natural resource rents and educational access inequality. The red vertical dashed lines show the lower bounds for natural resource rents quintiles 2, 3, 4 and 5. The regression has urbanization, population, regime duration and dependency ratio controls, country and year fixed effects and country clustered standard errors for panel A. In panel B, standard errors are robust.

The other results for ex-ante D&A are consistent with model predictions and with previous

[71]For resource rich countries (Q_D5), these trend do not always hold. Countries with somewhat and equal access to education see their public employment *grow* when natural resource rents are above 12 percent of the GDP.

findings, in sign and magnitude. Both more democratic institutions (MP3)[72] and higher productivity (MP1)[73] are associated with lower public employment.

b. Ex-ante autocracies

The results for ex-ante autocracies show an opposite story to that of ex-ante D&A, and some findings are consistent with my model predictions (Figure 2.15, panel B). Like for ex-ante D&A, natural resource rents distinctively "affect" public employment conditional on inequality. Resource rich autocracies (Q_A4 & Q_A5, with resource rents over 20% of GDP) that are also highly unequal, have on average 29 percent *less* public employment [74] than more equal autocracies in the bottom quintile of resource rents (Q_A1). This finding is consistent with MP5, but seems at odds with MP2.

The role of inequality in ex-ante autocracies requires further inquiry. As per figure 2.15 (panel B), highly unequal autocracies have 20 percent *higher* public employment, compared to more equal autocracies, conditional on being resource poor (Q_A1). Much like before, this positive coefficient is *not* the average effect of inequality, but rather simply a discrete change from the base level; consequently, it might not imply we can support MP2.

Once more, I clarify this point by observing the predicted public employment for ex-ante autocracies, by natural resource rents quintiles and inequality levels, at mean for other variables. Figure 2.16 (panel B) shows that, indeed, MP2 does *not* hold in autocracies. In resource poor and middle resource wealth countries (Q_A1 - Q_A3), public employment is statistically indistinguishable between highly unequal and more equal countries. However, after natural resource

[72] Being 1 percent more democratic brings about 0.13 less public employment.
[73] A 1 percent increase in output per worker correlates to 0.29 percent less public employment, on average.
[74] 35 and 22 percent for resource wealth Q_A5 and and Q_A4, respectively.

rents exceed 20 percent of the GDP (Q_A4-Q_A5), highly unequal autocracies have *lower* public employment than more equal ones.

Importantly, figure 2.16 (panel B) partially confirms MP5. It shows that *public employment decreases with resource rents when autocracies are relatively resource poor (Q_A1-Q_A2)*, with natural resource rents below 9 percent of GDP. This finding is consistent with MP5. When resource rents grow beyond it (Q_A3-Q_A5), however, public employment increases with resource riches, although only for more *equal* autocracies[75]. Hence, this U-shaped relationship between resource rents and public employment in ex-ante autocracies is only partially consistent with MP5: it holds for resource rents below, but not above 9 percent of GDP.

With regards to my other model predictions, in ex-ante autocracies the democracy score is not significant, unlike in ex-ante D&A. Public employment hence does not decrease as autocracies become less autocratic, so MP3 does not hold. On the opposite, consistent with MP1, public employment declines as autocracies become more productive[76].

The above mentioned U-shaped pattern clarifies a point for which I did not have enough empirical evidence so far: "the repression channel" dominates over the "stakes of staying in power channel" (see section 2.3.2.3)[77], *if autocracies are not very resource rich*. The relatively resource poorer autocracies redistribute their increasing natural resource rents between the small elites, and cut down public employment; they can afford doing this thanks to their resource riches funded repression apparatus ("the repression channel"). For the resource richer autocracies, on the contrary, the "stakes of staying in power" channel seems to dominate. When resource riches

[75] For very unequal autocracies, the trend is less clear. Public employment grows when resource rents are between 10 and 20 percent of GDP (Q_A3), but then drops once autocracies become very resource rich (Q_A4 -Q_A5).

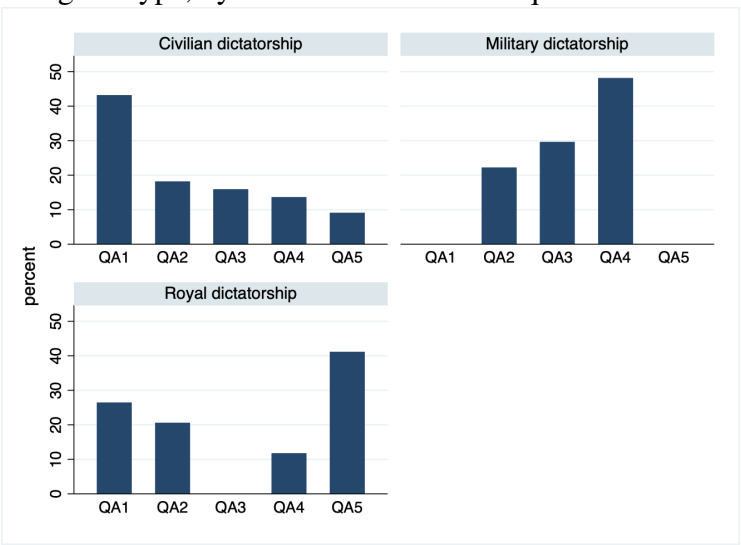
[76] A 1 percent increase in output per worker correlates to 0.44 percent less public employment, on average.

[77] In my predictions section 2.3.2.3, I *assumed* that in autocracies the "repression channel" dominates the "stakes of staying in power channel". This assumption determined the overall negative effect of natural resource rents on public employment. Nonetheless, as stated there, I did not have enough empirical evidence to substantiate the claim.

are high, the autocrat redistributes some of it to the population at large in the form of public employment, for the sake of political stability.

What explains the important differences between the relatively resource poor (Q_A1-Q_A2 and perhaps Q_A3) and the resource richer autocracies? The evidence I have provided so far only explicates the behavior of the relatively resource poor autocrats, where the "repression channel" prevails. For starters, we can observe that autocracies with *higher* resource rents are less likely to be *civilian* dictatorships, and more prone to be *military* dictatorships (figure 2.17).Furthermore, it is roughly consistent with "kleptocratic"[78] behavior to cut down redistributive public employment as resource rents grow, thanks to the possibility of repression. Indeed, as per Acemoglu et al. (2004) model, higher natural resource rents increase the likeliness of kleptocracy, as these additional resources can be used to buy the support of the "pivotal" groups.

Figure 2.17: Regime type, by natural resource rents quintile. 1995-2017 period.



Note: N=194. Graph excludes Indonesia, which later became a presidential democracy. The regime type variable comes from the Quality of Government Standard Dataset (2019).

[78] Kleptocracies are systems of government where small elites control the state for their own benefit, appropriating a large fraction of society's resources. This implies the institutionalization of corruption and theft, by and for the elites.

At the moment, I have no evidence that explains why very resource rich autocracies (Q_A4-Q_A5) act opposite to their resource poorer peers. Why does the "stakes of staying in power" channel prevails there, so that more resource rents increase redistributive public employment? To start with, resource rich autocracies are a very peculiar set of countries: Iran and the GCC. However, evidence suggests that Iran does not follow the same pattern as the GCC monarchies.

When observing the yearly data we can see that Iran's resource rents and public employment consistently *drop* during the period under study. Hence, while one can indeed observe a positive correlation between resource rents and public employment in Iran, we cannot claim that higher resource rents will *lead* to higher public employment there; this has not happened recently.

Consequently, the GCC countries are the only autocracies with simultaneous increases of resource rents and public employment. These countries are royal dictatorships, where the social contract historically has been that public jobs are an important rents redistribution mechanism. Furthermore, these monarchies have relatively small populations compared to their resource revenues size[79], so (unlike in a more populous country) it can be quite affordable to the employ the population for the sake of maintaining political stability.

While not consistent with my baseline predictions, this hypothesis is supported by Ali and Elbadawi (2016), who claim that GCC countries behavior can be explained by their incredibly high resource rents *per capita*, which make it politically unfeasible not to generously redistribute these riches with the population at large. I will further elaborate this point in section 2.6.3.1.

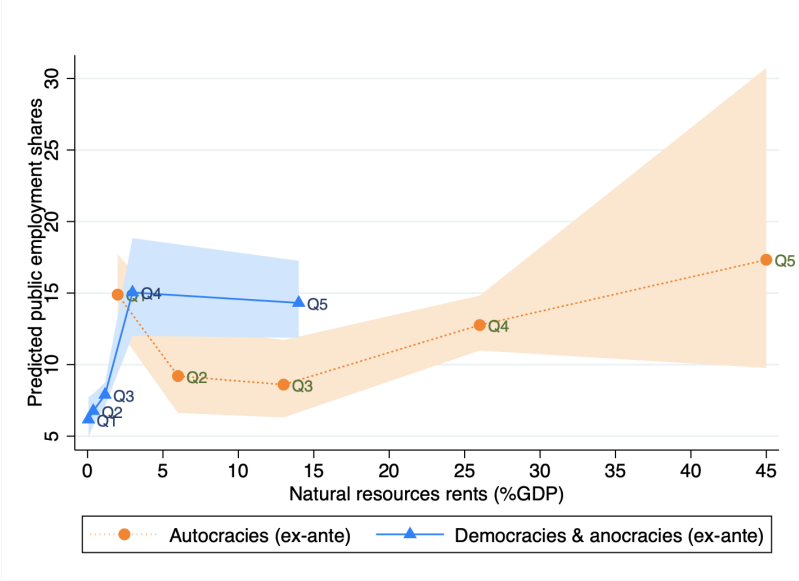
c. Summary: comparison between ex-ante democracies & anocracies vs. autocracies

In sum, how do the findings from the split-sample analysis for ex-ante D&A and autocracies compare? Figure 2.18 superimposes results for both samples, showing the average predicted

[79]Resource rents per capita are, on average, 3,800 and 46,000 USD per capita for Q_A4 & Q_A5, respectively.

public employment size by ex-ante regime type, over the respective resource-rent quintiles.

Figure 2.18: Predicted public employment as a share of WAP. Separate samples overlapped, by ex ante regime type. 1995-2017 period.



Note: This figure presents the marginal effects (at means) plus 95 % confidence intervals of table 2.24's regressions, columns (2) and (5), by natural resource rents quintiles and ex-ante regime type. The quintile labels (Q) correspond to the *average* natural resource rent for such quintile. Both regressions have urbanization, population, regime duration and dependency ratio controls, country and year fixed effects.

Figure 2.18 highlights that the split-sample analysis yields (strong) support for the central predictions under scrutiny in this paper: *In democracies, public employment increases with natural resource rents (MP4), whereas it decreases with natural resource rents in autocracies (MP5).* These predictions hold for all levels of natural resource rents, up to about 13 percent of GDP - but not beyond.

MP5 holds for ex-ante autocracies with low and medium resource wealth (Q_A1 & Q_A2)[80]. But MP5 does not hold for resource rich autocracies (Q_A3 - Q_A5)[81], when rents exceed 10 percent

[80] Bahrain, Belarus, China, Egypt, Indonesia, Laos, Morocco, Viet Nam and Zimbabwe.
[81] Azerbaijan, Bahrain, Egypt, Iran, Kuwait, Oman, Qatar, Saudi Arabia and Syria.

of GDP. For resource rich autocracies that are relatively equal[82], public employment increases with their natural resource rents, contrary to MP5. For autocracies, the overall relationship between resource rents and public employment is hence *U-shaped*. This is only partial inconsistency with predictions, but the positive trend in very resource rich autocracies be explained by the large size of rents relative to the population (to be discussed in section 2.6.3.1).

Figure 2.18 also underlines MP6: the prediction that public employment is *lower* in ex-ante D&A than in ex-ante autocracies holds particularly in resource-poor countries[83]. Figure 2.18 suggests that this prediction clearly holds only for resource wealth levels below 3 of GDP. At higher levels of resource wealth (between 4 and about 20 percent of GDP), figure 2.18 suggests the opposite happens: public employment is *higher* in ex-ante D&A than in ex-ante autocracies. No comparison is possible in very resource rich countries, with natural resource rents above 25 percent of GDP, since these are (nearly) exclusively autocracies.

The results from running the basic model[84] on two separate samples are robust to different definitions of public employment (see appendix figures 2.37 and 2.38), as well as to excluding regime "switchers"[85] from the sample (see appendix table 2.24, columns 3 and 6).

2.6.3 Robustness tests

Moving forward, I perform three sets of robustness tests. First, I will change the resource rents dependence metrics for ex-ante autocracies (section 2.6.3.1), following some literature that recommends measuring resource rents in per-capita terms (not over GDP). Second, I re-estimate

[82] Azerbaijan and Syria are excluded from this category, as access to education is unequal there.
[83] For empirical definitions of resource-poor, medium and rich, please refer to tables 2.7 and 2.15.
[84] Equation 2.10, with no interactions between natural resource rents and the democracy score.
[85] For ex-ante autocracies, "switchers" are those that became democracies at some point in the period under study, while for ex-ante democracies "switchers" are the ones who became autocracies.

the model under different samples (section 2.6.3.2), excluding countries with a small number of observations, or each geographical subregion. Finally, I further confirm the role of inequality in the model (section 2.6.3.3), by testing an alternative inequality measure and by substituting annual values per ex-ante ones.

2.6.3.1 Natural resource rents per capita in autocracies

In this section I study only ex-ante autocracies, and substitute the rents dependency variable (natural resource rents as a percentage of GDP) by rents per capita. The results presented below are consistent with those in my previous section, and further support my model's underlying logic and predictions.

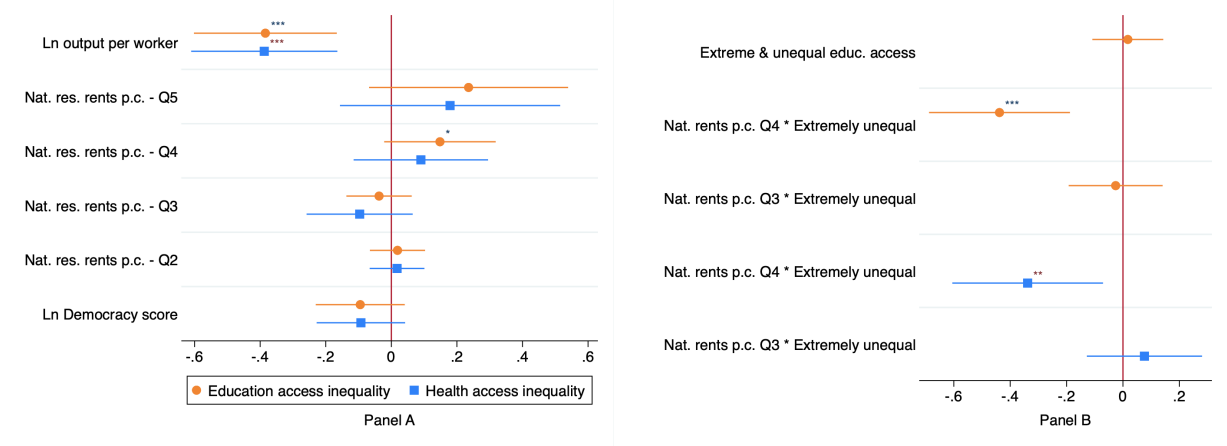
This robustness test comes about after my discussion on using probabilistic voting models in autocracies (sections 2.3.2.1 and 2.3.2.3). In general, I favor natural resource rents over GDP as a rents dependence measure, since it portrays their relative importance in the overall economy. Nonetheless, authors such as Anthonsen et al. (2012) indicate that rents per capita are a more appropriate metric if we are interested in the role of rents on regime stability, or in authoritarism. While my dependent variable is not regime stability, I find it useful to consider this innuendo, as redistributive public employment is one of the ways (according to my model) that authoritarian leaders use to ensure regime continuation.

Figure 2.19 shows the regression coefficients. Ex-ante autocracies in the 4th quintile of resource riches per capita ($Q_{pc}4$) have 13 percent *higher* public employment than otherwise similar autocracies in the bottom of per capita resource rents ($Q_{pc}1$)[86], conditional on access to educa-

[86]The natural resource rents per person are on average 24 and 3,792 USD (2010, constant), for $Q_{pc}1$ and $Q_{pc}4$, respectively.

tion being relatively equal. Nonetheless, when access to education becomes unequal, those same resource rich autocracies ($Q_{pc}4$)[87] will have on average 27 percent [88] *less* public employment than otherwise similar resource poor autocracies. These findings contradict my hypothesis on inequality (MP2), but are somewhat inconclusive in terms of the roll of rents.

Figure 2.19: Ln public employment determinants of ex-ante autocracies, regression coefficients. Natural resource rents per capita. 1995-2017 period.



Note: N=199. All regressions control for urbanization, population, dependency ratio, regime duration, country and year fixed effects and robust standard errors. Full regression results available in appendix table 2.26, columns (2) and (4). $* p < 0.10$, $** p < 0.05$, and $*** p < 0.01$

Figure 2.20 presents the predicted public employment for ex-ante autocracies, by natural resource rents quintiles and inequality levels, at mean for other variables. Once again, we find a U-shaped relationship between public employment and natural resource rents. In resource poor and middle resource wealth countries ($Q_{pc}1$-$Q_{pc}2$ [89]), public employment is comparatively small and decreases with natural resource rents per capita, in accordance with MP5. Nonetheless, when natural resource rents per capita exceed USD 140 ($Q_{pc}3$), public employment sharply increases

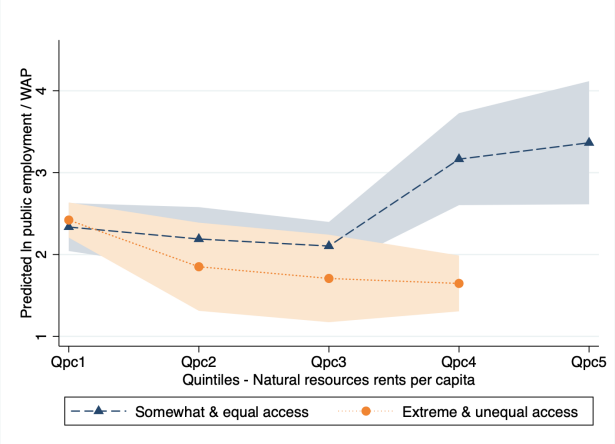
[87] The interaction between the 5th quintile of resource rents per capita and inequality cannot be estimated; there are no autocracies in that range with unequal education access.

[88] 0.13*$Q_{pc}4$ - 0.41*$Q_{pc}4$*Inequality=-0.27, conditional on $Q_{pc}4 = 1$.

[89] The natural resource rents per person are on average 24 and 99 (2010 constant), for $Q_{pc}1$ and $Q_{pc}2$, respectively. The countries included in this group are Belarus, China, Egypt, Indonesia, Laos, Morocco, Syria, Viet Nam and Zimbabwe.

with resource rents in autocracies with equal and somewhat equal access to education, contrary to both MP2 and MP5.

Figure 2.20: Predicted Ln public employment as a share of WAP for ex-ante autocracies. Natural resource rents per capita. 1995-2017 period.



Note: This figure presents the marginal effects (at means) plus 95 % confidence intervals of table 2.26's column (2), by natural resource rents per capita quintiles.

As in the previous section, I find evidence that "the repression channel" dominates when the autocracies rents per capita are relatively low. In these cases, larger natural resource rents (relative to the population size) allow leaders to be less responsive to political demands and thus save themselves some public employee wages. These autocrats can do better by simply increasing repression, or threatening to do so, and reward only their small elites.

However, the story switches when countries' natural resources wealth per capita is at the middle ($Q_{pc}3$) and top of the distribution ($Q_{pc}4$ & $Q_{pc}5$)[90], conditional on relatively equal access to education [91]. In these countries, where 5 out of 6 are GCC monarchies, the rulers increase public employment with their resource riches per capita (Figure 2.20). The sharp increase is lead by countries with more equal access to education; medium and resource rich countries with

[90] Resource rents per capita start at 3,792 to 46,463 USD (2010 constant), for $Q_{pc}4$ and $Q_{pc}5$, respectively.
[91] Bahrain, Iran, Kuwait, Oman, Qatar and Saudi Arabia.

unequal education access [92] will continue decreasing public employment as their resource riches grow. Hence, neither MP2 nor MP5 hold when autocracies are very resource rich. [93]

Is the evidently oversized public employment in the top per capita resource wealthy nations a GCC effect? It surely is, as they predominantly compose this group. Furthermore, if we were to observe in detail Iran's behavior in the period under studied, we will see that both public employment and natural resource rents (both as the share of GDP and in per capita terms) consistently *decrease*. Hence, one cannot claim that, in Iran, higher resource rents led to higher (or lower) public employment, as resource rents did not rise at any point during the period under study.

The findings on the behavior of resource rich autocracies are intuitively reasonable. Is it unsurprising that people expect a cut of the *at least* 45,000 USD per capita mana from heaven. Ruling elites who wish to maintain some stability find it rational too, and grant comfortable public sector jobs to anywhere between 16 and 49 percent of their native WAP. As previously discussed, it is against my prediction MP5 that autocracies might increase public employment as their resource rents grow. However, the fact that these extremely resource rich autocracies do, follows the model's most basic intuition: leaders prefer redistributing via public employment because, unlike public goods or other universal transfer schemes, it facilitates control over those they employ.

The above mentioned trends mirror those of Ali and Elbadawi (2016). They predict and empirically confirm that i) high resource rents *per capita* countries are likely to expand public employment as a mean to maintain stability. On the opposite, they find, ii) low resource rents *per capita* countries have smaller public employment and openly rely on repression to suppress any

[92] Azerbaijan is the only country in this category.
[93] Results with health inequality are virtually identical, and can be found in appendix figure 2.39.

aims for change.

With regards to my other model predictions, the results remain the same as in the previous section. Public employment hence does not decrease as autocracies become less autocratic (MP3 does not hold). On the opposite, consistent with prediction MP1, public employment declines as autocracies become more productive.

2.6.3.2 Sample exclusions

In this section, I perform two robustness tests. I will estimate results again under different samples, excluding either i) small N countries, or ii) each geographic subregion, from the ex-ante anocracies and democracies sample only.

a. Small N countries exclusions.

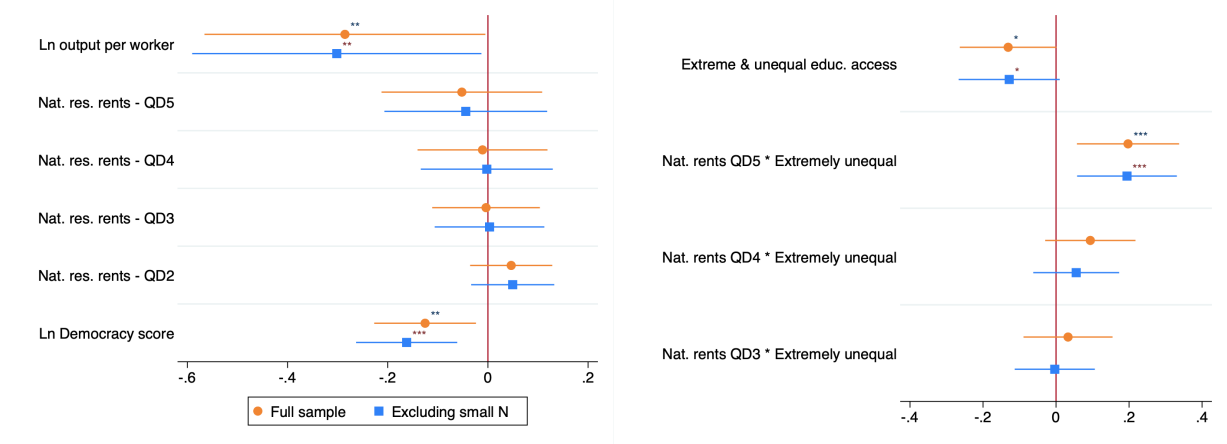
These are 35 countries (out of 137) that have $N \leq 7$, which is less than 1/3 of the total number of possible observations. As discussed in section 2.5.1, this small sample definition sets a somewhat arbitrary inclusion threshold. Furthermore, there is the question of whether dropping countries with few observations could induce bias, as these are on average resource richer, more unequal, considerably less productive and democratic and also have smaller bureaucracies. However, these "fears" are unfounded.

All coefficients retain their sign and significance after dropping small N countries. For *ex-ante democracies and anocracies* (figure 2.21), this means leaving 31 countries out, of which 18 are anocracies[94]. Furthermore, these are predominantly resource rich: 80 percent of them are in natural resource rents quintiles 4 and 5. For *ex-ante autocracies* (figure 2.22), dropping small

[94]Excluded ex-ante anocracies are Angola, Bhutan, Cameroon, Congo, Cote d'Ivoire, Ethiopia, Gabon, Haiti, Iraq, Myanmar, Nigeria, Rwanda, Suriname, Tajikistan, Togo and Yemen. Excluded democracies are Bangladesh, Benin, Cape Verde, Ghana, Liberia, Madagascar, Malawi, Mali, Namibia, Nicaragua, Niger, South Korea, Timor-Leste and Zambia.

N countries means excluding 4 countries out 17 [95], which on the opposite are predominantly resource poor: 86 percent of them are in natural resource rents quintiles 1 and 2. Nonetheless, the results for both groups of countries are essentially unchanged after the exclusions.

Figure 2.21: Ln public employment as a share of WAP determinants in ex-ante democracies and anocracies, regressions coefficients. Excluding small sample size countries. 1995-2017 period



Note: This figure represents 2 different regressions: one with the entire ex-ante democracies and anocracies sample (N=1788, for 122 countries), and another one excluding those countries with less than 8 observations (N=1676, for 98 countries). All regressions include urbanization, population, regime duration and dependency ratio controls, country and year fixed effects and country clustered standard errors. The resource wealth quintiles 3 and 2 dummies, as well as its interactions, are not significant, so they are omitted here (although not in the regression). Full results available on table 2.27. $*\ p < 0.10$, $**\ p < 0.05$, and $***\ p < 0.01$

b. Geographic subregions exclussions

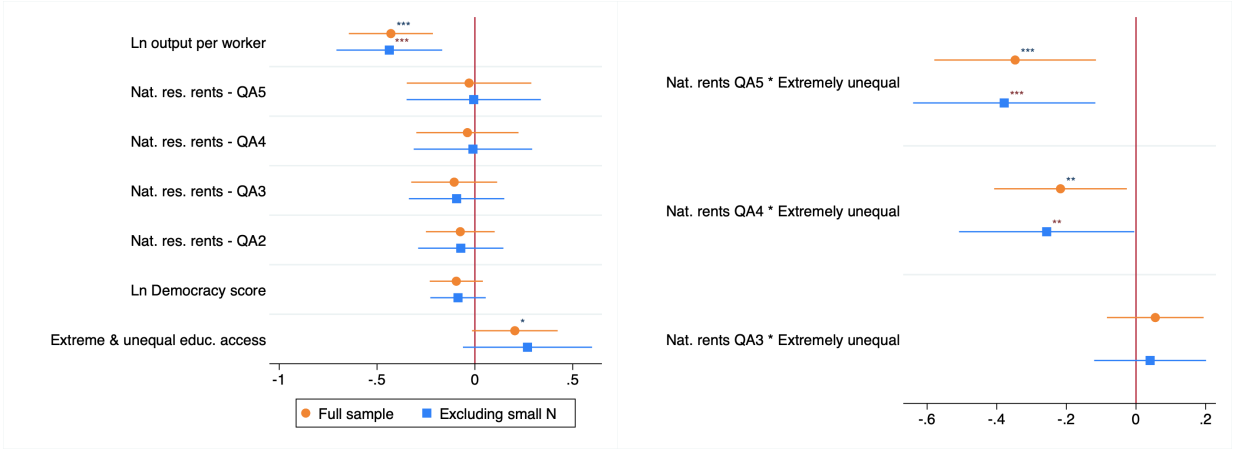
I perform this second robustness test only for the ex-ante D&A sample; it is not possible for ex ante autocracies, because of its small sample size. The results are generally robust, in magnitude and significance, to the exclusion of all geographical subregions but for Africa[96] and ECA[97]. Moving forward, I provide possible explanations for it and indicate why it likely does not

[95] China, Cuba, Laos and Zimbabwe.

[96] Angola, Botswana, Burkina Faso, Cape Verde, Congo, Côte d'Ivoire, Ethiopia, Gabon, Ghana, Guinea, Kenya, Liberia, Madagascar, Malawi, Mali, Mauritius, Namibia, Niger, Nigeria, Rwanda, Senegal, South Africa, Tanzania, Uganda and Zambia.

[97] Albania, Armenia, Bosnia and Herzegovina, Bulgaria, Croatia, Czech Republic, Georgia, Kazakhstan, Kyrgyzs-

Figure 2.22: Ln public employment as a share of WAP determinants in ex-ante autocracies, regressions coefficients. Excluding small sample size countries. 1995-2017 period



Note: This figure represents 2 different regressions: one with the entire ex-ante autocracies sample (N=199 observations, for 16 countries), and another one excluding those countries with less than 8 observations (N=165, for 12 countries). All regressions include urbanization, population, regime duration and dependency ratio controls, country and year fixed effects and robust standard errors. Full results available on table 2.27. $* p < 0.10$, $** p < 0.05$, and $*** p < 0.01$

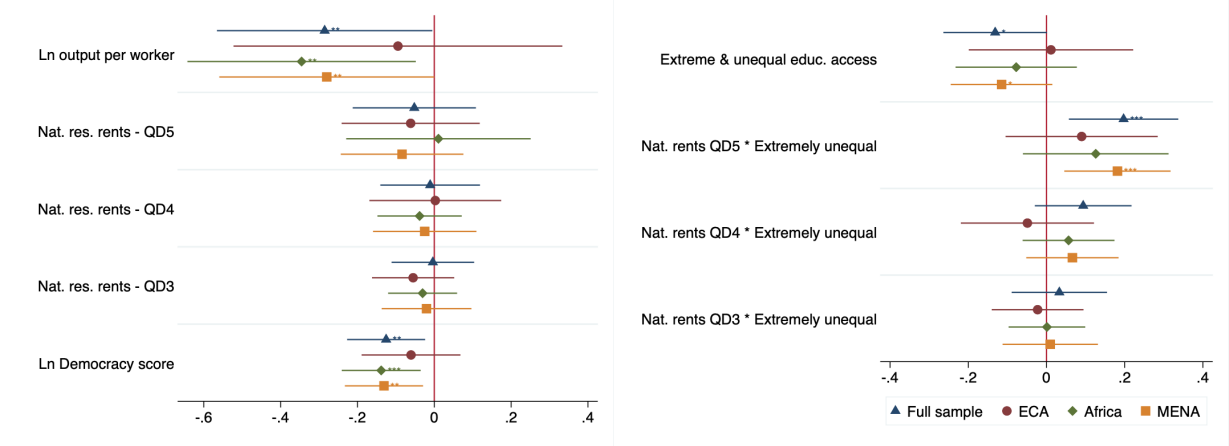
challenge my results.

If we exclude Africa from the sample (figure 2.23), neither the inequality dummy nor its interactions with resource rents are significant. In this world, natural resources seem to have no effect whatsoever on public employment. More dramatically, when we drop the ECA region (also figure 2.23), none of my main variables seems statistically significant anymore. Output per worker, the democracy score and the inequality dummy are less negative than for the full sample, and the interactions with resource rents are less positive. Furthermore, all variables have larger standard errors than before.

In under to understand these changes, we first ought to observe if there are any important differences in the distribution of the key variables (See table 2.10, and appendix figure 2.40 for

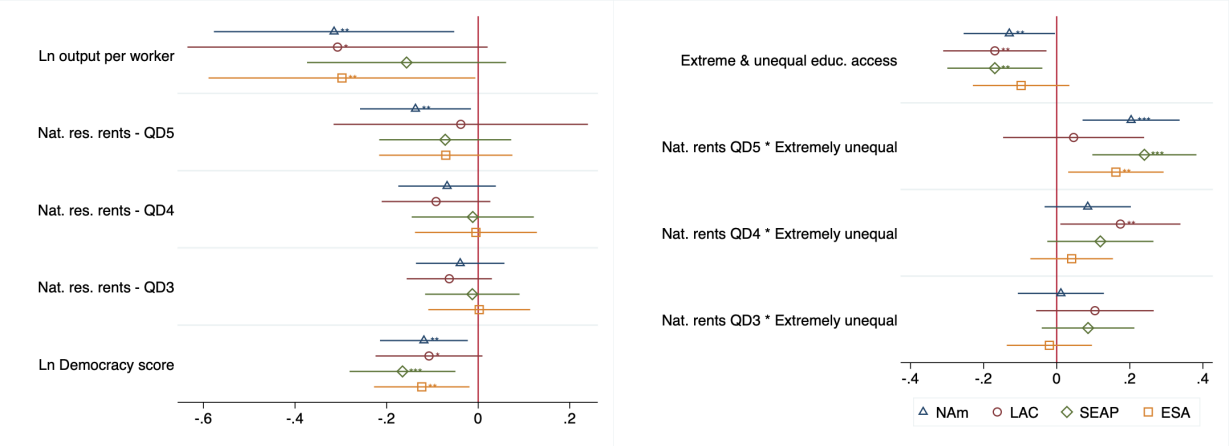
tan, Moldova, Poland, Russia, Macedonia, Turkey and Ukraine.

Figure 2.23: Ln public employment as a share of WAP determinants, excluding Africa, ECA and MENA subregions. 1995-2017 period



Note: All regressions control for urbanization, population, regime duration and dependency ratio, country and year fixed effects and country clustered standard errors. Full results available on appendix table 2.28. $* p < 0.10$, $** p < 0.05$, and $* * * p < 0.01$

Figure 2.24: Ln public employment as a share of WAP determinants, excluding LAC, NAm, SEAP and ESA subregions. 1995-2017 period



Note: All regressions control for urbanization, population, regime duration and dependency ratio, country and year fixed effects and country clustered standard errors. Full results available on appendix table 2.28. $* p < 0.10$, $** p < 0.05$, and $* * * p < 0.01$

kernel densities). Importantly, excluding Africa from the sample makes the world seem considerably resource poorer: 37 percent of African countries have medium resource wealth ($Q_W 4$) and

43 percent are resource rich (Q_W5). In consequence, excluding Africa means we lose 20 and 32 percent of the observations in medium (Q_W4) and high (Q_W5) natural resource rents countries, respectively.

Another fundamental difference is that both ECA and Africa are considerably less democratic than other regions in the sample. African countries represent 40 percent of all countries that became autocracies, and 27 percent of all anocracies in the sample. These are large percentages, considering Africa constitutes only 10 percent of the full ex-ante democracies and anocracies. The story for ECA is rather similar: while it constitutes only 16 percent of the observations in the ex-ante democracies and anocracies sample, 60 percent of all (turned) autocracies and 28 of all anocracies are in ECA. This could evidently affect estimation results.

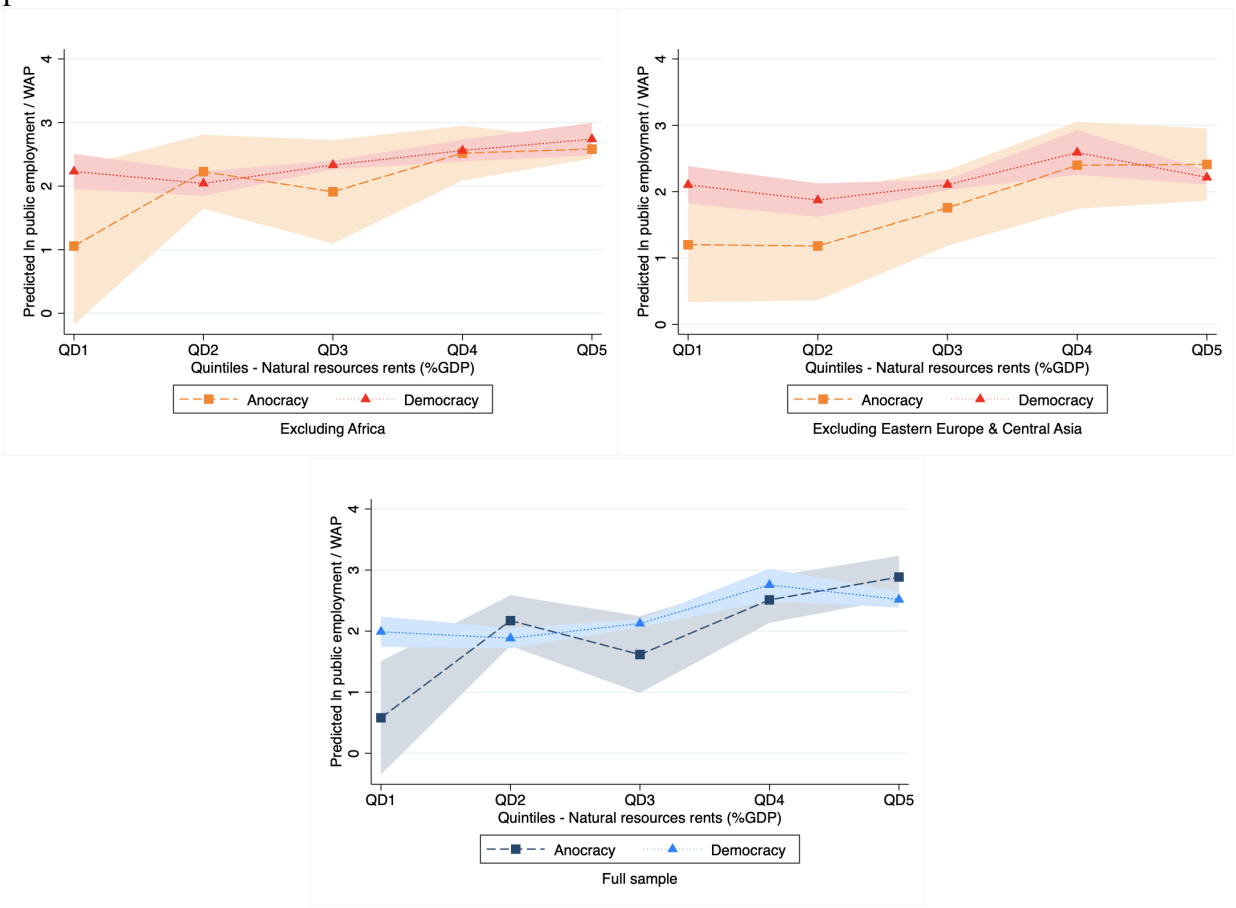
Table 2.10: Summary statistics, excluding Africa and ECA subregions. 1995-2017.

Exclusions	Variable	Mean	Std. Dev.	Min.	Max.
Africa	Natural resource rents (% GDP)	2.92	5.43	0.00	53.05
	Public sector employment (% WAP)	11.79	5.93	1.62	36.76
	Education inequality	0.49	0.80	0	2
	Output per worker	49,588	33,501	4,285	229,260
	Democracy score (0-10)	8,88	1,65	2	10
ECA	Natural resource rents (% GDP)	3.88	7.01	0.00	56.94
	Public sector employment (% WAP)	10.37	5.96	0.82	29.90
	Education inequality	0.68	0.89	0	2
	Output per worker	49,775	35,231	1,419	229,260
	Democracy score (0-10)	8,82	1,79	2	10
None	Natural resource rents (% GDP)	3.71	6.67	0.00	56.94
	Public sector employment (% WAP)	11.11	6.06	0.82	36.76
	Education inequality	0.59	0.85	0	2
	Output per worker	46,295	33,604	1,419	229.261
	Democracy score (0-10)	8,68	1,86	2	10

Figure 2.25 shows the model's predicted public employment by natural resource rents quintiles ex-ante regime type, after evaluating all other variables at mean levels. Importantly, *the average rents effect for democracies is roughly the same (ceteris paribus) in the full country sample and the ones excluding Africa and ECA*. On the other hand, estimates for anocracies become even

less precise than before when we drop the Africa and ECA subregions (that is, when we lose 27 and 28 percent of all observations for anocracies, in each case). Thus, this could explain the mayor changes observed in figure 2.23. With this in mind, I believe my main results are robust to dropping both Africa and ECA from the sample, as doing so does not substantially alter them and how they support my model's underlying logic.

Figure 2.25: Predicted Ln public employment as a share of WAP by natural resource rents quintiles ex-ante regime type. Full sample vs. excluding ECA and Africa subregions. 1995-2017 period.



Note: This figure presents the marginal effects (at means) plus 95 % confidence intervals of table 2.28's columns (1) and (5), by natural resource rents quintiles and yearly regime type. Full sample marginal effects are included for reference purposes. Because of lack of statistical significance, this figure excludes countries that became autocracies.

2.6.3.3 The role of inequality

In order to confirm the relevance of inequality in this model, I take two extra steps. First, I substitute educational access inequality by health access inequality, simply to test a different inequality measure[98]. Second, I change the annual inequality values by those in 1995 (ex-ante), to address the previously raised issue of reverse causality, where public employment size could potentially be affecting inequality. Furthermore, there is also the question of whether natural resource rents themselves affect inequality, so substituting these variables by their initial values may yield cleaner estimates.

a. Ex-ante democracies and anocracies

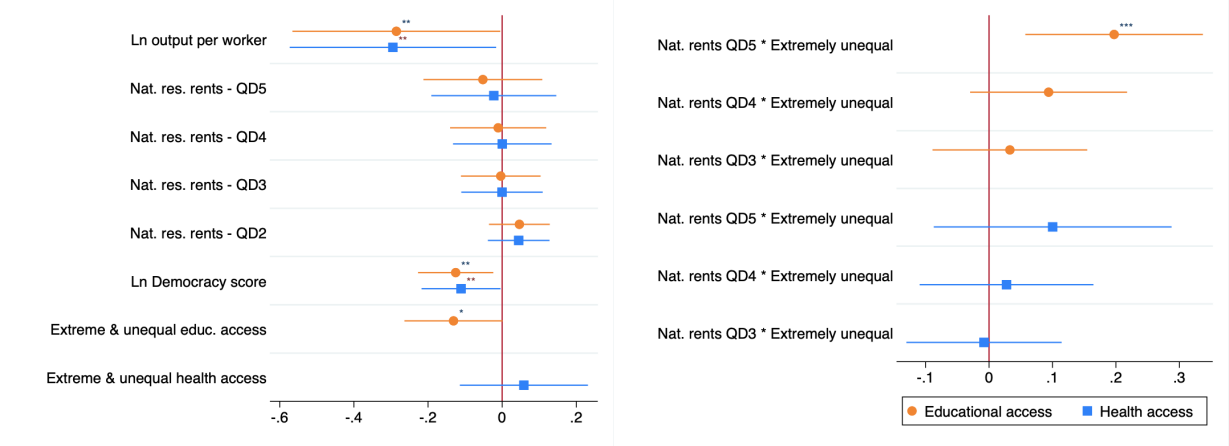
For ex-ante D&A's, most coefficients are similar in magnitude and significance when switching education by health access inequality (figure 2.26), but precisely for health inequality an its interactions. Health inequality's main coefficient is now positive (although non-significant), but its interactions keep a similar magnitude to those under the education inequality regression (they are non-significant, nonetheless, due to much larger standard errors). Interestingly, the predicted public employment, at means for all other variables, is similar regardless of whether we control for health or education access inequality (figure 2.27, panels A and B, respectively).

In the second robustness test, figure 2.28 compares yearly and ex-ante educational access inequality for ex-ante D&A's. The scenario is the same as before: coefficients magnitudes and significance remain similar under both specifications, despite the larger standard errors for the inequality interactions. Importantly, *Nat. rents Q5 * Extremely unequal* remains significant, and barely changed its magnitude. [99]

[98] Also because, as previously mentioned, I have no a priori reason to prefer one variable over the other.

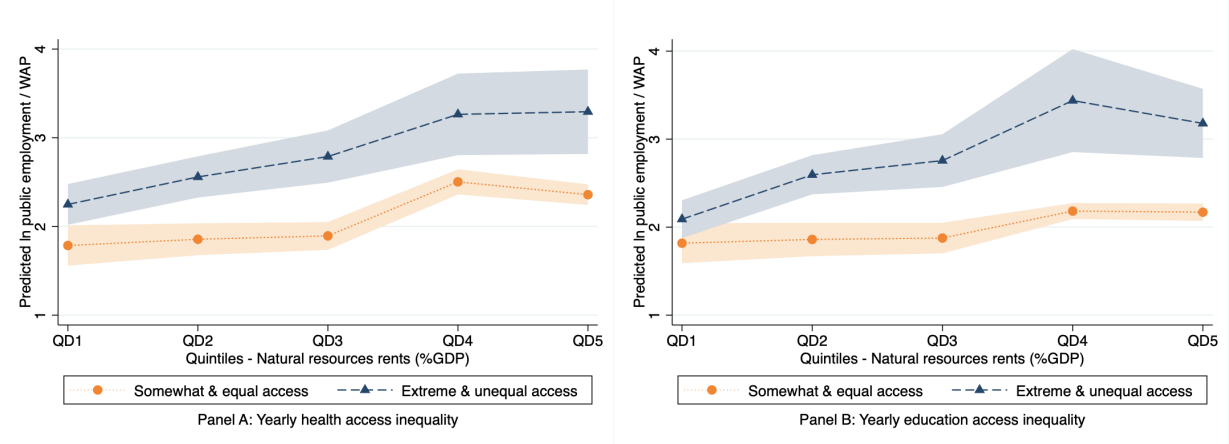
[99] Results with ex-ante health access inequality controls can be found in appendix table 2.29. The ex-ante inequal-

Figure 2.26: Ln public employment determinants of ex-ante democracies and anocracies, regression coefficients. Different inequality definitions. 1995-2017 period.



Note: N=1,768. All regressions control for urbanization and population, country and year fixed effects and country clustered standard errors. The "Somewhat unequal" access interactions with natural resource rents, are not significant, so they are excluded from this figure (but not from the regression). Full regression results available in appendix table 2.29, columns (1) and (3). $* p < 0.10$, $** p < 0.05$, and $*** p < 0.01$

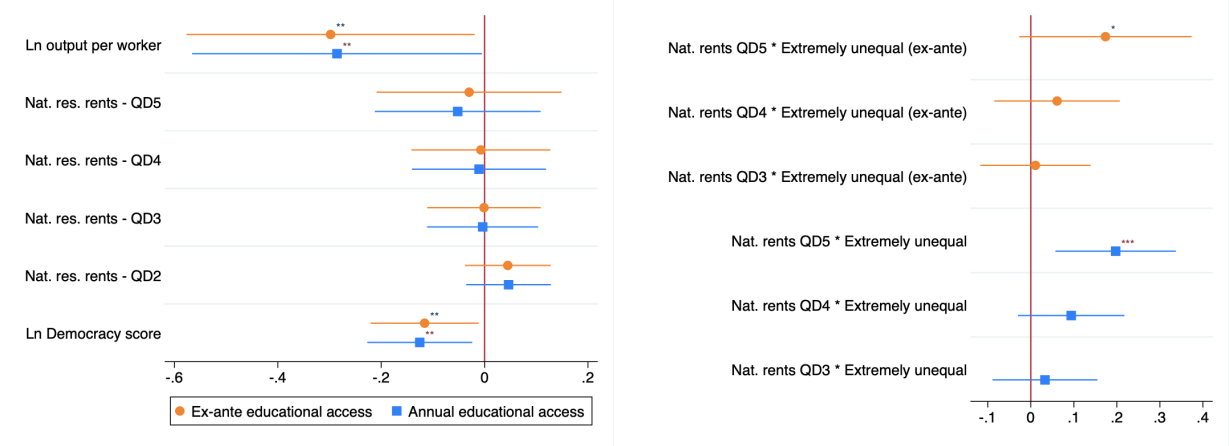
Figure 2.27: Predicted Ln public employment as a share of WAP for ex-ante anocracies and democracies. For health and education access inequality. 1995-2017 period.



Note: This figure presents the marginal effects (at means) plus 95 % confidence intervals of table 2.29's regressions, columns (1) and (3), by natural resource rents quintiles and inequality. Both regressions have urbanization, population, regime duration and dependency ratio controls, country and year fixed effects.

ity interaction remains non significant.

Figure 2.28: Ln public employment determinants of ex-ante democracies and anocracies, regression coefficients. Ex-ante education inequality controls. 1995-2017 period



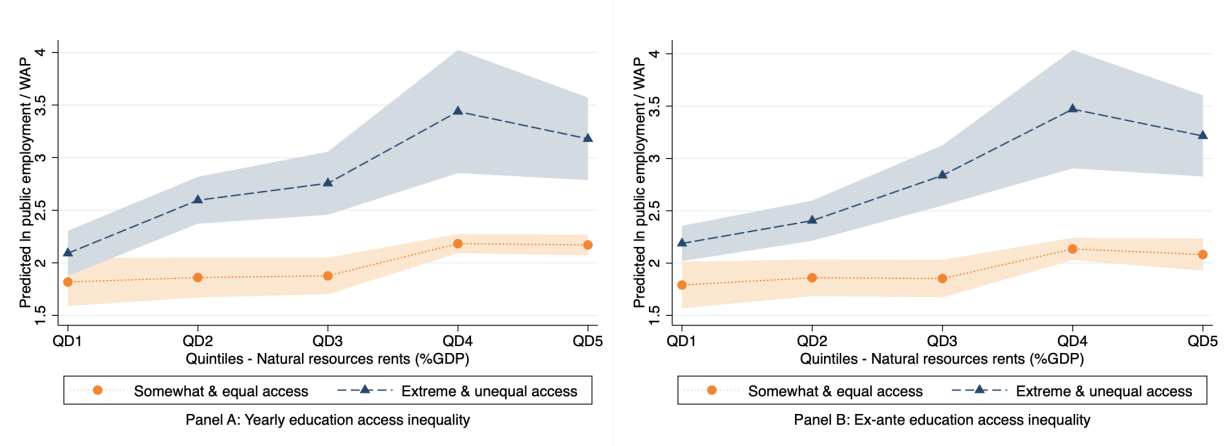
Note: N=1,768. All regressions control for urbanization and population, country and year fixed effects and country clustered standard errors. The "Somewhat unequal" access interactions with natural resource rents, are not significant, so they are excluded from this figure (but not from the regression). Full regression results available in appendix table 2.29, columns (1) and (2). $* p < 0.10$, $** p < 0.05$, and $*** p < 0.01$

If we observe the marginal effects of educational access inequality (figure 2.29), we still find that public employment increases with natural resource rents when in presence of high inequality (extreme and unequal access), once more confirming MP2 and MP4. Furthermore, just as with annual inequality controls, ex-ante highly unequal countries have consistently higher public employment than more equal ones (except when very resource poor). The case of health access inequality is of similar nature (appendix figure 2.41): countries with highly unequal health access (both ex-ante and yearly) have consistently higher public employment than more equal ones (except when very resource poor).

b. Ex-ante autocracies

My only robustness test for ex-ante autocracies is to exchange education by health inequality. Regressions with ex-ante inequality make no sense for these countries, as both health and

Figure 2.29: Predicted Ln public employment as a share of WAP determinants for ex-ante anocracies and democracies. By annual and ex-ante educational access inequality. 1995-2017 period.



Note: This figure presents the marginal effects (at means) plus 95 % confidence intervals of table 2.29's regressions, columns (1) and (2), by natural resource rents quintiles and educational access inequality. Both regressions have urbanization, population, regime duration and dependency ratio controls, country and year fixed effects.

education inequality remain virtually identical in the period under study[100].

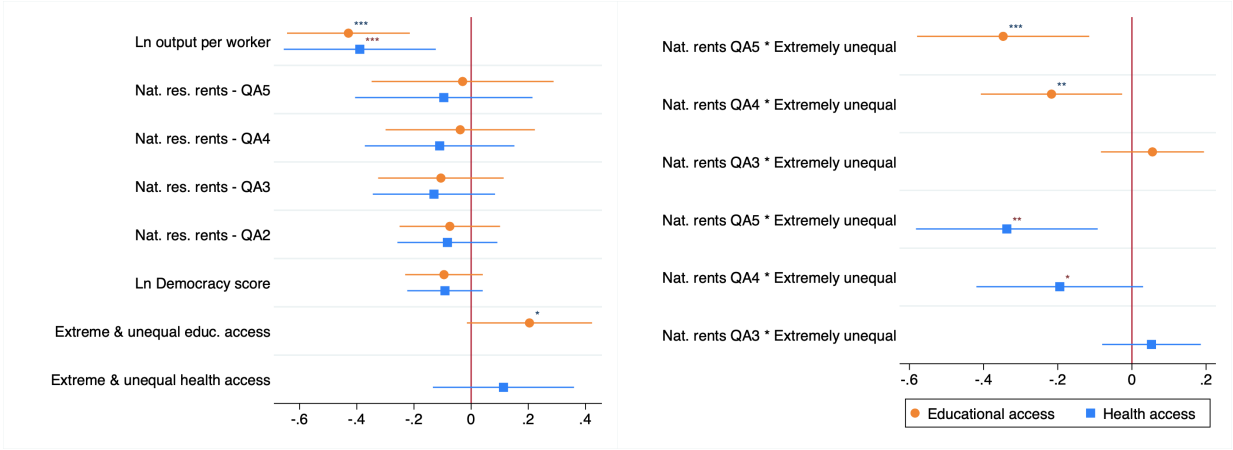
For ex-ante autocracies, regression coefficients are generally robust to changing inequality variables (figure 2.30). Furthermore, this change translates into slightly more precise inequality and natural resources joint trends (see figure 2.31), as the inequality dummy in itself (main effect) was not significant. In short, MP2 still does not hold for ex-ante autocracies, and MP5 still only does for those that are not resource rich (below $Q_A 2$).

c. In conclusion

To conclude, in ex-ante D&A, the role of inequality is not fully robust to switching educational by health access inequality (MP2 does not hold), but it is to considering ex-ante inequality (MP2 does hold). In ex-ante autocracies, results are robust to switching educational by health ac-

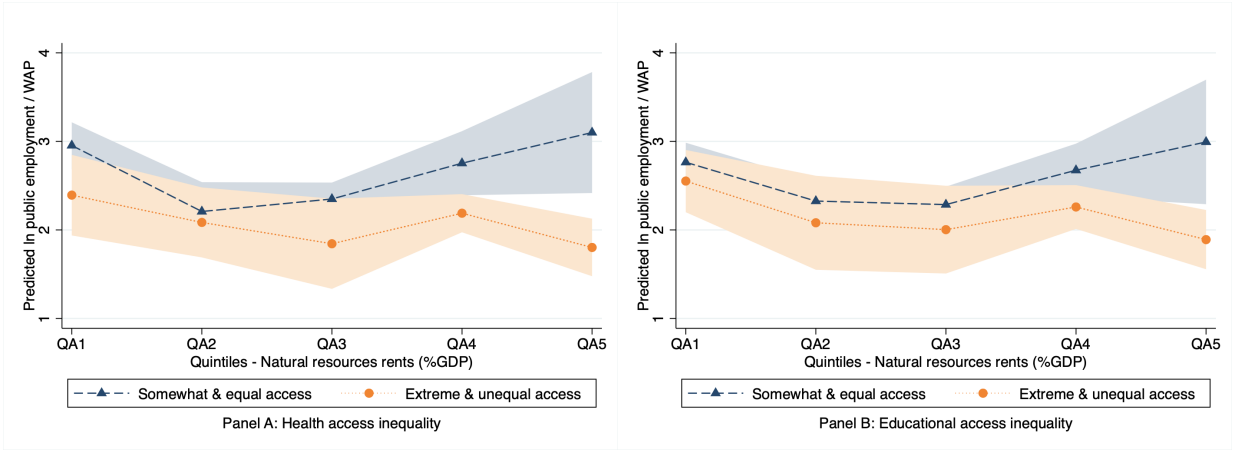
[100]Health inequality patterns do not change at all, and only Saudi Arabia went from initially having somewhat-equal access to education to very equal access to education (between 2012-2017).

Figure 2.30: Ln public employment determinants of ex-ante autocracies, regression coefficients. Different inequality definitions. 1995-2017 period.



Note: N=199. All regressions control for urbanization, population, dependency ratio, regime duration, country and year fixed effects and robust standard errors. Full regression results available in appendix table 2.30, columns (1) and (3). $* p < 0.10$, $** p < 0.05$, and $* * * p < 0.01$

Figure 2.31: Predicted Ln public employment as a share of WAP determinants for ex-ante autocracies, for health and education access inequality. 1995-2017 period.



Note: This figure presents the marginal effects (at means) plus 95 % confidence intervals of table 2.30's regressions, columns (1) and (3), by natural resource rents quintiles and health access inequality. Educational access inequality results are presented for comparison purposes. Both regressions have urbanization, population, regime duration and dependency ratio controls, country and year fixed effects.

cess inequality, and presumably would be too to considering ex-ante inequality, since inequality does not change for these countries.

There are 3 main takeaways from this section. First, the relationship between inequality and public employment remains in opposite directions for autocracies vis-a-vis democracies and anocracies. Second, in democracies and anocracies, yearly inequality changes affect public employment levels similarly to initial inequality levels.[101] Finally, in the period under study neither larger public employment nor changing natural resource rents seem to fundamentally affect the educational and health access inequality coefficients, as evidenced by the fact that the values of the $rents*inequality$ interactions are very similar when using either yearly or ex-ante inequality coefficients. Consequently, the observed effect of inequality over public employment does not seem to be due to reverse causality.

2.7 Conclusion

Are natural resource rich countries prone to redistributing their mana from heaven via overmanned public sectors? This paper is, to my knowledge, the first large N cross country study tackling this question. With 138 countries over 23 years, I include countries across the political institutions spectrum, thus considering the often overlooked and at times very resource rich autocracies. My findings are generally in line with my baseline theory and proposed extensions.

Overall, in the average country, resource riches are not associated with higher public employment. In the average country, productivity (MP1) and the degree of democracy (MP3) are the most consistent correlates of public sector employment size. In democracies, however, resource riches consistently predict higher public employment, possibly due to higher redistribution pres-

[101] As with the democracy score, were we part from is more relevant a determinant than were we currently are.

sures (MP4). This is particularly the case in democracies that are highly unequal (MP2), such as Venezuela. These results are generally robust to alternative definitions of public employment and to using different samples, but they are only partially robust to changes in the inequality definition.

In autocracies, by contrast, the association between resource riches and public employment appears U-shaped. As autocracies move from no resource riches to natural resource rents levels around 9 percent of the GDP, the number of public employees declines (MP5). When resource riches increase beyond it, however, this association turns positive and public employment grows with increasing resource wealth (MP5 does not hold). Unlike democracies, inequality plays no role here: public employment remains roughly the same regardless of how unequal the country is, with the exception of extremely rich autocracies (such as Saudi Arabia, and other GCC countries), which have relatively equal access to education throughout the period I studied. These results are generally robust to alternative definitions of natural resource rents dependency, public employment, inequality and to using different samples.

My model predicts that in situations of low productivity and high inequality, democracies with large natural resource rents are more likely to use public employment as a preferred redistribution mechanism, compared to countries without these riches. This behavior will be ever more likely as the rents from being in power increase, and such jobs will mainly be offered to the patron's poorer clients. Furthermore, there will be an expectation of political support in exchange for the jobs, a phenomenon known in the literature as *clientelism*. As a mean to account for other possible explanations, I include country and year fixed effects, and controlled for external risk measures and demographic information, which could capture whether government employment is used for social insurance purposes. Even after these adjustments, my empirical findings remain

consistent with my theory.

What is so special about natural resource rents that could be inducing clientelistic public employment? As Anthonsen et al. (2012) puts it, natural resource rents go straight to the state coffers, without taxing the citizens. Therefore, unless institutions are very strong to start with, the use of these funds is inherently less subject to accountability. Politicians, in turn, have higher leeway to spend this money in ways that will increase their probabilities of reelection. And, as previously discussed, overmanning bureaucracies, potentially with people in the patron's social network, is often a largely convenient mean to guarantee voters loyalty and thus stay in office.

Naturally, I cannot claim that all public sector increases that follow after higher natural resource rents are clientelistic, rather than perhaps simply redistributive and in general leading to overmanned bureaucracies. My model suggests this is the likely case, but my empirical strategy cannot fully support it. To further substantiate my claims, I need a cleaner identification strategy. Consequently, in the next essay I try to strengthen my case by focusing on a single country, and study whether natural resource rents not only increase public employment, but also affect its composition (in terms of skills), contractual arrangements, and public services provision.

2.A Model variables definitions and sources

2.A.1 General methodological considerations

Quality is a key concern when compiling different sources to create a data series, as potentially different definitions or methods of calculation may produce irregularities in the data. In order to minimize measurement error, I created the data according to the following principles: [102]

1. Primary sources are always preferred over secondary ones, as the former is often more transparent than the latter in terms of measurement methods;

2. When employing secondary sources, I chose those that provide detailed discussions of the methods and sources employed;

3. Minimize the number of sources used to construct any variable, ideally employing only one source per variable, under the assumption that the methods used for one same source tend to be constant in time; and

4. Employ the most updated data available from each source, *not preliminary estimates*.

In terms of the missing values, I used a linear interpolation procedure. When a country-year value is missing for the *rents* (flow) variables, I replaced the missing value with the average of the values that immediately precede it and follow it, only when the missing value occurs within a series of positive values. Haber and Menaldo (2011) recommend this procedure under the principle that not doing so will result in implicitly treating the missing values as zeroes, therefore undercounting rents in countries where otherwise I would have information.

[102] Haber and Menaldo (2011) thoroughly develop these points in their methodological appendix.

2.A.2 Variables

Dependency ratio: ratio of persons below 14 and above 64 years over the working age population (persons aged between 15 and 64 years). *Source*: Own calculations, World Bank World Development Indicators (WDI) (2020) data.

Education inequality: measures to what extent high quality basic education is guaranteed to all children, to enable them to exercise their basic rights as adult citizens. Basic education is typically provided between 6 and 16 years of age. The variable is ordinal, such that:

- Equal (0): basic education is equal in quality and less than five percent of children receive low-quality education;
- Relatively equal (1): basic education provision is overall equal in quality, but five to ten percent of children receive low-quality education;
- Somewhat equal (2): basic education provision is relatively equal in quality, but ten to 25 percent of children receive low-quality education;
- Unequal (3): high quality basic education provision is extremely unequal and least 25 percent of children receive low-quality education;
- Extreme inequality (4): high quality basic education provision is extremely unequal and at least 75 percent of children receive low-quality education.

In the original variable the order of the values is inverted; that is, 0 corresponds to extreme inequality and 4 to equality. I inverted this order to facilitate the easy of interpretation of my model.

Source: Pemstein et al. (2021)

Healthcare inequality: measures to what extent high quality basic healthcare is guaranteed to all citizens, enabling them to exercise their basic rights as adult citizens. Poor-quality healthcare fails to adequately treat preventable and treatable illnesses that hinder people from working, participating in social or political organizations, or voting (when allowed). The variable is ordinal, such that:

- Equal (0): basic healthcare is equal in quality and less than five percent of citizens receive low-quality education;
- Relatively equal (1): basic healthcare is overall equal in quality, but five to ten percent of citizens receive low-quality healthcare;
- Somewhat equal (2): poor quality healthcare prevents between 10 and 25 percent of citizens from fully exercising their political rights as adult citizens;
- Unequal (3): poor quality healthcare prevents at least 25 percent of citizens from fully exercising their political rights as adult citizens;
- Extreme inequality (4): poor quality healthcare prevents at least 75 percent of citizens from fully exercising their political rights as adult citizens.

In the original variable the order of the values is inverted; that is, 0 corresponds to extreme inequality and 4 to equality. I inverted this order to facilitate the easy of interpretation of my model.

Source: Pemstein et al. (2021)

Natural resource rents: Sum of country's rents for crude oil, natural gas, coal (hard and soft), minerals [103] and forestry (total roundwood harvest), as a share of GDP. The natural resource rents are calculated as the difference between the average cost of producing the commodity and its price. This is done by estimating the world price of units of specific commodities and subtracting estimates of average unit costs of extraction or harvesting costs; these unit rents are then multiplied by the physical quantities countries extract or harvest. *Source*: WDI (2020).

For those country years in which total natural resource rents as a share of GDP is missing, I proceeded in two ways. First, I established a lower boundary for natural resource rents by summing up all available rents for either oil, natural gas, coal, minerals and or forestry. Second, if I still had missing rents values, I followed Haber and Menaldo (2011) data interpolation procedure.

Polity IV composite index: This variable categorizes countries within a broad spectrum of regime authority, ranging from hereditary monarchies (-10) to consolidated democracies (+10). The index increases as countries become more democratic, achieving higher degrees of: i) competitiveness of political participation, ii) openness and iii) competitiveness of executive recruitment, and as there are less iv) constraints on the chief executive and v) regulation of participation. On the opposite, the indicator becomes more negative as countries are more autocratic, exhibiting lower degrees of the aforementioned variables.

Importantly, the polity index explicitly does *not* contemplate information on civil liberties. So this distinctive feature of political regimes is not being accounted for here.

Whenever the original polity IV variable was missing, I replaced it by the latest version of Teorell et al. (2019) imputed polity score. This variable closely mimics the rescaled democracy score, ranging from 0 (least democratic) to (10 most democratic). This scale adds imputed values

[103] Includes tin, gold, lead, zinc, iron, copper, nickel, silver, bauxite, and phosphate.

for countries where data on Polity is missing, by regressing Polity on the average Freedom House measure for level of democracy. Hadenius and Teorell (2005) convincingly assert that this averaged index is more valid and reliable than both Polity IV and Freedom House average freedom. Nonetheless, in compliance with my data compilation principles, I will keep polity IV (a primary source) as the main variable and use only the imputed scores whenever there is a missing value.

Regime type: As an alternative to the granular Polity IV score, the Center for Systemic Peace suggests arranging these into 3 broad categories, based on a country's polity IV value on any given year: autocracies (-10 to -6), anocracies (-5 to +5, plus special values -66,-77, and -88) and democracies (+6 to +10). Importanly, anocracies are mixed regime with characteristics of both democracies and autocracies. *Source*: Polity IV Project, Center for Systemic Peace (2018)

Democracy score (rescaled): This is a rescaled version of the Polity IV composite index, originally ranging from -10 (hereditary monarchy) to +10 (consolidated democracy). The index increases as countries become more democratic, achieving higher degrees of: i) competitiveness of political participation, ii) openness and competitiveness of executive recruitment, and as there are iii) less constraints on the chief executive. On the opposite, the indicator becomes more negative as countries are more autocratic, exhibiting lower degrees of the aforementioned variables. *Source*: Polity IV Project, Center for Systemic Peace (2018)

I rescaled this indicator so that it runs from 0 to 10, in the same spirit of Ali and Elbadawi (2016), by the following formula:

$$DemocScore_{ct} = \left[\frac{Min(Polity) - Polity_{ct}}{Min(Polity) - Max(Polity)}\right] * 10 \tag{2.13}$$

Where Min(Polity) and Max(Polity) represent the minimum and maximum possible scores

of the polity variable (-10 and 10, respectively), while $Polity_{ct}$ shows the score of country *c* in period *t*. $DemocScore_{ct}$ then ranges between 0 and 10, increasing as the polity variable becomes more positive.

Output per worker: GDP per worker, constant 2011 international USD in PPP. *Source*: Own calculations, based on WDI's (2020) GDP and ILO's (2020) total employment data.

Public sector employment: includes employees from the general government (local, state and central) and public enterprises. It covers all persons employed directly by these institutions, regardless of employment contract types. Argentina and Bolivia do not report data for public enterprises; therefore, for these public sector data will only include general government. *Source*: ILO (2020).

In the case of the GCC countries, I extracted employment data directly from official sources,[104] not from the ILO, since this is the only way to obtain *national* workers (as opposed to expatriates) employment numbers. While GCC countries do report some labor market statistics to the ILO, these usually contain total employment figures, including both country nationals and expatriates without distinction. Unlike other countries, the GCC has a very low presence of nationals in the private sector, which is predominantly occupied by foreign workers. Consequently, in order to properly reflect the relative importance of the public sector employment for GCC nationals, public sector employment shares should not include expatriate employment.

Regime duration: number of years since the most recent regime change, defined by either by: i) a three point change (positive or negative) in the polity score over a period of three years or less, or ii) the end of a transition period defined by the lack of stable political institutions.

[104]The few available yearly statistics come from the Central Statistical Bureau of the State of Kuwait (2019), the Labour Market Regulatory Authority of the Kingdom of Bahrain (2019), the General Authority for Statistics of the Kingdom of Saudi Arabia (2015), National Center for Statistics and Information of the Sultanate of Oman (2019). and the GLMM program (2019).

Computationally, the ”baseline year zero” (duration=0) is the first year during which a new polity is established; each subsequent year adds one to the value of the regime duration, until a new regime change or transition period occurs. The regime duration series begins with the first regime change since 1800, or the date of independence if it occurred after 1800. *Source*: Polity IV Project, Center for Systemic Peace (2018)

Trade: Sum of exports and imports of goods and services, as a share of GDP. When used in regressions, it was expressed in logs. *Source*: WDI (2020)

Urbanization: Share of urban population over total population. *Source*: WDI (2020).

Population: country's complete population, expressed in 10 millions. In all regressions it was expressed in logs. Whenever the original population variable was missing, I replaced it by the latest version of Feenstra et al. (2019) population. *Source*: WDI (2020)

2.B Additional graphs and tables

Table 2.12: Large N sample, observations per country and period. 1995-2017 period

Region	Country	N per period				
		Total	**1995-1999**	**2000-2004**	**2005-2009**	**2010-2017**
Africa	Botswana	13	2	5	5	1
(north excluded)	Burkina Faso	11	3	5	3	0
	Guinea	13	1	5	5	2
	Kenya	8	5	1	1	1
	Mauritius	23	5	5	5	8
	Senegal	11	3	5	1	2
	South Africa	23	5	5	5	8
	Tanzania	9	0	1	3	5
	Uganda	8	5	0	0	3

Table 2.12 – continued from previous page

Region	Country	N per period				
		Total	**1995-1999**	**2000-2004**	**2005-2009**	**2010-2017**
East Europe	Albania	19	5	5	4	5
& Central Asia	Armenia	23	5	5	5	8
	Azerbaijan	19	5	1	5	8
	Belarus	19	4	5	5	5
	Bosnia and Herzegovina	11	0	0	4	7
	Bulgaria	21	4	5	5	7
	Croatia	22	4	5	5	8
	Czech Republic	18	0	5	5	8
	Georgia	21	5	4	4	8
	Kazakhstan	10	5	1	1	3
	Kyrgyzstan	20	5	5	5	5
	Moldova	22	4	5	5	8
	Poland	23	5	5	5	8
	Russian Federation	15	0	5	5	5
	Macedonia	14	0	5	2	7
	Turkey	23	5	5	5	8
	Ukraine	14	3	3	5	3
Latin America	Argentina	21	4	5	5	7
& the Caribbean	Barbados	10	5	1	0	4
	Bolivia	18	5	1	5	7
	Brazil	21	5	4	5	7
	Chile	22	4	5	5	8
	Colombia	23	5	5	5	8
	Costa Rica	23	5	5	5	8
	Dominican Republic	22	4	5	5	8
	Ecuador	22	5	4	5	8
	El Salvador	15	5	1	1	8
	Guatemala	15	5	2	1	7
	Honduras	22	5	4	5	8
	Mexico	20	2	5	5	8
	Panama	22	5	5	4	8
	Paraguay	21	4	4	5	8
	Peru	17	0	4	5	8
	Trinidad and Tobago	20	4	5	4	7

Table 2.12 – continued from previous page

Region	Country	N per period				
		Total	**1995-1999**	**2000-2004**	**2005-2009**	**2010-2017**
	Uruguay	23	5	5	5	8
	Venezuela	15	5	0	5	4
Middle East	Bahrain	15	0	2	5	8
& North Africa	Egypt	17	0	4	5	8
	Iran	9	0	0	4	5
	Israel	20	3	5	5	7
	Jordan	14	4	5	5	0
	Kuwait	12	0	1	5	6
	Morocco	14	1	5	5	3
	Oman	13	0	0	5	8
	Qatar	8	0	0	0	8
	Saudi Arabia	21	3	5	5	8
	Syria	11	2	5	4	0
	Tunisia	8	0	0	4	4
	Yemen	6	0	2	3	1
North America	Canada	23	5	5	5	8
	United States	23	5	5	5	8
North, South	Austria	20	5	5	5	5
& West Europe	Belgium	19	1	5	5	8
	Cyprus	23	5	5	5	8
	Denmark	20	3	5	5	7
	Estonia	23	5	5	5	8
	Finland	23	5	5	5	8
	France	20	2	5	5	8
	Germany	23	5	5	5	8
	Greece	23	5	5	5	8
	Hungary	23	5	5	5	8
	Ireland	23	5	5	5	8
	Italy	23	5	5	5	8
	Latvia	22	4	5	5	8
	Lithuania	23	5	5	5	8
	Luxembourg	15	3	5	5	2

Table 2.12 – continued from previous page

Region	Country	N per period				
		Total	**1995-1999**	**2000-2004**	**2005-2009**	**2010-2017**
	Malta	20	5	5	5	5
	Netherlands	22	4	5	5	8
	Norway	23	5	5	5	8
	Portugal	17	0	4	5	8
	Slovakia	23	5	5	5	8
	Slovenia	23	5	5	5	8
	Spain	23	5	5	5	8
	Sweden	23	5	5	5	8
	Switzerland	21	4	5	5	7
	United Kingdom	18	1	4	5	8
South-East Asia & Pacific	Australia	16	5	5	3	3
	Cambodia	10	0	0	3	7
	Fiji	12	1	5	4	2
	Indonesia	9	5	3	0	1
	Malaysia	22	5	5	5	7
	New Zealand	17	5	5	5	2
	Philippines	23	5	5	5	8
	Singapore	10	5	0	4	1
	Thailand	22	5	5	4	8
	Viet Nam	17	0	5	4	8
Southern & Eastern Asia	India	11	5	5	1	0
	Japan	23	5	5	5	8
	Mongolia	9	0	0	2	7
	Pakistan	10	0	0	4	6
	Sri Lanka	18	2	3	5	8
World total		**1,841**	**363**	**398**	**435**	**645**

Table 2.11: Number of years in the sample. Frequency table

Years	Frequency	(%)	Cumulative
2	24	1.22	1.22
3	24	1.22	2.44
4	20	1.02	3.46
5	15	0.76	4.22
6	36	1.83	6.05
7	14	0.71	6.76
8	32	1.63	8.39
9	27	1.37	9.76
10	80	4.07	13.83
11	44	2.24	16.07
12	12	0.61	16.68
13	26	1.32	18.00
14	84	4.27	22.27
15	75	3.81	26.08
16	48	2.44	28.52
17	85	4.32	32.84
18	72	3.66	36.50
19	38	1.93	38.43
20	200	10.17	48.60
21	147	7.47	56.08
22	220	11.18	67.26
23	644	32.74	100
Total	1,967	100	

Table 2.13: Small N sample. Observations per country and period. 1995-2017 period

Region	Country	N per period				
		Total	1995-1999	2000-2004	2005-2009	2010-2017
Africa	Angola	3	0	1	1	1
(north excluded)	Cape Verde	2	0	0	0	2
	Congo	7	5	1	1	0
	Cote d'Ivoire	4	0	0	0	4
	Gabon	5	5	0	0	0
	Ghana	5	0	0	1	4
	Liberia	3	0	0	0	3
	Madagascar	6	1	1	1	3
	Malawi	2	1	0	0	1
	Mali	4	0	0	0	4
	Namibia	5	1	0	0	4
	Niger	2	0	0	0	2
	Nigeria	3	0	0	0	3
	Rwanda	2	0	0	0	2
	Timor-Leste	2	0	0	0	2

Table 2.13 – continued from previous page

Region	Country	N per period				
		Total	**1995-1999**	**2000-2004**	**2005-2009**	**2010-2017**
	Zambia	2	0	0	0	2
	Zimbabwe	6	5	0	0	1
Latin America	Cuba	2	0	0	1	1
& the Caribbean	Haiti	2	0	1	0	1
	Nicaragua	3	2	0	0	1
	Suriname	6	5	0	0	1
Middle East	Iraq	2	0	0	1	1
& North Africa	Yemen	6	0	1	3	2
South-East Asia	Laos	2	0	0	0	2
& Pacific	South Korea	6	2	0	0	4
Southern	Bangladesh	6	0	1	2	3
& Eastern Asia	Bhutan	4	0	0	1	3
	China	4	4	0	0	0
	Myanmar	2	0	0	0	2
World total		**126**	**33**	**6**	**17**	**70**

Table 2.14: Correlation between measures of government employment size. 1995-2017 period

		Public sector employment (PSE)			Wage expenditure
		% of employed	% of population	% of WAP	% Gov. Exp
	PSE (% population)	0.9581			
Africa	PSE (% WAP)	0.9695	0.9902		
(north excluded)	Wage exp.(% Gov. Exp.)	0.4596	0.5171	0.4805	
	Wage exp.(% GDP)	0.7838	0.7655	0.7542	0.7231
	PSE (% population)	0.7066			
Middle East	PSE (% WAP)	0.8188	0.9619		

Table 2.14 – continued from previous page

		PSE			Wage expenditure
		% of employed	% of population	% of WAP	% Gov. Exp
& North Africa	Wage exp. (% Gov. Exp.)	-0.5394	-0.3649	-0.4717	
	Wage exp. (% GDP)	-0.4543	-0.1552	-0.2202	0.7365
	PSE (% population)	0.7724			
Latin America	PSE (% WAP)	0.8065	0.9928		
& the Caribbean	Wage exp. (% Gov. Exp.)	-0.0265	-0.1656	-0.1389	
	Wage exp. (% GDP)	0.0944	0.0172	0.0301	0.5424
	PSE (% population)	0.9319			
Northern	PSE (% WAP)	0.9530	0.9969		
America	Wage exp. (% Gov. Exp.)	0.3147	0.5988	0.5601	
	Wage exp. (% GDP)	0.9500	0.9548	0.9634	0.5188
	PSE (% population)	0.9428			
Eastern Europe	PSE (% WAP)	0.9279	0.9976		
& Central Asia	Wage exp. (% Gov. Exp.)	0.4029	0.3667	0.3701	
	Wage exp. (% GDP)	0.4142	0.3813	0.4067	0.6256
	PSE (% population)	0.9310			
North, South	PSE (% WAP)	0.9279	0.9976		
& West Europe	Wage exp. (% Gov. Exp.)	0.4029	0.3667	0.3701	
	Wage exp. (% GDP)	0.4142	0.3813	0.4067	0.6256
	PSE (% population)	0.9698			
South-East	PSE (% WAP)	0.9810	0.9950		
Asia & Pacific	Wage exp. (% Gov. Exp.)	0.0075	0.1065	0.0946	
	Wage exp. (% GDP)	0.8933	0.9255	0.9336	0.2194
	PSE (% population)	0.9825			

Table 2.14 – continued from previous page

		PSE			Wage expenditure
		% of employed	% of population	% of WAP	% Gov. Exp
Eastern	PSE (% WAP)	0.9700	0.9946		
Asia	Wage exp. (% Gov. Exp.)	0.0289	-0.1456	-0.2013	
	Wage exp. (% GDP)	0.8619	0.7998	0.7673	0.3123
	PSE (% population)	0.9671			
Southern	PSE (% WAP)	0.9654	0.9997		
Asia	Wage exp. (% Gov. Exp.)	0.9064	0.8531	0.8465	
	Wage exp. (% GDP)	0.7224	0.6501	0.6366	0.8993

Table 2.15: Natural resource rents distribution, by ex-ante regime type. 1995-2017 period.

Sample	**Variable**	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**N**
Full sample	Q_W1	0.07	0.06	0	0.2	387
	Q_W2	0.49	0.2	0.2	0.87	386
	Q_W3	1.43	0.36	0.87	2.19	387
	Q_W4	4.2	1.58	2.19	7.38	387
	Q_W5	19.9	12.36	7.43	61.95	387
Autocracies	Q_A1	1.84	0.94	0.49	3.61	40
	Q_A2	6.26	1.41	3.69	8.51	39
	Q_A3	13.16	3.52	8.57	20.03	40
	Q_A4	26.78	4.61	20.22	34.83	40
	Q_A5	45.22	7.52	35.08	61.95	40
Democracies & anocracies	Q_D1	0.06	0.05	0	0.16	347
	Q_D2	0.38	0.15	0.16	0.71	347
	Q_D3	1.17	0.28	0.72	1.72	347
	Q_D4	2.98	1	1.72	5.26	347
	Q_D5	13.97	9.21	5.27	56.94	347

Table 2.16: Public sector employment as a share of the WAP. 1995-2017 period, by region

Region	*N*	*Mean*	*Std. Dev.*	*Min*	*Max*
Africa (but north)	184	5.28	3.70	0.43	12.63
Middle East & North Africa	167	12.86	9.16	4.20	49.70
Latin America & the Caribbean	380	7.69	3.45	1.59	18.08
North America	46	10.81	3.34	4.41	14.92
East Europe & Central Asia	314	16.12	8.42	6.32	45.13
North, South & West Europe	536	15.34	5.10	7.84	29.22
South-East Asia & Pacific	171	7.50	4.48	2.16	29.94
Southern & Eastern Asia	88	6.47	2.98	1.82	14.60

Source: Author's calculations from ILO (2020)

Table 2.17: Countries with large bureaucracies, by thresholds. 1995-2017 average

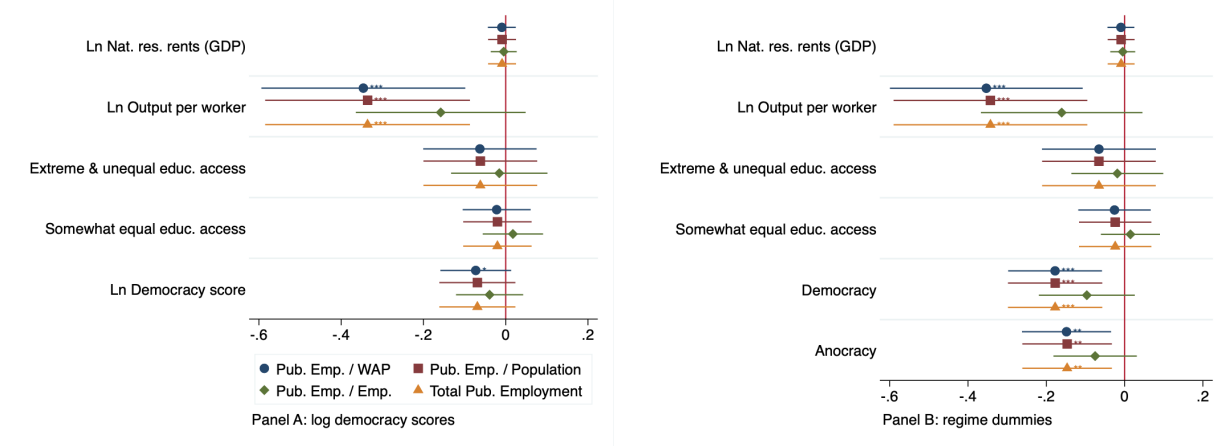
$25.3 \geq PSE/TE < 30.2\%$	$PSE/TE \geq 30.2\%$
Azerbaijan, Bahrain, Bulgaria, Czech Republic, Egypt, Estonia, Finland, France, Gabon, Hungary, Kazakhstan, Malta, Macedonia, Moldova, Poland, Russian Federation, Slovakia, Slovenia, Sweden, Syria, Trinidad and Tobago and Ukraine	Algeria, Belarus, Bosnia and Herzegovina, Croatia, Denmark Iraq, Kuwait, Latvia, Lithuania, Maldives, Norway, Saudi Arabia, Seychelles and Suriname

Note: The two thresholds correspond to the 75 and 90 percentiles of public sector employment as a share of total employment.

2.C Additional results

2.C.1 Basic Model

Figure 2.32: Public employment determinants regressions coefficients. Basic model with alternative definitions of (ln) public employment. 1995-2017 period.



Note: The regression specification is the same as in table's 2.19, column (4), but with alternative definitions of (ln) public employment. All regressions have 1,967 observations, include interactions between inequality and natural resource rents (consistently non-significant), control for urbanization, population, regime duration, dependency ratio and trade shares, and has country and year fixed effects and country clustered standard errors. $*\ p < 0.10$, $**\ p < 0.05$, and $***\ p < 0.01$

Table 2.18: Public employment determinants. Basic model with natural resource rents quintiles. 1995-2017 period.

Ln (Pub. Emp./ WAP)	**(1)**	**(2)**	**(3)**	**(4)**
Nat. res. rents - Q_W5	-0.073	-0.055	-0.073	-0.055
	(0.08)	(0.08)	(0.08)	(0.08)
Nat. res. rents - Q_W4	0.013	0.012	0.013	0.012
	(0.08)	(0.07)	(0.08)	(0.07)
Nat. res. rents - Q_W3	0.013	0.008	0.013	0.008
	(0.05)	(0.05)	(0.05)	(0.05)
Nat. res. rents - Q_W2	0.044	0.033	0.044	0.033
	(0.04)	(0.04)	(0.04)	(0.04)
Ln Output per worker	-0.477***	-0.354***	-0.477***	-0.354***
	(0.11)	(0.13)	(0.11)	(0.13)
Ln Democracy score	-0.104**	-0.075*	-0.104**	-0.075*
	(0.05)	(0.04)	(0.05)	(0.04)
Extreme & unequal educ. access	-0.111	-0.092	-0.111	-0.092
	(0.08)	(0.08)	(0.08)	(0.08)
Somewhat equal educ. access	-0.101*	-0.031	-0.101*	-0.031
	(0.05)	(0.06)	(0.05)	(0.06)
Nat. rents Q_W5 * Extremely unequal	0.197**	0.146*	0.197**	0.146*
	(0.08)	(0.08)	(0.08)	(0.08)
Nat. rents Q_W4 * Extremely unequal	0.052	0.019	0.052	0.019
	(0.07)	(0.07)	(0.07)	(0.07)
Nat. rents Q_W3 * Extremely unequal	0.020	-0.008	0.020	-0.008
	(0.05)	(0.05)	(0.05)	(0.05)
Nat. rents Q_W5 * Somewhat unequal	0.108	0.000	0.108	0.000
	(0.07)	(0.08)	(0.07)	(0.08)
Nat. rents Q_W4 * Somewhat unequal	0.052	-0.013	0.052	-0.013
	(0.09)	(0.09)	(0.09)	(0.09)
Nat. rents Q_W3 * Somewhat unequal	0.046	0.011	0.046	0.011
	(0.04)	(0.04)	(0.04)	(0.04)
Urbanization		0.013**		0.013**
		(0.01)		(0.01)
Ln population (in MM)		0.631***		0.631***
		(0.23)		(0.23)
Regime duration (years)		-0.000		-0.000
		(0.00)		(0.00)
Dependency ratio		0.402		0.402
		(0.29)		(0.29)
Ln trade share		0.048		0.048
		(0.08)		(0.08)
Year F.E.	No	No	Yes	Yes
Controls	No	Yes	No	Yes
R^2 (within)	0.158	0.229	0.158	0.229

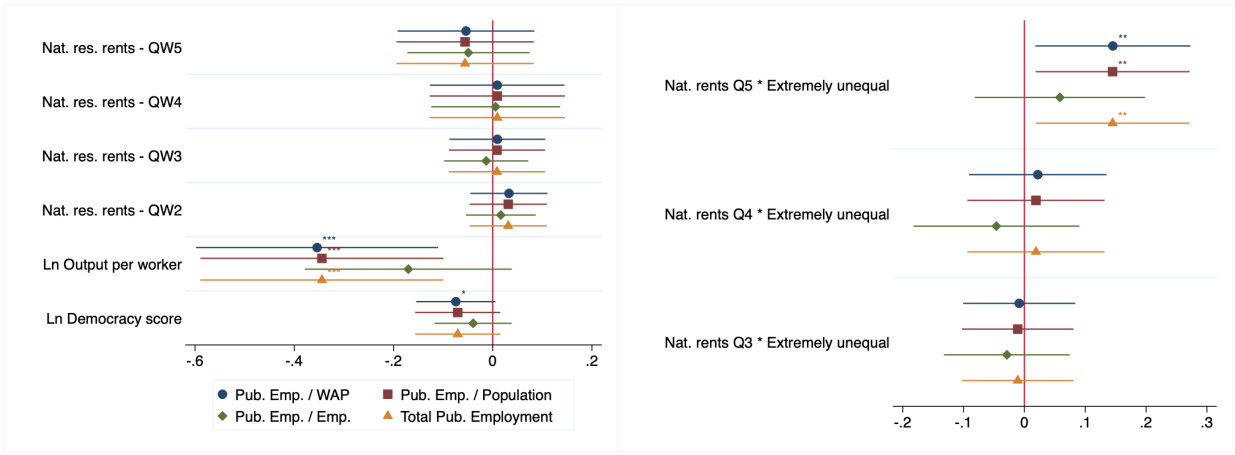
Note: All regressions have 1,967 observations, country fixed effects and country clustered standard errors (in parenthesis). When included, controls are urbanization, population, regime duration, dependency ratio and trade shares. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.19: Public employment determinants. Basic model, with regime type. 1995-2017 period.

Ln (Pub. Emp./ WAP)	**(1)**	**(2)**	**(3)**	**(4)**
Ln Nat. res. rents (% GDP)	0.011	-0.002	-0.006	-0.011
	(0.01)	(0.01)	(0.02)	(0.02)
Ln Output per worker	-0.308***	-0.396***	-0.482***	-0.355***
	(0.09)	(0.08)	(0.11)	(0.12)
Extreme & unequal educ. access	-0.071	-0.066	-0.058	-0.061
	(0.09)	(0.08)	(0.08)	(0.07)
Somewhat equal educ. access	-0.075	-0.029	-0.075	-0.026
	(0.05)	(0.05)	(0.05)	(0.05)
Democracy	-0.170**	-0.201***	-0.177***	-0.179***
	(0.07)	(0.06)	(0.07)	(0.06)
Anocracy	-0.116*	-0.164***	-0.139**	-0.149**
	(0.06)	(0.06)	(0.06)	(0.06)
Nat. rents * Extremely unequal	0.037	0.041	0.035	0.035
	(0.03)	(0.03)	(0.03)	(0.03)
Nat. rents * Somewhat equal	0.007	-0.005	0.015	-0.007
	(0.02)	(0.02)	(0.02)	(0.02)
Urbanization		0.013**		0.014**
		(0.01)		(0.01)
Ln population		0.529***		0.643***
		(0.19)		(0.23)
Regime duration (years)		-0.001		-0.001
		(0.00)		(0.00)
Dependency ratio		0.362		0.400
		(0.30)		(0.31)
Ln trade share		0.047		0.076
		(0.07)		(0.08)
Constant	5.626***	4.183***	7.398***	3.367*
	(0.92)	(0.92)	(1.11)	(1.75)
Year F.E.	No	No	Yes	Yes
R^2 (within)	0.104	0.201	0.144	0.215

Note: All regressions have 1,967 observations, country fixed effects and country clustered standard errors (in parenthesis). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 2.33: Public employment determinants regressions coefficients. Basic model with natural resource rents quintiles and alternative definitions of (ln) public employment. 1995-2017 period.



Note: The regression specification is the same as in table 2.18's column (4), but with alternative definitions of (ln) public employment. All regressions have 1,967 observations, include inequality dummies (omitted, non-significant) as well as its interaction with natural resource rents quintiles (all non-significant were omitted) urbanization, population, regime duration, dependency ratio and trade shares controls, country and year fixed effects and country clustered standard errors. $* \ p < 0.10$, $**$ $p < 0.05$, and $* * * \ p < 0.01$

2.C.2 The differential joint effect of natural resource rents and institutions

Table 2.20: Public employment determinants, with natural resource rents quintiles. 1995-2017 period.

Ln (Pub. Emp./ WAP)	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Nat. res. rents - Q_W5	0.032	-0.656	-0.798**	-0.042	-0.554	-0.839**
Nat. res. rents - Q_W4	0.075	-0.468	-0.632*	0.024	-0.342	-0.666*
Nat. res. rents - Q_W3	0.058	-0.160	-0.353	0.020	-0.011	-0.362
Nat. res. rents - Q_W2	0.069*	-0.002	-0.323	0.042	0.076	-0.365
Ln Output per worker	-0.299***	-0.295***	-0.367***	-0.476***	-0.466***	-0.324**
Extreme & unequal educ. access	-0.069	-0.106	-0.078	-0.051	-0.082	-0.068
Ln Democracy score	-0.112**	-0.307	-0.362**	-0.107**	-0.233	-0.356**
Nat. rents Q_W5 * Extremely unequal	0.170**	0.204***	0.171**	0.148**	0.175***	0.155**
Nat. rents Q_W4 * Extremely unequal	0.052	0.096	0.049	0.029	0.067	0.033
Nat. rents Q_W3 * Extremely unequal	0.017	0.058	0.013	0.004	0.040	0.005
Nat. rents Q_W5 * Ln democracy		0.327*	0.365**		0.251	0.365**
Nat. rents Q_W4 * Ln democracy		0.250	0.304*		0.173	0.306*
Nat. rents Q_W3 * Ln democracy		0.099	0.170		0.018	0.164
Nat. rents Q_W2 * Ln democracy		0.030	0.162		-0.016	0.174
Year F.E.	No	No	No	Yes	Yes	Yes
Controls	No	No	Yes	No	No	Yes
R^2 (within)	0.115	0.131	0.207	0.158	0.172	0.225

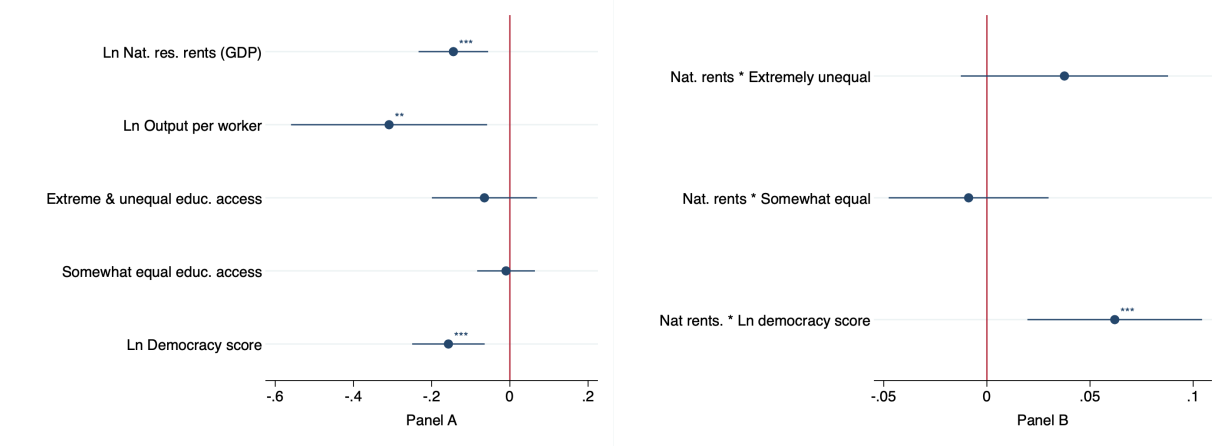
Note: All regressions have 1,967 observations, country fixed effects and country clustered standard errors. When included, controls are urbanization, population, regime duration and dependency ratio. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.21: Public employment determinants. Log of natural resource rents as main independent variable. 1995-2017 period.

Ln (Pub. Emp./ WAP)	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Ln Nat. res. rents (% GDP)	0.011	-0.132***	-0.107***	-0.006	-0.126***	-0.116***
Ln Output per worker	-0.308***	-0.281***	-0.382***	-0.482***	-0.437***	-0.332***
Extreme & unequal educ. access	-0.071	-0.133	-0.077	-0.058	-0.114	-0.074
Democracy	-0.170**	-0.394***	-0.349***	-0.177***	-0.362***	-0.341***
Anocracy	-0.116*	-0.280***	-0.270***	-0.139**	-0.269***	-0.270***
Nat. rents * Extremely unequal	0.037	0.059**	0.048*	0.035	0.054**	0.043
Nat. rents * Democracy		0.157***	0.115***		0.134***	0.117***
Nat. rents * Anocracy		0.095**	0.074**		0.077**	0.076**
Year F.E.	No	No	No	Yes	Yes	Yes
Controls	No	No	Yes	No	No	Yes
R^2 (within)	0.104	0.129	0.188	0.144	0.162	0.206

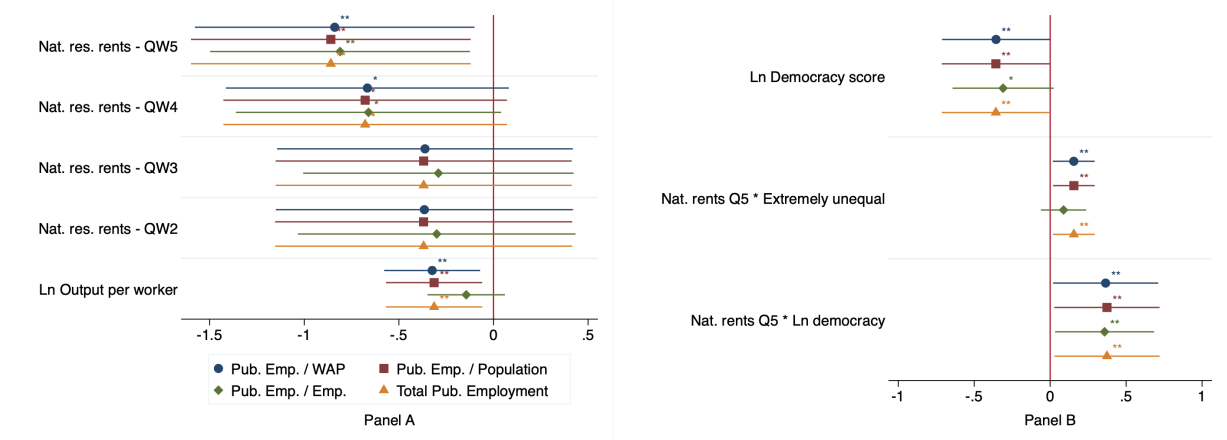
Note: All regressions have 1,967 observations, country fixed effects and country clustered standard errors. When included, controls are urbanization, population, regime duration and dependency ratio. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 2.34: Ln public employment as a share of WAP determinants, regressions coefficients. Log of natural resource rents as main independent variable. 1995-2017 period.



Note: The regression has 1,967 observations, includes urbanization, population, regime duration and dependency ratio controls, country and year fixed effects and country clustered standard errors. Full regression results available in table 2.21. $* p < 0.10$, $** p < 0.05$, and $* * * p < 0.01$

Figure 2.35: Ln public employment determinants regressions coefficients, as per different public employment measures. 1995-2017 period.



Note: The regression has 1,967 observations, includes urbanization, population, regime duration and dependency ratio controls, country and year fixed effects and country clustered standard errors. Full regression results available in appendix table 2.20. $* p < 0.10$, $** p < 0.05$, and $* * * p < 0.01$

Table 2.22: Predicted public employment as a share of WAP determinants, by natural resource rents quintiles and regime type. Marginal effects. 1995-2017 period.

		Prediction (%)	**95% Conf. interval**	
	Q_W1	7,22	5,89	8,86
	Q_W2	8,39	7,69	9,16
NRR	Q_W3	8,84	7,84	9,96
	Q_W4	14,50	11,33	18,55
	Q_W5	10,02	9,07	11,08
NRR	Autocracy	.	.	.
Q_W1	Democracy	7,61	6,30	9,21
NRR	Autocracy	18,73	13,46	25,79
Q_W2	Democracy	8,85	7,92	9,68
NRR	Autocracy	16,44	12,30	21,98
Q_W3	Democracy	10,28	8,94	11,82
NRR	Autocracy	14,88	11,25	19,89
Q_W4	Democracy	17,29	11,70	25,79
NRR	Autocracy	8,85	6,89	11,25
Q_W5	Democracy	12,06	10,18	14,30

Note: NRR= Natural resource rents. This table presents the marginal effects, at mean values, of table's 2.20 column (6). The regression has 1,967 observations, country fixed effects and country clustered standard errors. Included controls are urbanization, population, regime duration and dependency ratio. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

2.C.3 Separate samples: Ex-ante institutions

Table 2.23: Regime change frequency. 1995-2017 period.

	$Autocracy_c$ (1995)	$Anocracy_c$ (1995)	$Democracy_c$ (1995)	*Total*
$Autocracy_{ct}$	174	10	0	184
(%)	*86.6*	*6.1*	*0.0*	*9.4*
$Anocracy_{ct}$	20	289	48	357
(%)	*10.6*	*60.9*	*3.7*	*18.2*
$Democracy_{ct}$	5	157	1,264	1,426
(%)	*2.8*	*33.0*	*96.4*	*73.0*
Total	199	456	1,312	1,967
(%)	*100*	*100*	*100*	*100*

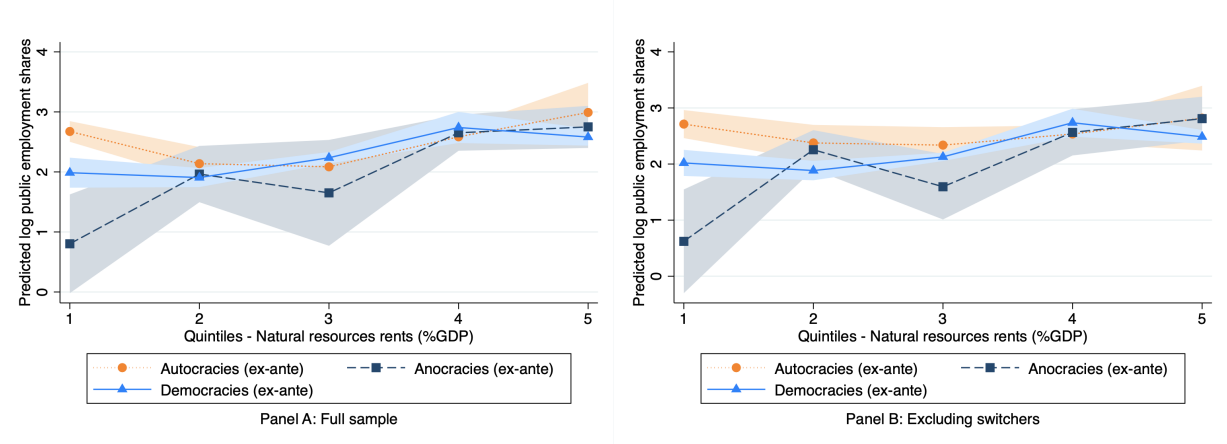
Note: This table shows the chances that a given country *c* changes its ex-ante regime (in year 1995) at any point *t* in time, during the entire period under study (1995-2007).

Table 2.24: Public employment determinants. Separate samples, by countries' ex-ante regime type. 1995-2017 period.

Ln (Pub. Emp./ WAP)	**Autocracies (ex-ante)**			**Democracies & Anocracies (ex-ante)**		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Ln output per worker	-0.362**	-0.429***	-0.441***	-0.469***	-0.285**	-0.268*
Nat. res. rents - Q5	-0.177	-0.030	-0.063	-0.030	-0.052	-0.060
Nat. res. rents - Q4	-0.137	-0.038	-0.046	0.006	-0.011	-0.005
Nat. res. rents - Q3	-0.152	-0.106	-0.093	-0.001	-0.004	0.001
Nat. res. rents - Q2	-0.061	-0.075	-0.040	0.047	0.046	0.049
Ln Democracy score	-0.016	-0.095	-0.138	-0.149**	-0.125**	-0.128**
Extreme & unequal educ. access	0.222*	0.204*	0.278	-0.103	-0.131*	-0.135**
Nat. rents Q5 * Extremely unequal	-0.320**	-0.347***	-0.416***	0.184***	0.197***	0.209***
Nat. rents Q4 * Extremely unequal	-0.219**	-0.217**	-0.318***	0.078	0.094	0.092
Nat. rents Q3 * Extremely unequal	0.070	0.055	-0.052	0.028	0.033	0.034
Standard errors	Robust	Robust	Robust	Cluster	Cluster	Cluster
Controls	No	Yes	Yes	No	Yes	Yes
Exclusions	No	No	Switchers	No	No	Switchers
R^2 (within)	0.395	0.536	0.527	0.153	0.228	0.194
Observations	199	199	167	1768	1768	1751

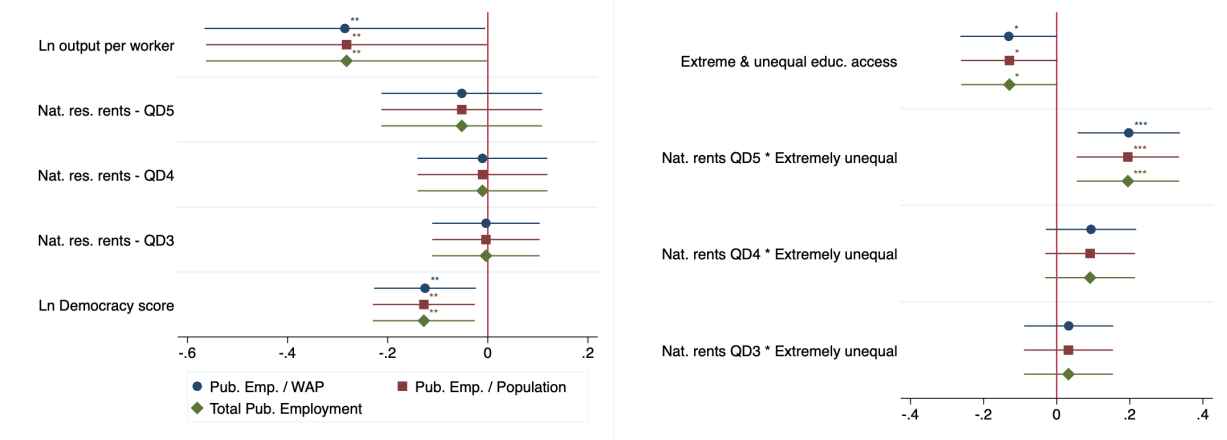
Note: For ex-ante autocracies, "switchers" are those that became anocracies or democracies at some point in the period under study, while for ex-ante democracies, "switchers" are those that became autocracies at some point. All regressions have country and year fixed effects, and when included controls are urbanization, population, regime duration and dependency ratio. Democracies & anocracies standard errors are country clustered. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 2.36: Predicted Ln public employment as a share of WAP determinants. Separate samples overlapped in one graph, by ex ante regime type. 1995-2017 period.



Note: This figure presents the marginal effects (at means) plus 95 % confidence intervals of table 2.24's regressions, by natural resource rents quintiles and yearly regime type. Panel A includes columns (2) and (5), and panel B columns (3) and (6). For reversed values (non logs) of the marginal effects, please refer to appendix table 2.25. All regressions have urbanization, population, regime duration and dependency ratio controls, country and year fixed effects.

Figure 2.37: Ln public employment determinants in ex-ante democracies and anocracies, by different definitions of public employment. 1995-2017 period.



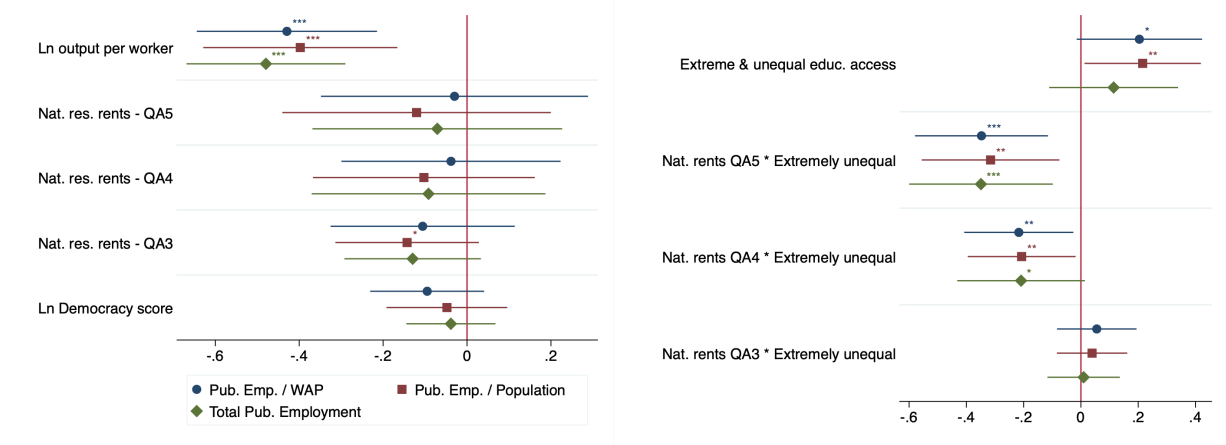
Note: All regressions have N=1,768, urbanization, population, regime duration and dependency ratio controls, country and year fixed effects, and country clustered standard errors.

Table 2.25: Predicted public employment as a share of WAP determinants, by ex-ante regime types. Marginal effects, by natural resource rents quintiles and yearly regime type. 1995-2017 period.

		Autocracies (ex-ante) Prediction (%)	95% Conf. interval		Democracies &Anocracies (ex-ante) Prediction (%)	95% Conf. interval	
NRR	Autocracy	16,3	13,7	19,4	.	.	.
Q1	Anocracy	7,3	4,5	11,7	1,8	0,7	4,4
	Democracy	7,8	4,9	12,8	7,6	6,0	9,5
NRR	Autocracy	8,1	5,9	11,1	7,9	3,6	17,2
Q2	Anocracy	5,79	3,33	10,07	8,6	5,9	12,6
	Democracy	5,76	2,77	11,98	6,6	5,6	7,7
NRR	Autocracy	9,4	7,9	11,2	.	.	.
Q3	Anocracy	4,2	2,2	8,1	4,8	2,7	8,6
	Democracy	5,8	3,0	11,2	8,5	8,9	9,1
NRR	Autocracy	12,6	11,5	13,9	.	.	.
Q4	Anocracy	.	.	.	11,5	8,0	16,6
	Democracy	.	.	.	16,1	12,4	20,8
NRR	Autocracy	23,3	12,7	42,8	7,2	5,6	9,4
Q5	Anocracy	.	.	.	16,7	5,6	24,0
	Democracy	.	.	.	12,5	11,0	14,2

Note: NRR= Natural resource rents. This table presents the marginal effects, at mean values, of table 2.24 regressions, columns (2) and (5). All regressions have urbanization, population, regime duration and dependency ratio controls, country and year fixed effects. "Autocracies" standard errors are robust, while those for democracies and anocracies are country clustered. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 2.38: Ln public employment determinants in ex-ante autocracies, by different definitions of public employment. 1995-2017 period.



Note: All regressions have N=199, urbanization, population, regime duration and dependency ratio controls, country and year fixed effects, and robust standard errors.

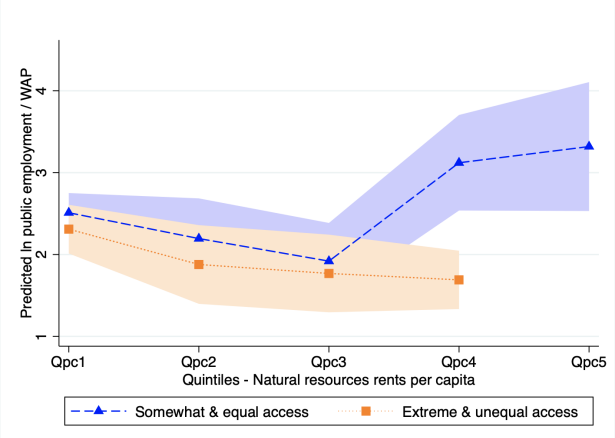
2.C.4 Robustness tests: natural resource rents per capita

Table 2.26: Public employment determinants of ex-ante autocracies. Natural resource rents per capita and different inequality definitions. 1995-2017 period.

Ln (Pub. Emp./ WAP)	**(1)**	**(2)**	**(3)**	**(4)**
Ln output per worker	-0.318*	-0.384***	-0.338*	-0.387***
Nat. res. rents p.c. - Q5	0.126	0.236	0.006	0.179
Nat. res. rents p.c. - Q4	0.124	0.148*	0.004	0.090
Nat. res. rents p.c. - Q3	0.006	-0.037	-0.111	-0.097
Nat. res. rents p.c. - Q2	0.079*	0.019	0.076	0.017
Ln Democracy score	0.016	-0.095	0.024	-0.093
Extreme & unequal access	0.062	0.017	-0.104	-0.012
Nat. rents p.c. Q5 * Extremely unequal	0.000	0.000	0.000	0.000
Nat. rents p.c. Q4 * Extremely unequal	-0.506**	-0.438***	-0.322	-0.338**
Nat. rents p.c. Q3 * Extremely unequal	-0.063	-0.026	0.121	0.076
Inequality type	Educ.	Educ.	Health	Health
Controls	No	Yes	No	Yes
R^2 (within)	0.408	0.538	0.410	0.538

Note: All regressions have N=199, country and year fixed effects and country clustered standard errors. When used, controls are urbanization, population, regime duration and dependency ratio. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 2.39: Predicted Ln public employment as a share of WAP for ex-ante autocracies. Natural resource rents per capita and health access inequality. 1995-2017 period.



Note: This figure presents the marginal effects (at means) plus 95 % confidence intervals of table 2.26's regressions column (4), by natural resource rents per capita quintiles and health access inequality. The regression has urbanization, population, regime duration and dependency ratio controls, country and year fixed effects and robust standard errors.

2.C.5 Robustness tests: Sample exclussions

Table 2.27: Public employment determinants, excluding countries with less than 8 observations. 1995-2017 period.

Ln (Pub. Emp./ WAP)	**(1)**	**(2)**	**(3)**	**(4)**
Ln output per worker	-0.429***	-0.437***	-0.285**	-0.302**
Nat. res. rents - Q5	-0.030	-0.006	-0.052	-0.044
Nat. res. rents - Q4	-0.038	-0.010	-0.011	-0.002
Nat. res. rents - Q3	-0.106	-0.094	-0.004	0.003
Nat. res. rents - Q2	-0.075	-0.072	0.046	0.050
Ln Democracy score	-0.095	-0.086	-0.125**	-0.162***
Extreme & unequal educ. access	0.204*	0.269	-0.131*	-0.128*
Nat. rents Q5 * Extremely unequal	-0.347***	-0.379***	0.197***	0.194***
Nat. rents Q4 * Extremely unequal	-0.217**	-0.257**	0.094	0.055
Nat. rents Q3 * Extremely unequal	0.055	0.041	0.033	-0.004
Exclusions	None	Small N	None	Small N
Ex-ante institutions	Autoc.	Autoc.	D&A	D&A
Standard errors	Robust	Robust	Cluster	Cluster
Observations	199	185	1768	1656
R^2 (within)	0.549	0.567	0.228	0.259

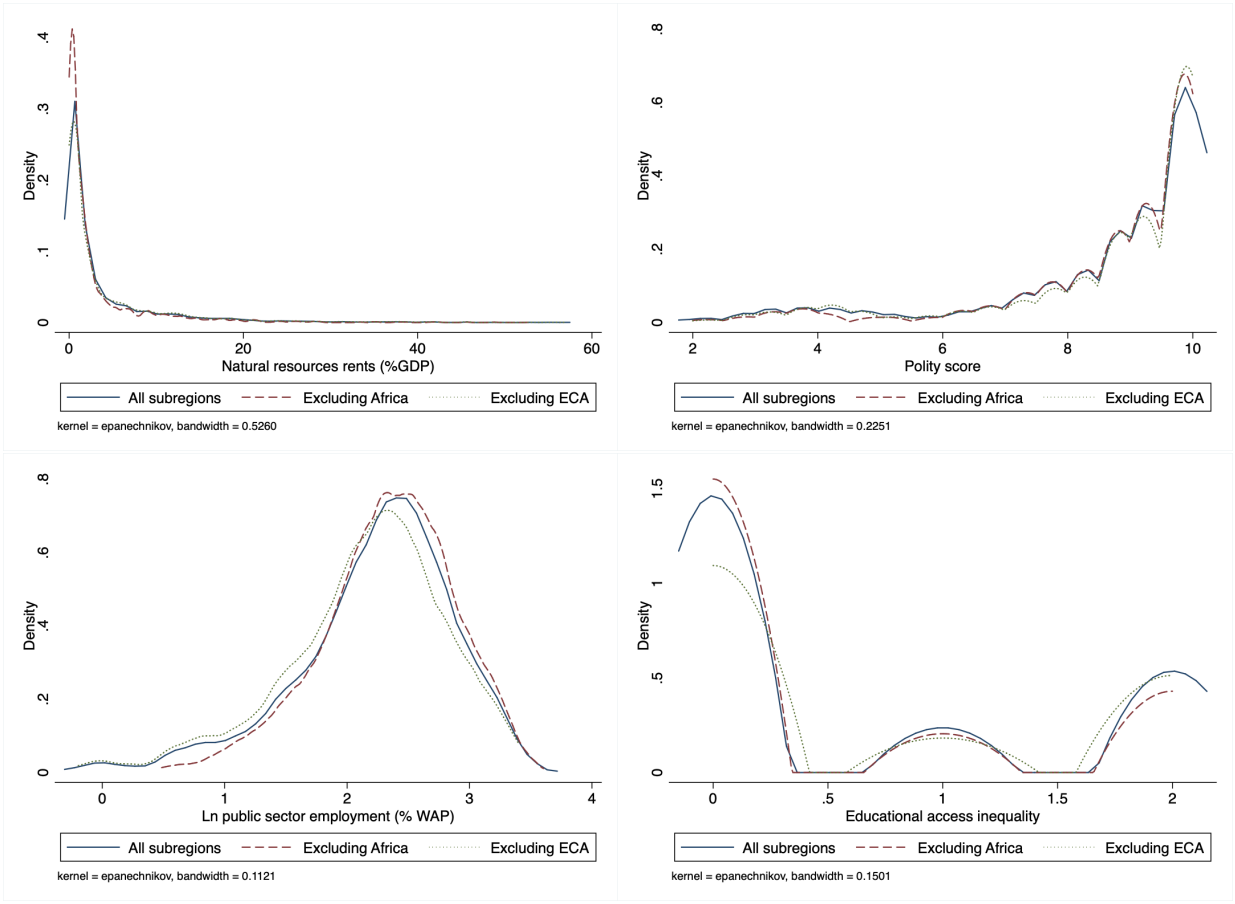
Note: Autoc= ex-ante autocracies, and D&A= ex-ante democracies and anocracies. All regressions control for urbanization, population, regime duration and dependency ratio, country and year fixed effects, and D&A standard errors are country clustered. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.28: Public employment determinants of ex-ante democracies and anocracies. Excluding subregions. 1995-2017 period.

Ln (Pub. Emp./ WAP)	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Ln output per worker	-0.345**	-0.280**	-0.307*	-0.315**	-0.095	-0.332*	-0.156	-0.298**
Nat. res. rents - Q5	0.011	-0.084	-0.038	-0.137**	-0.061	-0.022	-0.072	-0.071
Nat. res. rents - Q4	-0.038	-0.025	-0.092	-0.068	0.003	0.038	-0.012	-0.005
Nat. res. rents - Q3	-0.030	-0.020	-0.063	-0.039	-0.055	-0.048	-0.013	0.002
Nat. res. rents - Q2	0.034	0.029	-0.003	0.015	-0.003	-0.047	0.044	0.053
Ln Democracy score	-0.138***	-0.131**	-0.107*	-0.119**	-0.061	-0.098*	-0.165***	-0.123**
Extreme & unequal educ. access	-0.077	-0.115*	-0.169**	-0.130**	0.011	-0.089	-0.169**	-0.097
Nat. rents Q5 * Extremely unequal	0.126	0.181***	0.046	0.204***	0.090	0.162**	0.240***	0.162**
Nat. rents Q4 * Extremely unequal	0.056	0.066	0.174**	0.085	-0.049	-0.000	0.119	0.041
Nat. rents Q3 * Extremely unequal	0.001	0.010	0.105	0.011	-0.023	0.084**	0.086	-0.020
Excluded	Afr.	MENA	LAC	NAm	ECA	NSWE	SEAP	ESA
Controls & fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1566	1729	1382	1719	1453	1228	1611	1681
R^2 (within)	0.241	0.224	0.271	0.285	0.101	0.291	0.246	0.235

Note: All regressions control for urbanization, population, regime duration and dependency ratio, and include country and year fixed effects and country clustered standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 2.40: Main variables distributions, with and without Africa and ECA subregions. 1995-2017 period.



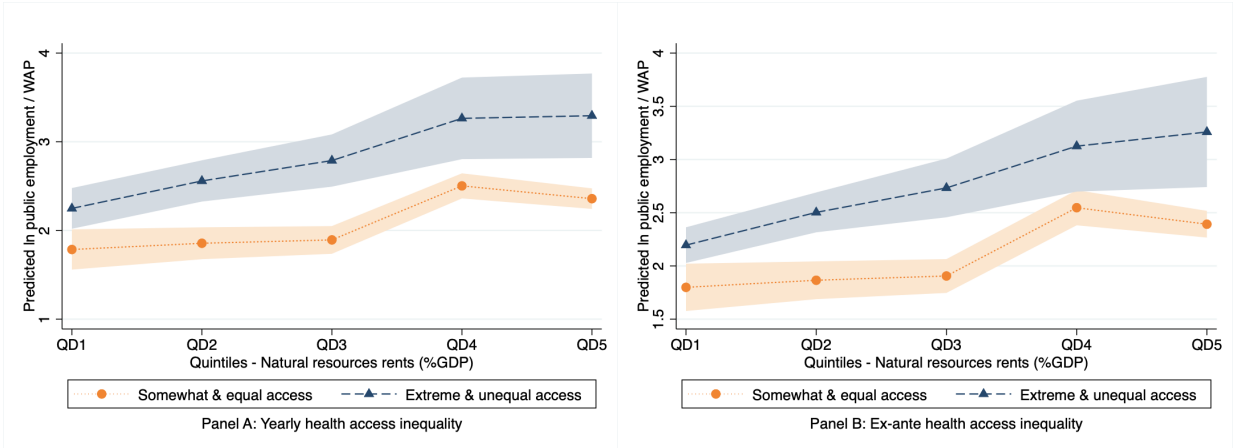
2.C.6 Robustness tests: The role of inequality

Table 2.29: Public employment determinants of ex-ante democracies and anocracies. Different inequality definitions. 1995-2017 period.

Ln (Pub. Emp./ WAP)	**(1)**	**(2)**	**(3)**	**(4)**
Ln output per worker	-0.285**	-0.324**	-0.328**	-0.330**
Nat. res. rents - Q5	-0.052	-0.042	-0.029	-0.006
Nat. res. rents - Q4	-0.011	-0.014	-0.005	-0.010
Nat. res. rents - Q3	-0.004	-0.003	-0.000	-0.002
Nat. res. rents - Q2	0.046	0.041	0.042	0.041
Ln Democracy score	-0.125**	-0.099**	-0.096*	-0.102*
Extreme & unequal educ. access	-0.131*		0.036	
Nat. rents Q5 * Extremely unequal	0.197***	0.144*	0.111	0.044
Nat. rents Q4 * Extremely unequal	0.094	0.080	0.046	0.050
Nat. rents Q3 * Extremely unequal	0.033	0.023	0.003	0.001
Inequality type	Educ.	Educ.	Health	Health
Inequality period	Annual	Ex-ante	Annual	Ex-ante
Controls & fixed effects	Yes	Yes	Yes	Yes
R^2 (within)	0.228	0.239	0.240	0.237

Note: All regressions have N=1,768, control for urbanization, population, regime duration and dependency ratio, country and year fixed effects and country clustered standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 2.41: Predicted Ln public employment as a share of WAP determinants for ex-ante anocracies and democracies. By annual and ex-ante health access inequality. 1995-2017 period.



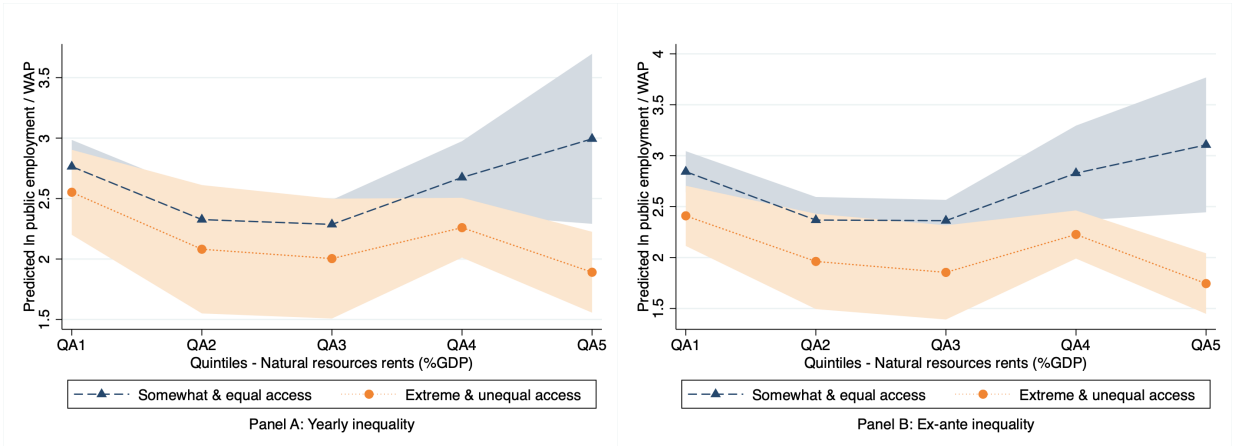
Note: This figure presents the marginal effects (at means) plus 95 % confidence intervals of table 2.29's regressions, columns (3) and (4), by natural resource rents quintiles and health access inequality. Both regressions have urbanization, population, regime duration and dependency ratio controls, country and year fixed effects.

Table 2.30: Public employment determinants of ex-ante autocracies. Different inequality definitions. 1995-2017 period.

Ln (Pub. Emp./ WAP)	**(1)**	**(2)**	**(3)**	**(4)**
Ln output per worker	-0.429***	-0.420***	-0.390***	-0.402***
Nat. res. rents - Q5	-0.030	-0.013	-0.096	-0.058
Nat. res. rents - Q4	-0.038	-0.022	-0.110	-0.074
Nat. res. rents - Q3	-0.106	-0.103	-0.130	-0.111
Nat. res. rents - Q2	-0.075	-0.069	-0.083	-0.081
Ln Democracy score	-0.095	-0.092	-0.091	-0.090
Extreme & unequal educ. access	0.204*		0.113	
Nat. rents Q5 * Extremely unequal	-0.347***	-0.379***	-0.337**	0.361***
Nat. rents Q4 * Extremely unequal	-0.217**	-0.251**	-0.195*	-0.235**
Nat. rents Q3 * Extremely unequal	0.055	0.047	0.053	0.050
Inequality type	Educ.	Educ.	Health	Health
Inequality period	Annual	Ex-ante	Annual	Ex-ante
Controls & fixed effects	Yes	Yes	Yes	Yes
R^2 (within)	0.536	0.536	0.530	0.532

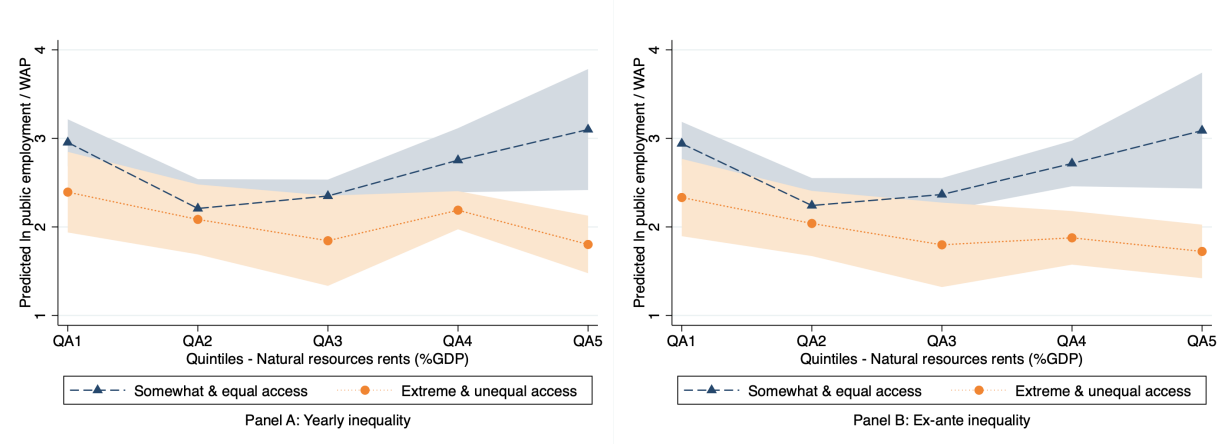
Note: All regressions have N=199, control for urbanization, population, regime duration and dependency ratio, country and year fixed effects and robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 2.42: Predicted Ln public employment as a share of WAP determinants for ex-ante autocracies. By annual and ex-ante educational access inequality. 1995-2017 period.



Note: This figure presents the marginal effects (at means) plus 95 % confidence intervals of table 2.30's regressions, columns (1) and (2), by natural resource rents quintiles and educational access inequality. Both regressions have urbanization, population, regime duration and dependency ratio controls, country and year fixed effects.

Figure 2.43: Predicted Ln public employment as a share of WAP determinants for ex-ante autocracies. By annual and ex-ante health access inequality. 1995-2017 period.



Note: This figure presents the marginal effects (at means) plus 95 % confidence intervals of table 2.30's regressions, columns (3) and (4), by natural resource rents quintiles and health access inequality. Both regressions have urbanization, population, regime duration and dependency ratio controls, country and year fixed effects.

2.D Robinson and Verdier's modified standard probabilistic voting model

2.D.1 Basic set-up

The game-theoretical model focuses on four main agents: the incumbent political decision-maker (P) and the challenging political decision-maker (N), plus the two groups of voters each politician caters to, g_P and g_N. Both groups of agents will have a set of preferences and budgets, and the game will proceeds in four steps:

1. The incumbent decision-maker (P) chooses the amount of public investment I, which affects average productivity and as such determine private sector jobs income;

2. Both politicians compete an election by offering distinct policy bundles $B(T_g^j, m_g^j, n_g^j)$, consisting on a combination of group specific: i) tax rates T_g^j, ii) income transfers m_g^j; and iii) government employment positions n_g^j. Importantly, the qualitative difference between public sector employment and a pure transfer of income is that the former generates rents for the politician, some of which can directly be translated to the employee.

3. The winner of the election takes power and optimally choses the policy bundle to implement $\tilde{B}(\tilde{T_g^j}, \tilde{m_g^j}, \tilde{n_g^j})$. This, of course, tends to differ from the bundle B offered at election time.

4. Finally, production, taxation and consumption take place.

Voters maximize their utilities, where a representative agent i from group g will have a linear utility function U_i^P if voting for the patron and $U_i^N = c_i$ otherwise. When voting for the

patron the utility function will depend on i's consumption level c_i, the effort exerted in production e_i with its associated cost in utility terms of $\psi(e_i)$, as well as i's ideological proclivities δ_i and θ.

$$U_i^P = c_i - \psi(e_i) + \delta_i + \theta \tag{2.14}$$

Workers consumption depends on their income, which primarily derives from two employment options: the private and the public sector. When working in the private *formal* sector, a worker's pre-tax income of $Ay(I)$ depends on the public investment I chosen by the incumbent politician, as well as on total factor productivity (A). On the other hand, in the *informal* private sector income is only $\alpha Ay(I)$, a share of that in the formal one. Informality has the advantage that it avoids tax payments, but at the cost of losing an income share of $(1 - \alpha)$, due to the inherently lower productivity in the sector.

When working in the public sector, voters choose an effort level $e \in \{0, \epsilon\}$, observed with probability q_g^j. As a result, each voter will earn a wage of W_g^j, which increases with effort and decreases with the probability of this effort being observed. Both effort and wages depend on who wins the election ($j = P, N$) and what group the worker belongs to ($g = 1, 2$). A worker will find in his best interest to accept public employment if the wages derived from it are higher than the income he could get from the private informal sector.

On the other hand, **patrons want to maximize expected consumption**, which depends on them being in power (otherwise they get rent zero) and is maximized by a combination of tax revenues and profits from public employment, minus transfers. Profits from public employment will be $\Pi = R(\epsilon) - \Psi(\epsilon)/q_1^P$, a rent generating technology in which rents $R(\epsilon)$ are taxed by the associated cost in production $\Psi(\epsilon)$, but such "tax" decreases the higher the probability of

observing public sector employees effort for its constituency (q_1^P) is.

Patrons compete offering a combination of the policy instrument B that maximizes their expected consumption, while providing direct rents to individuals (in the form of public employment or transfers) and/or - productivity enhancing - public goods. The specific pre-election offered combination of policy bundles $B^P(T_g^j, m_g^j, n_g^j)$ and $B^N(T_g^j, m_g^j, n_g^j)$ (the incumbent's and challenger's proposals, respectively) will depend on the needs of the voters g_N and g_P, each of which represents a certain share of the population λ_g that gets utility from both the politician's policy choices and their personality traits and ideology.

Summing up, equation 2.15 shows that patrons will maximize their consumption by choosing the maximum level of public investment I that will allow them to extract the most taxes from those that are not his clients, while extracting the most rents they can out of those that are clients and as such employed in the public sector.

$$max_I V_c^P(I, A) = P^P(I)\{(1-\alpha)\tau A y(I)\lambda_2 + [R(e) - w_1^P]\lambda_1\} - I \tag{2.15}$$

2.D.2 Model assumptions

For the model to hold, we need to impose two assumptions, critical for reducing moral-hazard and for enabling patrons to potentially make credible employment offers.

A1. On moral hazard: the probability for policy makers to observe public sector employees efforts increases when they belong to the same social network

Formally, for both politicians $j = P, N$,

$$1/qN_1 > R(\epsilon)/\psi(\epsilon) > 1/qP_1 \tag{2.16}$$

$$1/qj_2 > R(\epsilon)/\psi(\epsilon) \tag{2.17}$$

The assumption that members of a group can observe each others actions better than those of members of other groups is a standard one in the political economy literature. In this line of thought, the model claims that when workers and patrons are in the same social network, patrons are more likely to observe effort of such employees. Furthermore, patrons' expected consumption is also to increase with the probability of observing effort of public employees, by either of the following mechanisms:

1. Patron's rents (that is, "rents from public employment") can be directly generated by workers effort itself: there are higher returns to their work, given their wages W_g^j;

2. Alternatively, the probability of observing effort and behavior might not directly affect rents (the politician has another mechanism to produce rents), but it makes employment offers more credible, since it implies politicians can also observe voting behavior of those in their group and give or take away jobs based on such outcome.

An important consequence is that the incumbent can *only* make rents of employing voters from group 1, and the challenging politicians can only make rents of employing voters of group 2. Only the incumbent (P) can make positive rents from employing group 1 in the public sector, since P and $g1$ are in the same social network, so P can observe effort with higher probability. The same logic applies to the challenging political decision-maker (N) and $g2$, who are in the same social network.

A2. Binding constraints, and the credibility of public employment offers

Once the winner politician takes power, he will optimally chose the policy bundle that maximizes his consumption: $\tilde{B}(\tilde{T_g^j}, \tilde{m_g^j}, \tilde{n_g^j})$. First, for the model to hold it must be true that $\tilde{m_g^j} = 0$ for all *j* and *g*; this implies that post-electoral income transfers are necessarily not credible. Then, $\tilde{T_g^j}$ will be chosen as to maximize revenues, set in order to make voters indifferent between staying in the formal sector or moving to the informal one: $\tilde{T_g^j} = (1-\alpha)Ay(I)$.

Finally, we must consider the politician's public sector employment choice ($\tilde{n_g^j}$). Public-sector employment is different to a pure transfer of income because it generates rents for the patron; furthermore because of moral hazard, some of these rents can be transferred to employees in the form of efficiency wages. However, commitments to transfer rents to clients by employing them are only credible within certain circumstances.

Given a wage offer W_g^P, a client will exert effort if the probability is of being observed (q_g^j) is high enough to compensate the effort made at a given wage level. This yields an efficiency wage to be offered by the incumbent pattern of $W_g^P = \psi(\epsilon)/q_P^j$, which decreases with q_g^j.

As a result, a voter's standard participation constraint (equation 2.18) will be one such that he will only accept public sector employment if the efficiency wage offered minus the exerted effort is no less than the income he would earn in the informal private sector.

$$W_g^P - \psi(\epsilon) \geq \alpha Ay(I) \tag{2.18}$$

Taken together, these two sets of constraints imply that the public sector wage offer of the incumbent patron is also tied up to public investment levels I, as well as to its possibilities to extract taxes from private sector workers.

$$R(\epsilon) - W_P^j \geq (1-\alpha)\tau Ay(I) \tag{2.19}$$

In this sense, it will only be optimal for the incumbent to employ clients *ex-post* if the benefits that they derive from providing public sector jobs to the client ($R(\epsilon) - W_P^j$) are no less than the revenues to be collected if such client was in the private sector $(1-\alpha)\tau Ay(I)$, with τ being the "cost" of tax collection. That is, public employment should be a relatively more effective method of extracting rents to citizens.

Given equation 2.19, which sets the level of public wages and investment at which credible investment offers can be made, the authors are ready to set up an additional set of assumptions:

$$Ay(0) > R(\epsilon) - \psi(\epsilon) \tag{2.20}$$

$$(1-\alpha)Ay(0) > R(\epsilon) - \psi(\epsilon)\tau > [R(\epsilon) - \psi(\epsilon)]/[(1-q_g^P)\psi(\epsilon)] \tag{2.21}$$

These guarantee two things:

1. Equation 2.20 implies that $Ay(I) > R(\epsilon) - \psi(\epsilon)$ for all levels of public investment I, meaning that the socially efficient level of public employment is always zero;

2. Equation 2.21 implies that equation 2.19 is slack; in other words, we assume 2.19 is the binding constraint on clientelism as public investment I goes up.

2.D.3 Patron selection and the model's solution

Now that we know what policies are credible, we return to the selection stage. We assume here that voting behavior is **not** observable, which in turn implies that policy offers cannot be

made contingent on voting decisions. This assumption is made for the purpose of simplifying the model's algebra. Nonetheless, there is a model extension in which voting behavior is observable, which allows electoral results to be contingent to larger collections of voters, with results that are qualitatively very similar to the case with no observable behavior. [105]

We know from equation 2.19 when will a public sector employment offer is credible, for a given level of public investment I. For the group that the patron can make credible job offers, voter *i* will support the patron if it holds that:

$$W_P^1 - \psi(\epsilon) + \delta_i + \theta \geq (1-\alpha)Ay(I) \tag{2.22}$$

Equation 2.22 indicates that voters will support the patron if the combination of the job wages and net utility ($W_P^1 - \psi(\epsilon)$) plus the utility of voting for the patron ($\delta_i + \theta$) is larger than the payoff of the potential patron (in other words, the after tax private sector job income of $(1-\alpha)Ay(I)$).

Solving equation 2.22 for δ_i and integrating over the set of agents in group 1 (for whom this equation is satisfied) yields the probability that the incumbent patron remains in power, which is just the probability that at least half of the population supports the incumbent.

Now that we understand the probability of wining the elections, we know that two regimes are possible depending on the level of public investment. $I(\tilde{A})$ follows from equation 2.19, and it is the minimum value I at which the incumbent benefits from employing clients in the public sector, satisfying that $R(\epsilon) - W_P^j = (1-\alpha)\tau Ay(I)$. From this we can finally conclude that:

- If $I \leq I(\tilde{A})$ we will have a clientelistic regime, the patron can offer credible public-sector

[105] Inefficient underprovision of public investment *I* arises, but rather as a punishment to members of group 1 if they vote against the incumbent. For more details please refer to Robinson and Verdier (2013, p.278-280).

employment to voters of group 1.

- If $I > I(\tilde{A})$ we will have a non-clientelistic regime, as the patron prefers to have all voters work in the private sector.

2.D.4 Main predictions

Prediction 1: In the clientelistic regime, the equilibrium level of public investment I is smaller than the socially efficient level of I^e. Inefficiencies in provision arises through five channels:

1. The existence of an informal sector (inherently less efficient) prevents the patrons from providing the socially efficient I^e and then taxing away their own benefits for self consumption;

2. The incumbent discounts benefits from public investment I, based on the probability that he will be elected;

3. When voters are employed in the public sector only as a mean to influence their political/voting behavior, there will be under-supply of public goods that only increase private sector productivity;

4. Under-provision of public goods arises because it raises the incumbent's political advantage; as the provision of public goods increases, the alternative patron can raise citizens utility;

5. Reducing public goods investment *I* keeps offers of employment credible, because it makes the private sector less productive and comparatively less attractive then. In this sense,

under-investment is also a result of the incumbent's need to increase the probability of re-election.

Prediction 2: In the non-clientelistic regime, the equilibrium level of public investment I_u^* is smaller than the socially efficient provision I^e, but on occasions it can be greater than the level if I under the clientelistic regime, I_c^*.

Prediction 3: if total factor productivity (A) is low, the incumbent inefficiently employs his clients to bias election results, as a public sector job will be more desirable than a private sector one (for the most unproductive subset of the population). However, If A is high enough, it is rational to abandon clientelism, since the opportunity cost of only being able to tax a subset of the population (the group that was not employed) can be very high.

Prediction 4: The patron will reduce investment I supply as the rents from being in power $R(\epsilon)$ increase, and will increase investment I as the population is less subject to ideological bias.

Summing up, this implies that given the state of the world, and a certain probability of a patron wining the elections, we can solve for the optimal choice of investment I, giving rise to two types of regime depending on how high the economy's productivity is:

1. Clientelistic regime: when productivity is low, public sector employment offers are credible;

2. Non-clientelistic regime: when productivity is high the patron prefers clients to work in the private sector, as public sector employment is no longer as attractive an option.

2.D.5 Extension: inequality

This subsection expands the original model to understand the implications of inequality for clientelism. There are now 3 groups of voters, and the incumbent patron (P) can make credible job offers to both groups g_{P1} and g_{P2} at the same efficiency wage W_g^j [106], since he belongs to the same social network. Group 3 is not in the same network as the incumbent, so he cannot make any of its members any credible job offers. Both groups 1 and 2 are size $\lambda/2$, while group 3 is size $1-\lambda$.

Members of g_{P1} and g_{P2} are ideologically uniform ($s_1 = s_2 = s$), and the main difference between them is their income. g_{P1} is richer than g_{P2}, with associated incomes of $\sigma^1 Ay(I) > \sigma^2 Ay(I)$, where $\sigma^1 = 1+x$ and $\sigma^2 = 1-x$ with $0 \leq x \leq 1$. Here x measures income inequality between them.

As before, voter's of g_{P1} and g_{P2} will support the incumbent if the offered job and associated net utility is larger than the pay-off from the potential patron. Equation 2.22 becomes for $g = 1, 2$:

$$W_P - \psi(\epsilon) + \delta_i + \theta \geq \sigma^g \alpha Ay(I) \tag{2.23}$$

Also as before, public-sector employment for each group must be ex post credible. That is, equation 2.19 becomes for $g = 1, 2$:

$$R(\epsilon) - W_P \geq (1-\alpha)\sigma^g \tau Ay(I) \tag{2.24}$$

[106] Voters of g_{P1} and g_{P2} have the same q_g^j, which reflects the probability that their effort level as public sector employees ($e \in \{0, \epsilon\}$) is observed. As a result, each voter will earn a wage of W_g^j, which increases with effort and decreases with the probability of this effort being observed.

And solving equation 2.24 yields the levels of investment over which the incumbent patron can make credible offers to the agents of $g = 1, 2$:

$$y[I_1(x)] = \frac{R(\epsilon) - W_P}{A(1-\alpha)\tau} \frac{1}{1+x} \tag{2.25}$$

$$y[I_2(x)] = \frac{R(\epsilon) - W_P}{A(1-\alpha)\tau} \frac{1}{1-x} \tag{2.26}$$

When income inequality is null (so that $I_1(0) = I_2(0) = I(\tilde{A})$), [107] it will be the case that $I_1(x) < I_2(x)$, because the opportunity cost of offering public sector jobs to the richer and more productive individuals of g_{P1} is higher than to offer it to those in g_{P2}.

As a result, the model predicts that there will be 3 possible regimes, for any given value of I:

1. Complete clientelism: When investment I is so low that $I \leq I_1(x)$, the patron can make credible employment offers to both groups 1 and 2;

2. Incomplete clientelism: when investment I ranges between $I_1(x) < I \leq I_2(x)$, the patron can only make offers to the poorer agents (those of g_2); and

3. Non-clientelistic regime: for $I_2(x) < I$, productivity is high enough that the patron prefers all of his clients to work in the private sector, as public sector employment is no longer as attractive an option.

Prediction 5: An increase in inequality among patron's clients will increase the likeli-

[107] From before we know that $I(\tilde{A})$ is the minimum value I at which the incumbent benefits from employing clients in the public sector

hood of clientelism, which will more likely be geared towards the poorest clients. Furthermore, when productivity is very low (so we already have clientelism), the optimal level of government provided bundle good I decreases with inequality.

Chapter 3: Mining Rents and Public Employment. Evidence from Peruvian Municipalities

3.1 Overview

This essay explores the effect of natural resource revenues on municipal public employment in Peru. I exploit the variation in exogenous mining revenues shocks across municipalities, due to a Mining Canon law reform in year 2004, which sharply increased the amount of mining revenues transferred to mining-producer municipalities.

I use difference in differences (DID) estimations to identify whether public employment size increased due to the larger mining revenues (H1), induced by the legislation reform. I aim to show that part of such additional employment is driven by redistribution concerns and is potentially clientelistic, by confirming that it significantly increase the number of employees under temporary contracts (H3), with low skill levels (H2). I also contribute to a related emerging empirical literature on the link between resource rents and public employment by testing an additional mechanism: whether clientelism is higher in more unequal municipalities (H4).

My results confirm H1, showing a consistent increase in public employment in mining producing municipalities. These new employees are mainly under temporary contracts (H3), and many are in low skilled occupations, such as security guards and cleaning staff (H2). I can-

not, however, confirm my hypothesis on inequality (H4). Higher mining rents also substantially increased the number of directors and officers, which are either elected officers or political appointees. Finally, in a preliminary analysis, I find no evidence of a proportional increase in selected public goods and services that would match the size and composition of the new hires.

This essay builds upon my previous work on oversized public sector employment in rentier states. In my first essay I studied the relationship between natural resource rents and public employment across countries, using a panel dataset with aggregate country-level data. This second essay focuses on explaining the variation in the size of public employment across Peruvian municipalities.

Peru is a promising case to study, for several reasons: (i) clientelism in municipal civil services is allegedly highly prevalent and subject to weak meritocratic checks; (ii) the above-noted exogenous shocks enable reasonably robust identification and (iii) rich and reliable administrative data is available for Peru's municipalities, particularly from the National Registry of Municipalities. I have at hand data on staffing levels by different categories, public income and expenditure, mining production and the corresponding Mining Canon, population, among others.

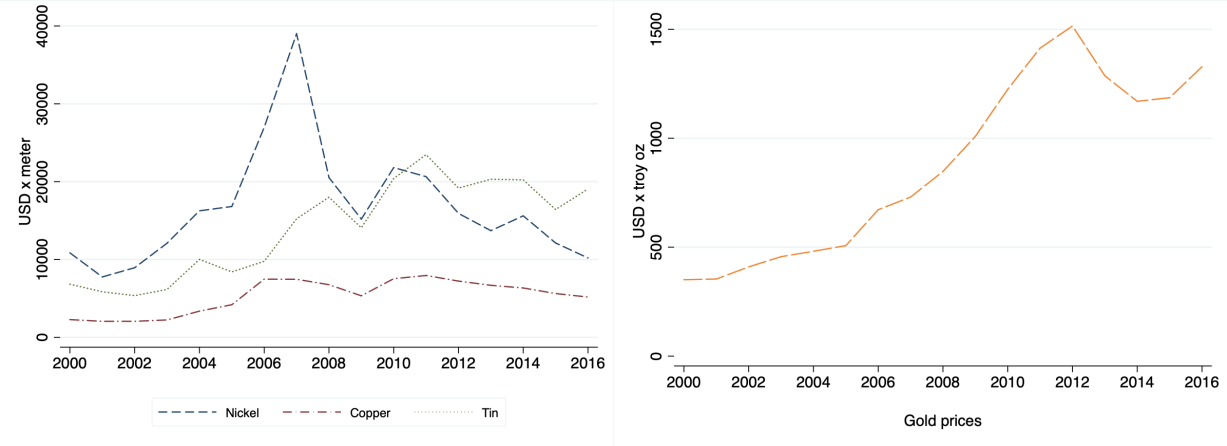
The remainder of the essay proceeds as follows. First, I provide some context on the Mining Canon law reform in year 2004, and on Peru's municipal employment (section 3.2). Second, I briefly refresh the theoretical baseline, to then position it within the literature and explain how it contributes to it (section 3.3). Third, I describe the data (section 3.4.1) and confirm the validity of my proposed identification strategy (section 3.5). Finally, I present the regression analysis (section 4.5), robustness tests (section 3.7) and further discuss the results (sections 3.8 and 3.9).

3.2 Context

3.2.1 Mineral rent shocks: Peru's Mining Canon Law and prices

Peru has a long-standing tradition as a mineral producer, but in recent decades it experienced an unprecedented mining bonanza, due to steep increases in the prices of some of its main ores and metal exports (figure 3.1). Between 1990 and 2018, the share of mineral rents in Peru's GDP grew by factor 9, from 0.8 percent to approximately 8 percent of the GDP (WDI, 2020).

Figure 3.1: Selected commodity markets prices. Years 2000-2016



Source: own elaboration, using World Bank Commodity Markets data (2021).

Since year 1996, Peru's Central government shares with subnational governments (SNGs) part of the centrally collected income tax revenues from mining companies. This scheme, known as the "Mining Canon", initially transferred 20 percent of these revenues to regional (departments) and local governments (municipalities). Starting year 2003, this share increased to 50 percent.

The Mining Canon redistribution rules changed several times subsequently[1]. However, August 2004 Law No. 28322 (the one to be analyzed in this paper) fundamentally altered the distribution of mining revenues, to further increase mining producing municipalities' revenue shares. Unlike before, this law granted producers a part of the Canon *at each distribution stage*. Moving forward, 50 percent of the income tax revenues from mining companies were to be redistributed to municipal and department governments, as per the following rule:

1. 10 percent went straight to *producing municipalities* (those with at least 1 tax paying mining facility in the period under consideration);

2. 25 percent was distributed among *all* municipalities in producing *provinces*;

3. 40 percent corresponded to *all* municipalities in producing *departments*; and

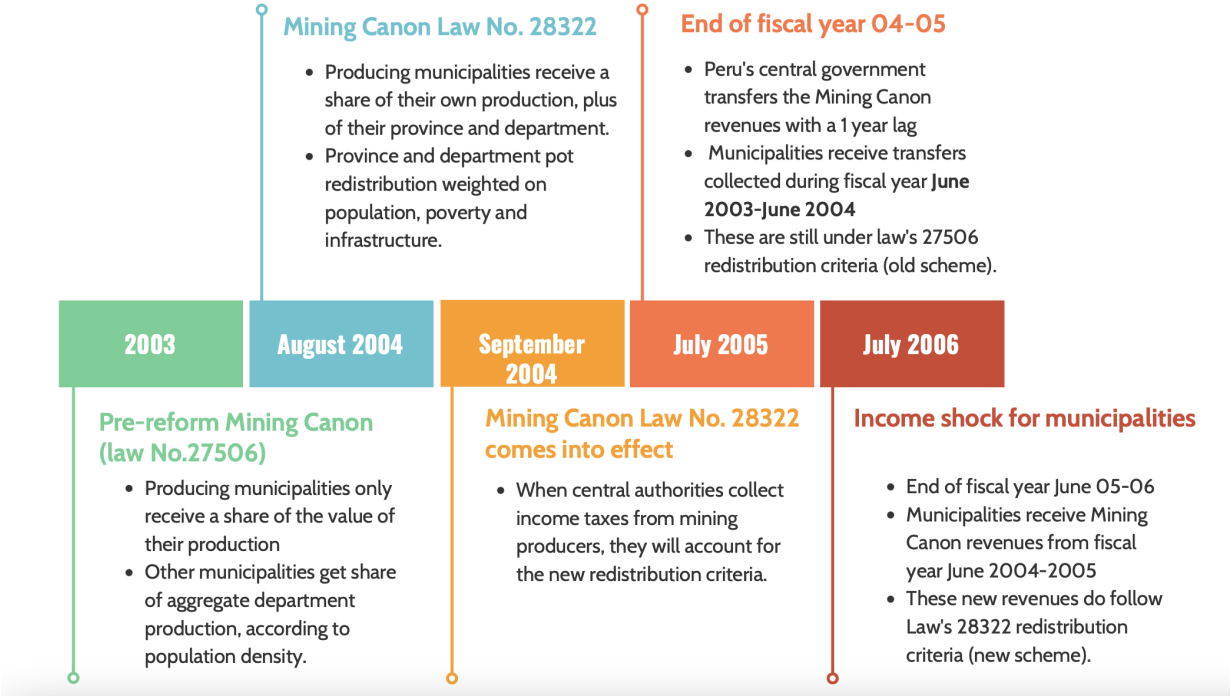
4. 25 percent was transferred to the producing department governments.

Apart from the 10 percent transferred exclusively to producing municipalities, the allocation of the remaining canon across all municipalities (points 2 and 3 above, for both producing and non-producing municipalities) depends on "distribution indexes" created by the Ministry of Economics and Finance (MEF). Such indexes weight criteria such as population projections, poverty indicators (based on the unsatisfied basic needs methodology) and infrastructure deficits (MEF, 2020a). Moreover, population density was no longer part of the redistribution criteria, as it disfavored sparsely populated areas, which were often the poorest. Appendix figure 3.39 summarizes the differences between the pre and post mining revenues redistribution rules.

[1]The legislation governing this distribution changed frequently, starting with 2001 Law No. 2756, followed by 2003 Law No. 28077 (which varied the percentages to distribute the canon between different levels of government) and 2004's Law No. 28322.

Importantly, there is a time-lag between the collection of income tax revenues from mining companies and their transfers to the respective SNGs: The Mining Canon collected in a given year is distributed one year later. In Peru, the fiscal year ends in June, so the Mining Canon collected by June of any year is received by the municipalities on July of the year after. Thus, after August 2004 Law No. 28322, mining producing municipalities only started to benefit disproportionally from the legal reform in *July of 2006*. Figure 3.2 summarizes the reform process.

Figure 3.2: Mining Canon Law reform timeline



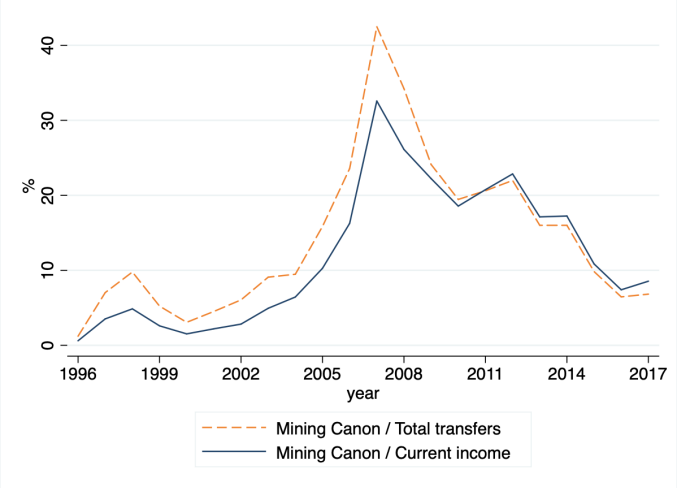
Source: own elaboration, based on Law No. 28322 (2004) and MEF (2020a).

SNGs are not obliged to spend Mining Canon transfers (MCTs hereto) in the fiscal year in which they are received. Because the Mining Canon is a "defined resource" by Peru's law, if these funds are not fully spent in a given fiscal year, they may be reused by the municipality

on the next one[2] (Loayza et al., 2014). Nonetheless, these **transfers are earmarked for capital expenditures, and may *not* be used for current expenditure such as wages** and social contributions.

The Mining Canon constitutes an important source of income for SNGs. Between 1996 and 2017, aggregate MCTs increased from 0.6 to 8.6 percent of total SNG revenues (figure 3.3), with a sharp peak at 32.5 percent in 2007. Likewise, as a share of total central government transfers to SNGs, MCTs grew from 1 to 10 percent over the same period. After 2012, the Mining Canon became less important relative to other transfers, mainly because other transfers grew rapidly and in part because total mining revenues declined (figure 3.4).

Figure 3.3: Mining Canon transfers to municipalities, over total transfers and current income. Peru. 1996-2017

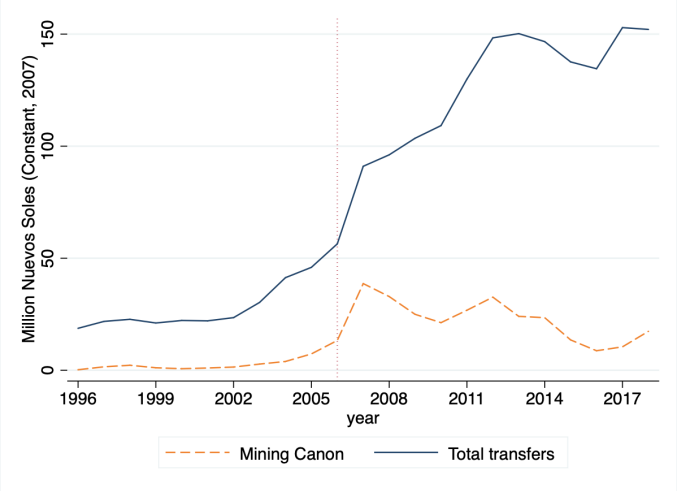


Source: own calculations, using MEF (2020c).
Note: Current income is the regular source of revenue, composed of taxes and mandatory contributions, social contributions, sales of good and services and administrative rights and property rents.

The Mining Canon Law clearly privileged producing municipalities in terms of the growth

[2]Ordinary resources, by contrast, must be spent within the same fiscal year as allocated, or otherwise they revert back to the central government.

Figure 3.4: Mining Canon and total transfers to municipality governments. Peru, 1996-2018



Source: own calculations, using MEF (2020c).

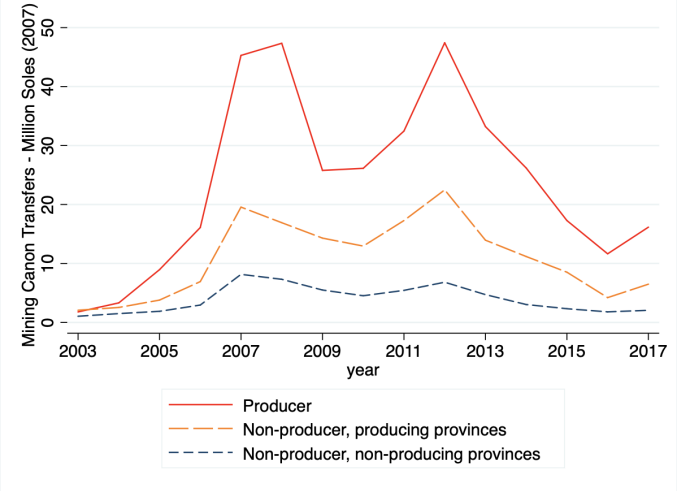
of *total* magnitude of transfers (figure 3.5), but slightly less so with regards to total income. The relative contribution of the Canon in the budget is similar within *producing provinces* (non producing provinces are far below), and only slightly higher for producing municipalities (figure 3.6). This is likely due to the mechanisms enforced by the new law, with distribution indexes that overly compensate the poorer municipalities within producing provinces and departments.

3.2.2 Municipal employment

Municipalities, also known as districts, are the second lowest tier of government in the Republic of Peru[3], a unitary and decentralized state. Municipalities are governed by municipal councils, which are composed by a Mayor and Council members, both publicly elected every 4 years. Among their many attributes, these councils can autonomously decide how many municipal civil servants to recruit and whom, according to their budgets and needs.

[3]Small population centers are the smallest politico-administrative subdivision in Peru. Nonetheless, I do not consider them in this paper, mainly because they do not receive MCTs. Please refer to appendix 3.A for details.

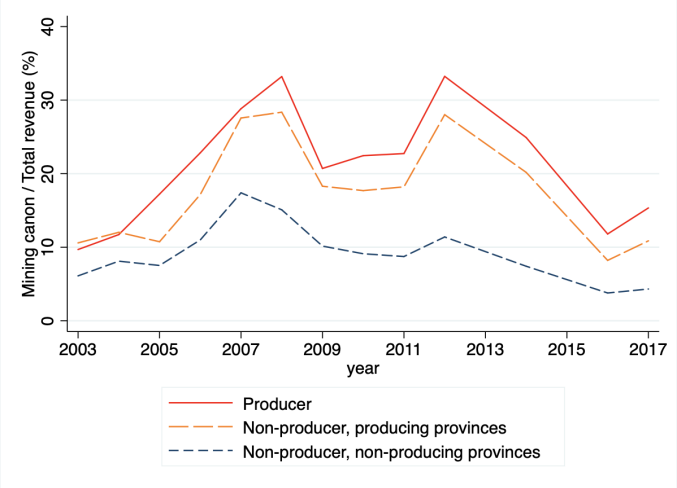
Figure 3.5: Mining Canon transfers to local governments, by municipalities' contemporaneous production status. Peru, 2003-2017



Source: own calculations, using MEF (2020c) and and MINEM (2020).

Note: Because of the one-year lag between Mining Canon generation and redistribution, in this graph municipalities or provinces are producers in any given year if they produced the year before.

Figure 3.6: Local governments Mining Canon transfers as a share of total revenue, by municipalities' contemporaneous production status. Peru, 2003-2017

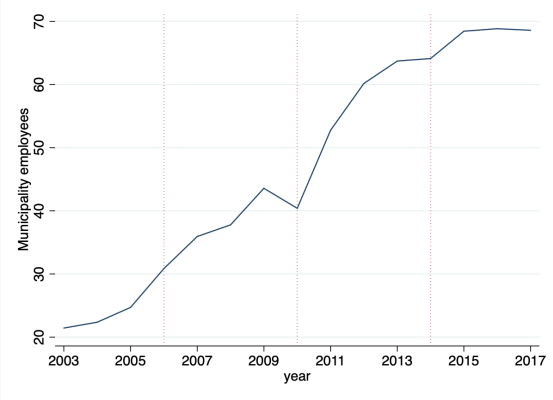


Source: own calculations, using MINEM (2020) and RENAMU (2020).

Note: Because of the one-year lag between Mining Canon generation and redistribution, in this graph municipalities or provinces are producers in any given year if they produced the year before.

Municipal public employment has been steadily growing since at least since 2003 (before it, there is no publicly available information on local public employment). This is true in terms of the number of employees (figure 3.7) and the wage bill (figure 3.8), both of which grew by 95 and 87 percent between 2007 and 2017, respectively. Nevertheless, municipal public employees have consistently represented a modest share of total public employment in Peru. The National Authority for the Civil Service (SERVIR) estimates that between 2007 and 2014 municipal public employment represented about 14 percent of total public employment (figure 3.9).

Figure 3.7: Municipalities employees. Peru, 2003-2017

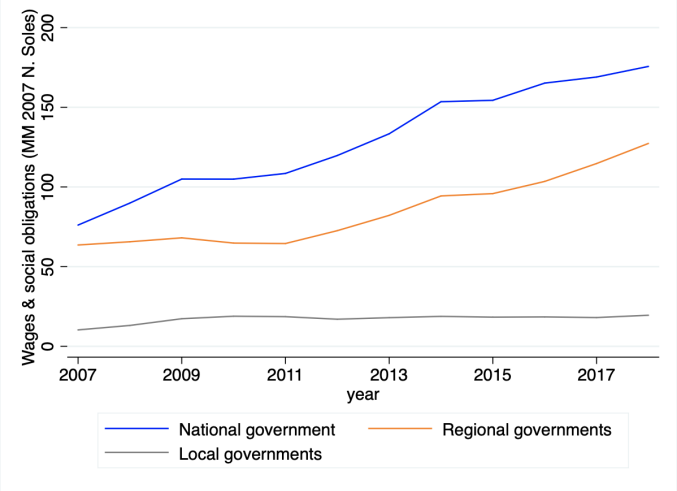


Source: own elaboration, using RENAMU (2020).
Note: 2006, 2010 and 2014 were electoral years.

Municipal workers can be classified according to their occupation and their contractual arrangements. Notably, there is no single match between workers occupation and their contracts, so employees with the same occupations can be hired under different employment regimes (see figure 3.10). The many coexisting labor regimes lead to well-known inequities in terms of the employees' duties and their benefits (Iacoviello, 2015).

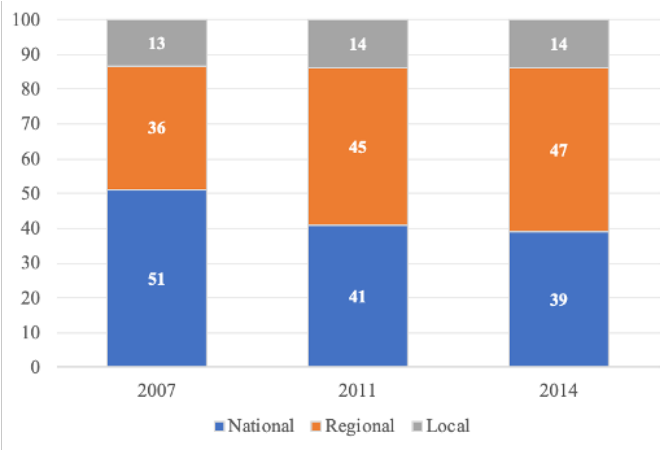
With regards to their contractual arrangements, employees can be either i) appointed or

Figure 3.8: Wages and social obligations expenditure by level of government. Peru, 2007-2017



Source: own calculations, using MEF (2020b).

Figure 3.9: Public sector employment composition by level of government. Peru. 2007-2014

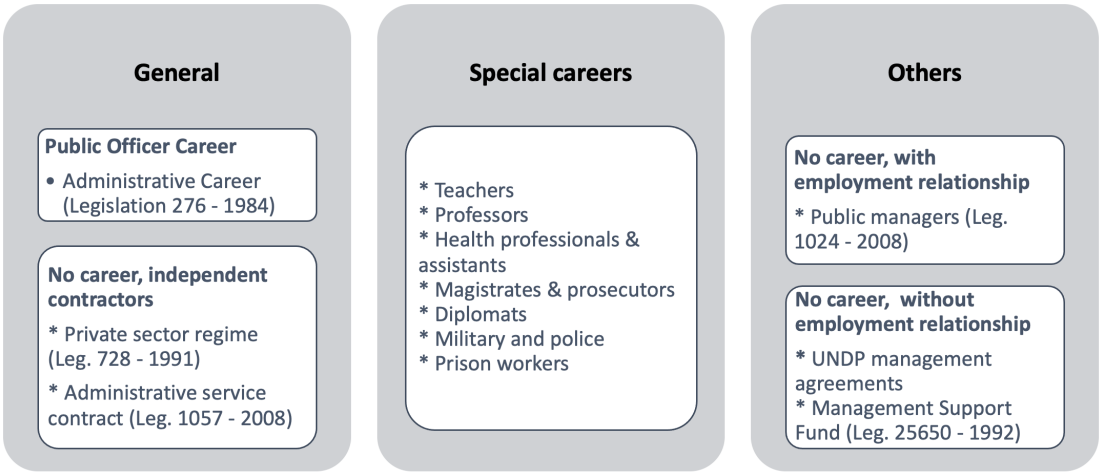


Source: own calculations, using SERVIR (2009, 2012a & 2020).

permanent contract holders, ii) have temporary contracts[4] or iii) be hired via a third party (contractor). Core civil servants and political appointees (category i above) are under the more gen-

[4]Initially, these workers were under 1991's Law No. 728, hired through contracts for non-personal services (CSNP). Starting year 2009, these contractual scheme switched to the administrative service contracts (CAS, originating from 2008's Law No. 1057), the State's new special contractual arrangement for temporary workers. These newer CAS offered better benefits than those previously available under the CSNP.

Figure 3.10: Peru's public administration labor regimes



Source: own translation, based on SERVIR (2012b).

erous public administration labor laws (1984's Law No. 276), while employees with temporary contracts or hired via contractors (categories ii and iii) are the facto under private sector laws.

In terms of occupation, workers can be i) directors and officials[5], ii) professionals, iii) technicians and auxiliary employees, or iv) janitors, security guards, and other blue-collar workers. These classifications reflect workers education, training and recognized experience. Importantly, teachers, health professionals and law enforcement agents ("special regime workers" [6], see figure 3.10) are *not* municipal employees, being under the regional and national governments. (SERVIR, 2012b)

In year 2004, when the new Mining Canon law was approved, Peru's civil service was among the weakest in Latin America and the Caribbean. As per the Inter-American Development Bank (IDB), Peru's civil service development index placed it in the 4th lowest position in the

[5]These positions are either for elected officials or of free appointment and removal.

[6]This civil servant category corresponds to a given set of professions, with particular contractual arrangements for each sub-group.

region. *The civil service was vastly politicized*, with high discretion in HR decisions and hardly any merit considerations in hiring. (Iacoviello and Strazza, 2014) This was enhanced by the fact that Peru's National Authority for the Civil Service (SEVIR) was only created in later 2008; before it, there was no single entity in charge of Peru's civil service (SERVIR, 2012b). In this context, it is not to be surprised if clientelistic public employment is widespread.

Peru's civil service begun an important modernization process by year 2011. The young SEVIR worked in a series of rulings to i) establish a unique legal regime for all people who work in the public sector; ii) improve hiring and evaluation processes, as to promote meritocracy; iii) clarify the rules in terms of monetary and non-monetary employee compensations. As a result, a new Civil Service Law was approved during year 2013, which later lead to the Rule for the Special Regime of Local Governments. While these reforms were no panacea, independent evaluators conclude that 2011 was an inflection point towards the creation of a more professional and meritocratic civil service. (Iacoviello, 2015).

3.2.3 Mining revenue and public employment trends

The basic theoretical prediction of interest for this paper is that higher natural resource rents are associated with larger public employment, and even more so with higher inequality. A first glance at juxtaposed average Mining Canon and public employment trends in Peru suggest that they fit this pattern.

In year 2004, when the legal reform under study was approved, Peru was a lower-middle income country with highly segmented labor markets and ethnically driven income inequality[7].

[7]For instance, as per 2003's National Household Survey, indigenous workers labor incomes were on average only 56 percent of that for non-indigenous workers.(Barron, 2008)

At the aggregate national level, there is a subtle but clear parallel trend in public sector employment and mineral exports income (figure 3.11). Between 2003 and 2012 there is a mild increase in public employment, possibly linked to the also growing income derived from mineral exports; likewise, after the mining exports begin decreasing in year 2012, so does public employment.

Figure 3.11: Total Public employment and mineral exports evolution. Peru, 2003-2017

Source: own calculations, using WDI (2020), ILO (2020) and Banco Central de Reserva del Peru (2020).
Note: Current income is the regular source of revenue, composed of taxes and mandatory contributions, social contributions, sales of good and services and administrative rights and property rents.

At the municipal level, the trends also coincide with theoretical predictions. In the face of a plausibly exogenous increase in mining rents [8], there was considerably higher public employment in mining producing municipalities, with peaks in the year *after* elections (see figure 3.13, in section 3.5.2). Peruvian municipal elections are typically held in November, while RENAMU's reported public employment numbers reflect December figures. Consequently, an increase in

[8]The increase in resources rents comes from two likely exogenous sources: i) higher commodities prices, determined in world markets, and ii) a legal reform that increases the share of mining fiscal resources that the central government transfer to local ones. Importantly, this means that municipalities have little say on the resource rents they will receive each period. Municipalities are small units which can hardly set production prices, and cannot decide on produced quantities either, since mining concessions are granted to private actors either by the central government (for small and large mining) or by the regional governments (for small and artisanal mining).

public employment figures the year *after* elections, not the year off, suggests that public employment could have been used to reward voters' political support.

3.3 Theory and literature

3.3.1 Theory: Clientelistic Public Employment in Rentier States

This paper builds upon my previous work on oversized public sector employment in rentier states. Robinson and Verdier (2013) analytical framework helped me study politicians' dilemma of how to spend increasing natural resources rents: should they be distributed as patronage in the form of public employment to a narrow clientele, or spent on providing broad-based public goods? Consistent with theory, my first paper (a cross-country analysis) finds that more natural resources rents will encourage politicians to increase public employment, provided that: i) the country is a democracy, with ii) low to medium productivity, and iii) moderately high inequality.

The intuition behind this theory is that more public employment can be convenient for politicians, because it increases their chances of staying in power. When productivity is low, incumbent politicians employ their clients in exchange for votes, since public jobs are more desirable than private ones for the low income/skills voters. This transfer of labor to potentially unproductive jobs in the public sector further reduces the efficiency of the economy, which in turn further increases public employment attractiveness. In addition, higher inequality raises the share of potential low-income voters who could profit more from a public job. In these conditions, clientelism is more likely to arise, and the model predicts that as the rents from being in power increase, so will the use of public employment for inefficient redistribution.

Within this framework, the objective of this paper is to test the following hypotheses:

- H1: Municipalities will increase their public employment as the mining revenues grow;
- Higher mining revenues favor the hiring of a distinctive type of employee, with:
  - H2: Lower skill level occupations (blue collar workers), and
  - H3: Shorter contracts (temporary employees).
- H4: Municipalities with high income inequality will increase their public employment, as mining revenues grow, more than those with low levels of inequality.

Hypotheses H1, H2 and H4 directly follow from my theory. After the mining bonanza, one expects that municipalities receiving larger transfers will differentially increase their number of public employees (H1), particularly so in terms of low skilled workers (H2). Higher inequality should further intensify this phenomenon (H4). As to their contractual arrangements, it is reasonable to assume these workers should be under temporary contracts (H3), since this allows the incumbent politicians to hire and fire them at will, to reward their political loyalty (or otherwise punish dissent).

Much like temporary employees, workers with appointed contracts can be easily hired and fired, as they are essentially political appointees. These workers, in theory, are meant to be highly skilled, and hence are not the target population for redistributive employment (what my model is about). However, it is not inconsistent with my model's logic to also find a differential increase in political appointees. Some of these positions can also be (and often are) granted to reward support, or simply to ones friends. [9]

[9]The possibility to quickly hire and fire is less likely for the 2 remaining contract types. Permanent workers are core civil service employees, which must go through much longer official employment processes (*concursos* in Spanish) and have lifelong tenure at the State bureaucracy. Contractors are private firms employees hired for specific purposes; the contractual arrangements would go through the workers firm, following private sector law.

3.3.2 Positioning in the Literature

My approach is nested in an emerging set of quantitative studies on the resource curse, which exploit within-country variation in natural resource revenues across SNGs to identify their impact on a range of social outcomes, including public employment. Within the literature, my paper relates most closely to Monteiro and Ferraz (2012) and Larrain and Perello (2019). In Brazil, Monteiro and Ferraz (2012) observe that off-shore oil discoveries are associated with a large increase in the number of public employees in those municipalities that benefit. They however find no impacts on household infrastructure, education and health supply. This, together with the post-oil boom re-election advantage for politicians, suggests that voters reward (clientelist) increases in public sector employment without associated improvements in service delivery.

More recently, Larrain and Perello (2019) study the effect of an increase in the portion of mining patents assigned to mining producing municipalities in Chile. Their findings provide analogous evidence of clientelist employment: The authors encounter that the extra mining revenues increased the municipal *wage bill* [10], but had no effect on other municipal outcomes such as transfers to health and community programs and municipal investment.

While there is obvious proximity, my research will add to the aforementioned papers in a few regards. First, my detailed administrative data permits to explore the changes in the types of employees hired, in terms of contractual arrangements/length and their occupation[11]. Both Larrain and Perello (2019) and Monteiro and Ferraz (2012) discuss changes in the *wages* of workers with distinct contract types, but disregard their occupation. I believe it relevant to also

[10] Their data only includes wage bills, not the number of employees.

[11] I will not discuss wage bills because this variable has several missing observations in the years before the reform was implemented.

consider changes in the number of workers by occupation, since this potentially sheds light on the type of client that politicians cater to when offering public jobs. As per theory, politicians with more natural resources will offer "clientelistic" public jobs predominantly to their lower income/skill customers, when in a low productivity and high inequality setup. However, whether this relationship holds is an empirical question that has not been properly addressed in the related literature.

Second, I observe how inequality mediates the effects of natural resource rents on public employment. I measure the heterogeneous effects of resource rents on public sector employment, in municipalities with distinct levels of consumption inequality (as per the gini index). Inequality is an entirely absent feature in Larrain and Perello (2019), and it is treated as an outcome (not an explanatory variable) in Monteiro and Ferraz (2012). Inequality is traditionally considered a pervasive feature of Peruvian society [12], and it is often more present for the historically disenfranchised indigenous population[13].

There is empirical value in testing Robinson and Verdier (2013)'s predicted relationship between inequality and clientelism within the context of natural resource wealth (H4), as there is so far no other paper that does it (that I know about). According to my theory, high inequality will increase clientelism, especially among politician's poorest clients. Moreover, when patrons and clients belong to the same social network, this may increase patrons' ability to observe or infer voting behavior, and thus punish non-compliers by removing them from the job.[14]

Peru's municipal civil service provides a suitable context for testing clientelistic-driven

[12]For summaries in the subject, see Castillo (2020), Yamada and Castro (2007) and Yamada et al. (2016).

[13]Ethnicity in Peru is a strong predictor of poverty and inequality: In 2003, early in the Mining Canon decentralization process, the mean income of indigenous workers was only 56 percent of that of non-indigenous workers (Barron, 2008)

[14]This type of social network effect has also been emphasized in Fearon and Laitin (1996), Finan and Schechter (2012), La Ferrara (2003), Stokes (2005), among others.

public employment increases, since institutional checks that could contain politically motivated hiring remain weak there. While the Peruvian civil service has undergone a successful reform that considerably improved its performance[15], line-managers still have high discretion in HR decisions. There are hardly any merit considerations in hiring; moreover, difficulties to attract personnel prevail (Iacoviello and Strazza, 2014). This context stands in contrast to the ones of Brazil and Chile, studied by Monteiro and Ferraz (2012) and Larrain and Perello (2019). Brazil and Chile have highly professionalized civil services and they are consistently the top performers in Latin America.

Within the broader natural resources windfalls literature, two noticeable papers study the mining boom's effect on socioeconomic outcomes in Peru. However, none of these studies considers the issue of public employment. Aragon and Rud (2013) measured the local economic impact of Yanacocha, the world's second largest gold mine, finding positive effects of the mine's demand for local inputs on real income between 1997 and 2006. More generally, Loayza and Rigolini (2016) found a mixed impact of mining on socioeconomic outcomes at local municipalities, since it has a positive income effect but a negative distributional effect.

On a similar vein, Caselli and Michaels (2013) evaluated the impact of oil windfalls in living standards in Brazil municipalities. They found that oil-rich municipalities expanded reported spending on public goods and services, but with no associated increase in social outcomes. Their evidence suggests that funds might have been allocated to a combination of self-enrichment and vote buying. Also in Brazil, in the interception between fiscal decentralization and natural resources, Litschig and Morrison (2013) find that revenues derived from natural resources do not contribute to human capital accumulation and poverty alleviation, unlike other central govern-

[15]Peru's civil service development index, gathered by the IDB, went from 14 in 2004 to 29 in 2012.

ment transfers. Furthermore, Brollo et al. (2013) see that incumbent majors are more likely to be reelected in Brazil after large federal transfers, despite higher corruption; this was in part because of the diminished quality of the challenging political candidates.

In summary, this paper will add to this literature by testing the effect of mineral rents on public sector employment, with particular consideration on whether: i) higher inequality enhances this relationship; and ii) there is a differential increase in the number of workers in lower skills occupations. This research project can also contribute to the fiscal decentralization literature, adding to our knowledge of how the type of additional funding received by local governments affect their performance.

3.4 Data

3.4.1 Data sources

This research builds on a compilation of several rich municipal level datasets. The data comprises information on staffing levels, public income and expenditure, mining production and the corresponding Mining Canon, population, and elections results, among others. Table 3.1 presents the main datasets and sources used.

Table 3.1: Main datasets, at the municipality level

Variable	Data source	Years	Note
Public Employment	RENAMU	2003-2018	Municipality census
Local public services	RENAMU	2003-2018	Diverse outcomes
Income and expenditure	RENAMU	2003-2018	Provided by MEF
Mining production	MINEM	2001-2018	Administrative data
Commodity prices	World Bank	2000-2020	
Mining Canon transfers	MEF	1995-2019	Administrative data
Gini coefficient	INEI	2007	
Population	INEI	2005-2015	Estimates

Municipal public employment data is publicly available in Peru, via the annual National

Registry of Municipalities (*Registro Nacional de Municipalidades*, RENAMU). This census of municipalities is run by the National Institute of Statistics (INEI), and covers the period 2003-2018[16]. Content wise, RENAMU provides information on the total number of workers per municipality, per contractual arrangement and occupation. As a downside, RENAMU's numbers are possibly under-reported, as they do not include data on workers under management agreements with the United Nations Development Program (UNDP), or the management support funds[17]. However, and unlike other similar datasets, RENAMU does include workers who are hired through contractors.

RENAMU also provides highly detailed information on **local public services and infrastructure provided by the municipality**. I will focus on a few outcomes only, to assess i) electricity, water, sanitation and roads infrastructure, ii) garbage collection and public hygiene services. Appendix 3.C provides details for those variables that are not self-explanatory.

Finally, RENAMU also discloses **municipalities income and expenditures**, following standard public budgeting practices and definitions. This data, although found in RENAMU, was provided by Peru's Ministry of Economics and Finance (MEF).

Mining Canon transfers (MCTs) received by municipalities come from MEF's economic transparency initiative. MEF maintains a website which includes all transfers made by the National Government to all other levels of government (national, regional and local). This information is available since year 1995, so we can keep track of MCTs since their inception. Importantly, the Canon generated in a given year will be distributed 12 months after it was produced, not during that same fiscal year. Consequently, the Mining Canon registered in a given year corresponds

[16]Before year 2003, there is no publicly available information on local public employment.
[17]There is no publicly available data on the number of workers under these arrangements.

to production levels of previous years.

Municipalities **mining production** information comes from plant-level mining data, from Peru's Ministry of Energy and Mining (MINEM). In particular, I keep track on the production of the following minerals: gold, silver, copper, zinc, lead, tin, iron and molybdenum; these constitute over 98 percent of all Peru's mining production. In order to obtain the value of production, I simply multiplied the produced amount by annual average of the international price of the commodity, using World Bank's Commodity Markets data (2021).

The INEI provides **Gini coefficients** at the municipal level, available from year 2007 onwards. INEI also compiles yearly **municipal population**, for year 1993 as well as for all years between 2005 and 2017. The INEI does not provide population data for years 2003 and 2004, so I computed them using the population average yearly growth rate for the 2005-15 decade.

3.4.2 Sample and exclusions

I include in my analysis all Peruvian departments, both mining producers and non-producers. Mining producing departments are located at the Andean and Coastal regions[18], while non-producers are predominantly in the Jungle one[19] (see appendix figure 3.40).

I will exclude from my sample those municipalities that in any given year: i) are in the upper 1 percent of total public sector employment and/or MCTs; ii) do not have data for any dependent or independent variable; or iii) are in the province (not the department) of Lima, home to the politically influential and densely populated Peruvian capital (see table 3.2). Consequently,

[18]Mining producer departments are Ancash, Apurimac, Arequipa, Ayacucho, Cajamarca, Cusco, Huancavelica, Huanuco, Ica, Junin, La Libertad, Lima, Moquegua, Pasco, Puno and Tacna. Madre De Dios is the only mining producing Department located in the Amazon basin.

[19]Non-producer departments are Amazonas, Lambayeque, Loreto, Piura, San Martin, Tumbes and Ucayali. These are in the Jungle ("Selva") region.

from the existing 1833 municipalities, after exclusions I am left with 1579, municipalities, over 195 provinces and 24 departments. Of these municipalities, in year 2004: i) 62 are mining producers, ii) 462 are non-producers in producing provinces; iii) 877 are non-producers in producing provinces (NP.NPP); and iv) 283 are in non-producing departments.

Table 3.2: Sample exclusion criteria, by municipality mining production status

	Municipality type		
	Non-producer	**Producer**	**Total**
In sample	1,517	62	1,579
(%)	*96*	*4*	*100*
Missing values	143	10	153
(%)	*94*	*6*	*100*
Outliers	54	4	58
(%)	*93*	*7*	*100*
Province of Lima	42	1	43
(%)	*98*	*2*	*100*
Total	1,756	77	1,833
(%)	*96*	*4*	*100*

There are relevant baseline differences between producing and non-producing municipalities. First and foremost, producing municipalities have on average more public employees at baseline (table 3.3). This difference is largely driven by the big gap in the number of appointed employees (political appointees and the likes), as well as by the number of mid and low skilled workers (technicians, auxiliaries, and blue collar workers)[20]. Next section will provide more details on public employment by production status.

In terms of more general characteristics (table 3.4), when compared to other municipalities in producing departments, mining producers are on average less poor (smaller population percentage with 1+ NBI), have larger populations, are more likely to be provincial capital districts, and have larger territories. In relation to municipalities in non-producing departments, producers

[20]By contrast, the number of high skilled employees (directors, officials and professionals) is roughly equal in producing and non-producing municipalities.

are considerably more unequal and have smaller territories. These attributes will be controlled for in the regression analysis, either through municipal or province fixed effects.

Table 3.3: Summary statistics, public employees by contract type and occupation, at baseline. Peru, 2003.

Variable		Mean	Std. Dev.	Min	Max
		Producers			
	Public employees (total)	30.98	50.88	2	264
Contract type	Appointed employees	14.92	42.29	0	254
	Contractors	0.1	0.43	0	2
	Permanent contract	8.48	17.18	0	78
	Temporary contract	7.48	9.99	0	52
Occupation	Directors & officials	2.68	5.23	0	24
	Professionals	3.06	4.3	0	18
	Technicians & auxiliary	14.85	22.84	0	105
	Blue collar workers	10.39	23.64	0	139
		Non-Producers, producing provinces			
	Public employees (total)	16.69	29.69	1	255
Contract type	Appointed employees	4.52	12.4	0	118
	Contractors	0.12	1.39	0	27
	Permanent contract	5.64	15.47	0	137
	Temporary contract	6.41	9.9	0	92
Occupation	Directors & officials	1.49	4.33	0	55
	Professionals	2.63	6.87	0	121
	Technicians & auxiliary	7.22	13.86	0	133
	Blue collar workers	5.36	13.3	0	150
		Non-Producers, non-producing provinces			
	Public employees (total)	20.26	33.1	1	283
Contract type	Appointed employees	6.10	16.1	0	168
	Contractors	0.08	0.45	0	4
	Permanent contract	6.03	13.71	0	140
	Temporary contract	8.06	13.29	0	195
Occupation	Directors & officials	1.25	2.85	0	44
	Professionals	3.09	4.31	0	41
	Technicians & auxiliary employees	9.22	15.98	0	182
	Blue collar workers	6.45	15.39	0	161
		Non-producing departments			
	Public employees (total)	27.82	34.43	1	210
Contract type	Appointed employees	8.70	16.47	0	152
	Contractors	0.11	0.71	0	9
	Permanent contract	10	18.03	0	130
	Temporary contract	9.02	13.69	0	111
Occupation	Directors & officials	2.81	4.91	0	26
	Professionals	2.54	3.12	0	24
	Technicians & auxiliary employees	14.45	21.02	0	135
	Blue collar workers	8.02	13.95	0	99

Note: The number of yearly observations per groups is: Producers = 62; Non-Producers in producing provinces = 396; Non-Producers in non-producing provinces = 838; and Municipalities in non-producer departments=283.

Table 3.4: Summary statistics, control variables at baseline. Peru, 2003.

Variable	Mean	Std. Dev.	Min	Max
	Producers			
% Pop. with 1+ NBI (1993)	78.85	19.49	7.9	100
Population	11,194	15,534	836	86,074
Provincial capital (%)	14	37	0	1
Altitude (m)	2,858	1,402	36	4705
Area (Km^2)	1,090	2,858	6.7	22,219
	Non-Producers, producing provinces			
% Pop. with 1+ NBI (1993)	82.08	18.88	10	100
Population	6,494	12,982	232	118,644
Provincial capital (%)	8	27	0	1
Altitude (m)	2,717	1,104	14	4,679
Area (Km^2)	370	746	2.2	10,955
	Non-Producers, non-producing provinces (in producing departments)			
% Pop. with 1+ NBI (1993)	83.46	16.67	17.4	100
Population	8,583	14,902	183	213,657
Provincial capital (%)	11	31	0	1
Altitude	2,696	1,152	9	4,470
Area (Km^2)	421	1,032	2	19,136
	Non-Producing departments			
% Pop. with 1+ NBI (1993)	80.5	16.21	32.7	100
Population	10,296	12,147	254	76,610
Provincial capital (%)	9	29	0	1
Altitude	684	848	5	3,151
Area (Km^2)	1,693	3,580	3,09	24,050

Note: The number of yearly observations per groups is: Producers = 62; Non-Producers in producing provinces = 396 ; Non-Producers in non-producing provinces = 838; and Municipalities in non-producer departments=283.

3.5 Identification strategies

I use the DID methodology to estimate the impact of the mining revenue increases in the size and composition of municipal employment. I will compare the differences in the outcomes of interest between the treatment and control municipalities, both before and after the mining revenues shock. The before period includes years between 2003 and 2005, since the jump in resource allocation became effective only after July of 2006, and the post period includes years 2006 until 2011.[21]

For robustness purposes, I will: i) add controls, province and year trends; ii) measure public employment both as a total and in per capita terms; iii) reestimate results excluding from the sample the Coast and Andes geographic regions and those municipalities that changed production status in the years under study [22]; iv) change the year defining the production status to 2003 and 2005; and v) successively reduce the post-treatment end period from 2011 to 2008, to ensure that my results do not capture other (omitted) income shocks during the post period. Furthermore, I will repeat all estimations under a secondary DID specification, based on the estimated magnitude of the Mining Canon transfers (MCTs).

3.5.1 Exogeneity of resource shocks?

Since the early 2000s, Peru's municipalities experienced a sharp increase in mining rents, which came from two likely exogenous sources: i) higher commodities prices, determined in world markets, and ii) a legal reform that increases the share of mining fiscal resources that the

[21] While available, I will not use data for years 2012 forward, as to avoid possible confounding effects from as major civil service reform that started then.

[22] Be it producers that became non-producers, or non-producers that became producers at some point. The 41 municipalities constitute 2.6% of the sample for any given year.

central government transfers to local ones. Both of these shocks come into play in this paper.

In terms of commodity prices, while country level economics and politics can influence mining world markets (Peru is an important mining producer), at lower administrative levels mining production is mostly geology driven. Moreover, mining concessions are granted to private actors either by the central government (for large and medium mining) or by the regional governments (for small and even artisanal mining), not by municipalities. Thus, it is unlikely that any individual municipality can purposely influence international commodity prices.

The legal reform under study is also arguably exogenous for municipalities. To start with, it occurred within a broad national decentralization process, which made emphasis in political and economic deconcentration in favor of department and (especially) municipal governments. The Mining Canon is only one of the 6 natural resources revenues redistribution schemes, or "Canons"[23], that were further decentralized in this national reform process. These new Canon laws follow similar general principles and redistribution formulas, regardless of the nature of the resource under consideration.

Mining reforms are only relevant for a small group of municipalities (only 3,5 percent in year 2004), which were not properly organized so as to lobby for their own interests when the Mining Canon law was being elaborated (or during its subsequent revisions either). There is only one major civil association which gathers municipalities and lobbies for their interests: The Association of Municipalities of Peru (AMPE). While AMPE surely had an important role in the early 2000's legal reforms that encouraged fiscal revenues decentralization, it is unlikely that they would seek to benefit mining municipalities over others, particularly considering that the Mining Canon is just one of the many central transfers that were negotiated in the broad decentralization

[23]Fisheries, Hidroelectric, Forestry, Gas and Oil Canons.

process.

Finally, most municipalities are small units and do not have enough influence to individually affect parliamentary decisions. The one exception is the municipalities in the province of Lima, in the capital region, which is why I exclude them from the sample.

3.5.2 DID strategy I: mining production status

In my first strategy (DID I) I define treatment groups by the municipalities' administrative status of "mining producer" versus "non-producing". The 2004 legal reform used this status to determine the percentage of MCTs that each municipality would receive after July of 2006. Accordingly, in DID1 municipalities fall into 4 groups:

1. Producing municipalities (P), which have at least one mining facility with (tax paying) mineral production *in year 2004*;

2. Non-producing municipalities in producing provinces (NP.PP);

3. Non-producing municipalities in non-producing provinces (NP.NPP); and

4. Control group: municipalities in non producing departments.

The intensity of treatment varies between the 3 treatment groups, since the 2004 legal reform on average benefits P more than NP.PP, and NP.PP more than NP.NPP. On the opposite, municipalities in the control group receive no MCTs, as there is no mineral production in such departments.[24]

[24] I follow this strategy after Loayza and Rigolini (2016), who also consider municipalities' production status as a treatment for evaluating the effects of mining revenue shocks on poverty. In their research, however, NP.NPP was the control group, and municipalities in non-producing departments were all together excluded.

I chose year 2004 to define a municipality's mining production status, since the last substantial legal reform to the Mining Canon was introduced in August of that year. Because of the lag between the law enactment and the Mining Canon production and distribution, the main jump in resource allocation started only *after July* of 2006 (Section 3.2.1 figure 3.2 summarizes this process). Results are virtually the same, however, if the production status corresponds to years 2003 or 2005, as well as if we exclude from the sample those municipalities that changed their production status at any point in time.

Equation 3.1 presents my DID regression, where outcome Y_{it} is affected by: i) whether municipality *i* is a P, a NP.PP or a NP.NPP; ii) a Post dummy variable taking the value of 1 for year 2006 onwards, and iii) municipality fixed effects ($Municipality_i$) and clustered standard errors (ϵ_{it}). For robustness purposes, I also include year effects, to control for common shocks that could affect all municipalities in certain years. Alternatively, I present results without municipal fixed effects, adding instead a set of time invariant characteristics (X_i) [25], as well as province fixed effects.

$$\begin{aligned} Y_{it} = \alpha_0 + \alpha_1 P_i + \alpha_2 NP.PP_i + \alpha_3 NP.NPP_i + \alpha_4 Post \\ + (\beta_1 P_i + \beta_2 NP.PP_i + \beta_3 NP.NPP_i) * Post + \alpha_5 Municipality_i + \epsilon_{it} \end{aligned} \tag{3.1}$$

The main outcome variable is the number of municipality employees (H1), but I also explore the number of employees by occupational group (H2) [26] and contract type (H3) [27].

The identification assumption in this paper is that in the absence of the Mining Canon law reform and the commodities prices shock, public sector employment trends would remain similar

[25] Municipality's budget at baseline (2003), population percentage with at least one Unsatisfied Basic Need (abbreviated as *NBI*) in year 1993, consumption inequality (Gini index) in year 2007, surface area, average altitude, and a dummy indicating whether the district is a province capital.

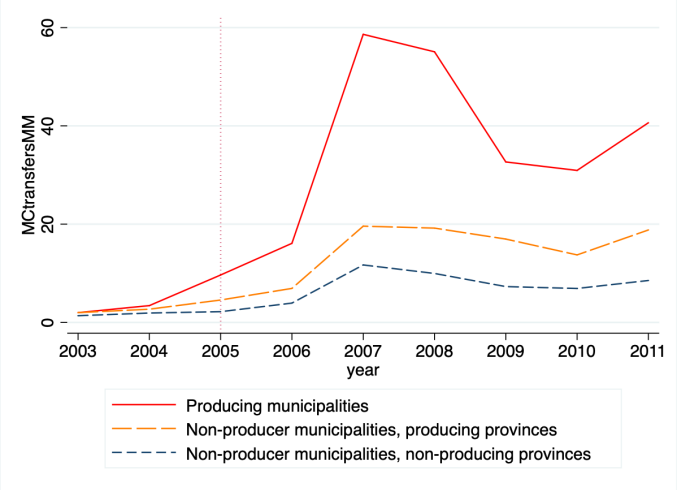
[26] i) Directors and managers, ii) professionals, iii) technicians & auxiliary employees, and iv) blue collar workers.

[27] i) Appointed, ii) permanent contract, iii) hired through a third party (contractor), or iv) temporary contract.

for the treatment and control groups. Consequently, for this empirical strategy to work, two premises must hold. First, the Mining Canon legal reform must effectively have increased total municipal revenues for the treatment groups (A1). Second, pre reform public employment levels must be parallel for the treatment and control groups (A2).

With regards to *A1*, simple visuals (figure 3.12) confirm that after year 2006 producing municipalities on average receive considerably larger transfers than non-producing ones, while NP.PP receives larger transfers than NP.NPP. These increases holds both in absolute terms and when observing MTCs as a share of revenues (appendix figure 3.41)[28].

Figure 3.12: Mining Canon transfers to local governments, by municipalities' mining production status. Peru, 1996-2017



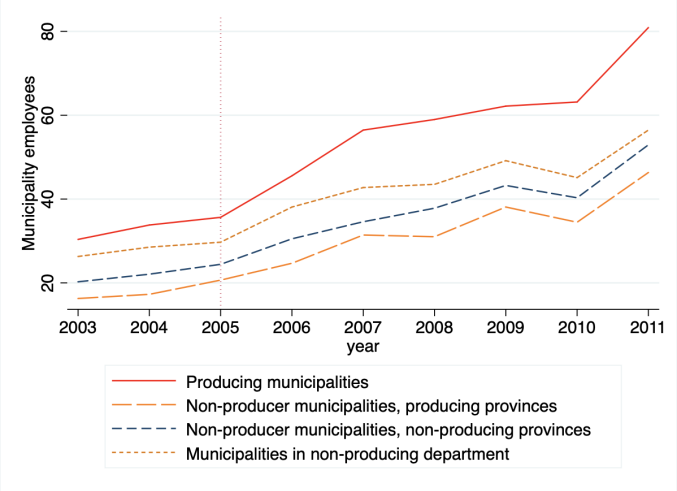
Source: own calculations, using MEF (2020c) and MINEM (2020).
Note: Municipalities and provinces are producers if they had 1+ tax paying mining facility in year 2004.

In terms of *A2*, a graphic exploration of the public employment trends (figure 3.13) shows what seems to be parallel trends in years 2003-2005, followed by a clear jump in public employment in producing municipalities in year 2006. On the contrary, non-producing municipalities

[28] Appendix table 3.10 formally shows that the differences are, indeed, greater than zero after year 2006.

seem to continue following the previous trend. These alleged increases can be confirmed more formally in appendix table 3.10, which shows a statistically significant positive difference in total public employment between the treatments and control group post reform.

Figure 3.13: Municipalities employees, by municipalities' mining production status. Peru, 2003-2011



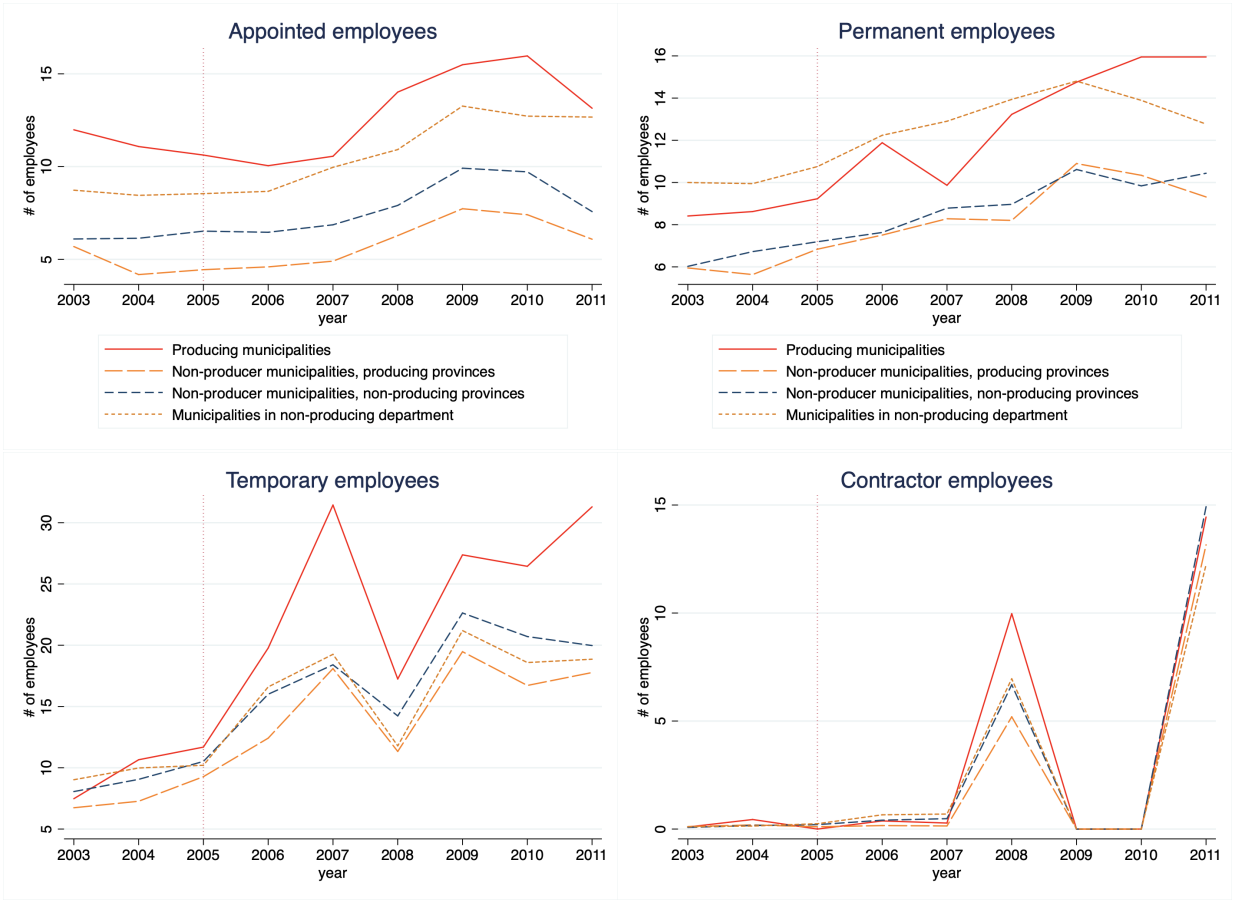
Source: own elaboration, using RENAMU (2020).
Note: Production status corresponds to year 2004. Election years are 2006 and 2010.

Because of the short time frame pre-reform, this graphic exploration might not suffice to test the parallel trends assumption. Hence, I confirm it trough a different method. I run a regression for the outcome of interest (log of public employment) on a constant, the treatment dummies, year dummies, and their interactions. I then apply an F test where the null hypothesis (H_0) is that joint interaction terms *pre-reform* (until year 2005) are equal to zero. I do not find enough evidence to reject H_0 before the shock (years 2003-2005), since $F(9, 190) = 0.67$ with a P-value=0.6776.

When decomposing public employment growth by employees contract type and occupation, interesting patterns emerge. In terms of contract modalities (figure 3.14), producing munic-

ipalities seem to differentially increase the number of temporary contracts post reform, compared to the control group. This is also the case for contractor employees, although the increase quickly reverses after year 2008. An increase in temporary employees supports my theory, since they are easier to both hire and fire than permanent employees. Hence, public employment fluctuations that are predominantly based on this contract type, could support clientelistic and/or redistributive politics.

Figure 3.14: Employees by contract type and municipality's mining production status. Peru, 2003-2010

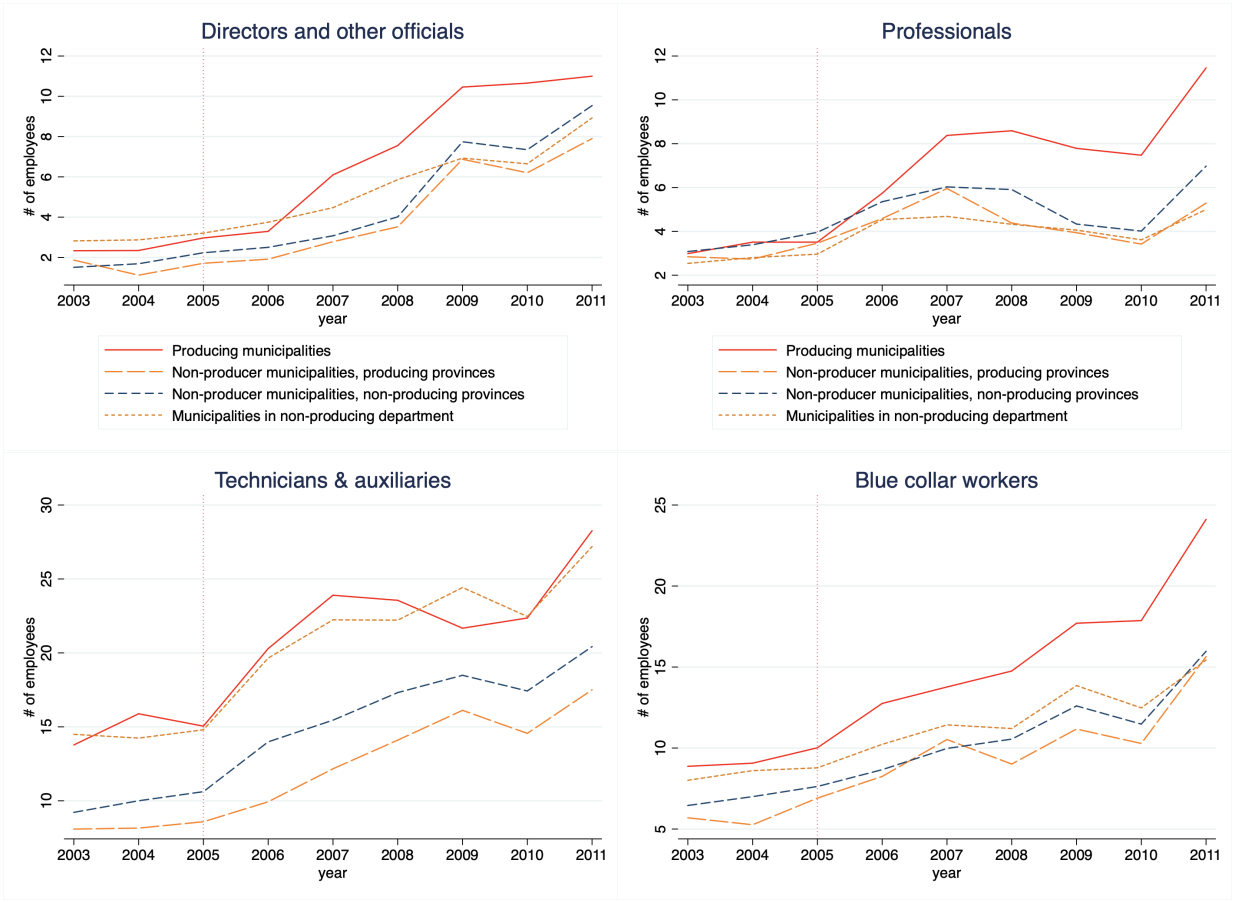


Source: own elaboration, using RENAMU (2020).
Note: 2006 and 2010 were electoral years. Production status corresponds to year 2004

With regard to the workers occupations (figure 3.15), after the reform producing municipal-

ities have a differential increase in all types of workers, except for mid skilled ones (technicians and auxiliaries). As per my theory, the biggest increase should have been for blue collar workers and possibly for directors and officials.

Figure 3.15: Employees by occupation and municipality's mining production status. Peru, 2003-2010



Source: own elaboration, using RENAMU (2020).
Note: 2006 and 2010 were electoral years. Production status corresponds to year 2004

3.5.2.1 Triple DID, to test H4

In its current form, DID I (equation 3.1) only permits to tests hypotheses 1 through 3, but not H4 (on inequality). To be able to test H4, I will use a triple DID, which includes the

interactions between the municipalities mining production status and consumption inequality quintiles ($Q = 2, 3, 4$ and 5), as per formula 3.2. The parameters of interest (δ_{1Q}, δ_{2Q} and δ_{3Q}) are those of the triple interactions for $ProductionStatus_i * Inequality_{Qi} * Post_t$.[29].

$$Y_{it} = \alpha_0 + \delta_{1Q} P_i * Inequality_{Qi} * Post_t + \delta_{2Q} NP.PP_i * Inequality_{Qi} * Post_t + \delta_{3Q} NP.NPP_i * Inequality_{Qi} * Post_t + (\alpha_{1Q} P_i + \alpha_{2Q} NP.PP_i + \alpha_{3Q} NP.NPP_i + \alpha_{4Q} Post_t) * Inequality_{Qi} + \beta_1 P_i + \beta_2 NP.PP_i + \beta_3 NP.NPP_i + \beta_4 Post_t + \beta_5 Municipality_i + \epsilon_{it} \quad (3.2)$$

Like before, the DID regression is one where outcome Y_{it} is affected by: i) municipality *i* mining production status in year 2004, ii) a Post dummy variable taking the value of 1 from year 2006 onwards, and iii) municipality fixed effects ($Municipality_i$) and clustered standard errors (ϵ_{it}). In addition, equation 3.2 includes i) the triple interaction $ProductionStatus_i * Inequality_{Qi} * Post_t$ (the parameters of interest); as well as the interaction between ii) mining production status and the post dummy ($\alpha_{1Q} - \alpha_{3Q}$); and iii) inequality quintiles and the post dummy (α_{4Q}). The outcome variable is the number of municipality employees.

As a word of caution, municipal level inequality data is only available from year 2007 onwards. This somewhat challenges this identification strategy, since the Mining Canon legal reform became the facto effective after July of 2006. Nonetheless, although it is likely that the new law affected municipal income inequality, it is unlikely that consumption inequality *significantly* shifted in only a year. For this reason, it is relatively safe to still pursue this test, although under the assumption that **the testing of H4 is exploratory**, more of suggestion than an actual proof.

[29] As before, a municipality's production status will be either producers (P), non-producers in producing provinces (NP.PP) and non-producers in non-producing provinces (NP.NPP). Municipalities in non-producing departments are the control group.

3.5.3 DID strategy II: predicted mining canon transfer quartiles

My second DID specification (DID II), used for robustness purposes, measures how the *magnitude* of transfers affects public employment size, regardless of municipalities' mining production status. Here, I define the treatment in terms of the predicted size of MCTs received by municipalities in year 2004 (pre-reform), based on the post reform mining revenue redistribution formula. This strategy accounts for the legally induced changes in the size of transfers, based on the value of mining production pre-shock.

Pre reform MCTs, by themselves, are not a suitable treatment. This is because the 2004 Mining Canon law fundamentally affected how the MCTs are redistributed. Post reform MCTs do not work either: some municipalities could self-select into it, by producing more minerals. Consequently, to capture the potential magnitude of transfers induced by the legal change, I predict the MCTs that municipalities could have received in year 2004 under the new mining revenue redistribution criteria (equation 3.3, to be explained below). This accounts for the potential magnitude of rents post shock, based on the value of mineral production pre shock.

DID II strategy adds to DID I in two ways. First, it does not assume that mining producers are always top MCTs receivers; in fact, while on average they are top receivers, often times they were not both pre and post reform.[30] Second, it accounts for the value of production pre reform. This is important because the MCTs are proportional to the size of the mining production; hence, a non-producing municipality in a high production province will likely receive higher transfers than a producing municipality with small production.

To define my treatment, I predict the MCTs that municipalities could have received in year

[30] For instance, before the Mining Canon law reform, 24 percent of producing municipalities received MCTs below the median, while 52 percent of non producers were above the median of MCTs. See appendix figure 3.42.

2004, if the new revenue redistribution formula (equation 3.3) had been in place then. According to it, municipality *i* receives mining revenues based on the annual value of its own mining production ($ProductionValue_{ipd}$), as well as that of the province *p* ($ProductionValue_{pd}$) and department *d* ($ProductionValue_{d}$) to which it belongs. The production values will be weighted by the corresponding redistribution shares mandated by the law [31] and by province and department population shares (Pop_{ipd}/Pop_{pd} and Pop_{ipd}/Pop_{d}, respectively).

$$\begin{aligned} MCT_{ipd} = 0.10 * ProductionValue_{ipd} + 0.25 * ProductionValue_{pd} \\ * (Pop_{ipd}/Pop_{pd}) + 0.40 * ProductionValue_{d} * (Pop_{ipd}/Pop_{d}) \end{aligned} \tag{3.3}$$

This formula reflects with high accuracy the post reform revenue redistribution (see appendix figure 3.43). A simple regression indicates that, after year 2007, a 1 percent increase in the predicted MCTs will yield a 0.87 increase in the actual MCTs in the year after. The relationship is not 100 percent accurate because the Mining Canon law also indicates that municipal poverty levels and infrastructure deficits should be considered at the time of redistribution. However, I cannot factor NBI based poverty nor infrastructure deficits in my formula, as there is no publicly available information on how to weigh it in the early years of the reform. However, I will control for baseline NBI in my DID regression.

Under this identification strategy, the treatment groups are defined based on whether in year 2004 municipality *i* is in the 3 highest quartiles of MCTs (quartile 1 is the control):

$$\begin{aligned} Y_{it} = \alpha_0 + \alpha_1 MCTQ4_i + \alpha_2 MCTQ3_i + \alpha_3 MCTQ2_i + \alpha_4 Post_t \\ + (\beta_1 MCTQ4_i + \beta_2 MCTQ3_i + \beta_3 MCTQ2_i) * Post_t + \alpha_5 Municipality_i + \epsilon_{it} \end{aligned} \tag{3.4}$$

Like before, the DID regression is one where outcome Y_{it} is affected by: i) municipality *i* predicted MCT quartiles in year 2004, ii) a Post dummy variable taking the value of 1 from

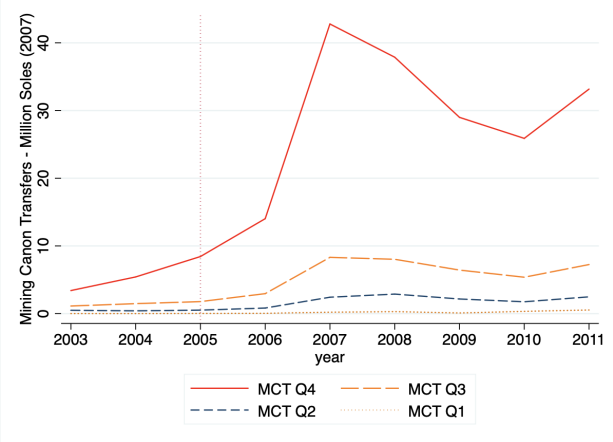
[31] 10, 25 and 40 % of the annual value of production of each municipality, province and department, respectively

year 2006 onwards, and iii) municipality fixed effects ($Municipality_i$) and clustered standard errors (ϵ_{it}). The main outcome variable is the number of municipality employees, as a total and by occupational group and contract type. MCTs quartiles correspond to the absolute transfer amounts (in 2007 thousand Soles).

For this empirical strategy to work, two assumptions need to hold. First, the Mining Canon legal reform must effectively have increased total mining revenues for the treatment groups (A1). Second, the parallel trends assumption must hold for pre reform public employment levels (A2).

With regards to *A1*, simple visuals confirm the evident increase in MCTs from 2006 onwards, both in nominal terms (figure 3.16) and as well as a share of the municipality's total revenues (appendix figure 3.44). This holds for each transfer quartile but quartile 1, the control (see appendix table 3.11). Hence, the first assumption is satisfied.

Figure 3.16: Mining Canon transfers to local governments, by predicted transfers quartiles. Peru, 2003-2011

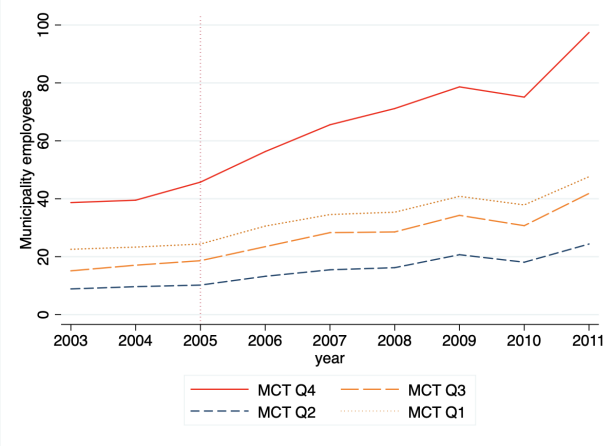


Source: own calculations, using MEF (2020c) and MINEM (2020).
Note: Transfer quartiles correspond to year 2004.

In terms of *A2*, pre-reform public employment levels are parallel for the treatments and

control group[32]. And starting year 2006 (post shock), as expected, there is a sharp increase in public employment for the group with the highest Canon transfer quartiles, while the other groups display more modest changes (figure 3.17).

Figure 3.17: Municipalities employees, by predicted Mining Canon transfers quartiles. Peru, 2003-2011



Source: own calculations, using RENAMU (2020).
Note: predicted Mining Canon rransfer quartiles correspond to year 2004.

3.6 Results

The following section presents my results, in the order of the hypotheses under study. My main results are DID I based, where the treatment is defined in terms of the municipalities mining production status. I then follow with the main robustness check, where I further explore the magnitude of the Mining Canon Transfers (DID II, section 3.7.1). Next, I go over an additional series of robustness tests (section 3.7.2). Finally, section 3.8 preliminarily examines whether the

[32] As before, I test this by running a regression for log of public employment on a constant, the treatment dummies, year dummies, and their interactions. I then apply an F test where the null hypothesis (H_0) is that joint interaction terms *pre-reform* (until year 2005) are equal to zero. I do not find enough evidence to reject H_0 before the shock (years 2003-2005), since $F(9, 190) = 0.67$ with a P-value=0.6776.

increase in the number of municipality workers can be associated with the provision of more good and services. Section 3.9 summarizes my findings and discusses possible implications.

3.6.1 Municipalities will increase their public employment as their mining revenues grow (H1).

As per my theory, higher mining rents will encourage politicians to increase public employment. Furthermore, the increase in public employment should be proportional to the size of the mining rents, in the following order: $Producers > NP.PP > NP.NPP > Control$. This section supports this hypothesis.

All the municipalities in my sample, which were affected to different extents by the Mining Canon reform, have considerably higher public employment post-reform. After year 2006, public employment was on average 42 percent higher across *all* municipalities ("Post reform" coefficient, table 3.5, column 1). This trend holds regardless of the specification, in every single regression performed in this paper[33], and is consistent with the mining revenues bonanza experienced by a big share of these municipalities, as well as with the ongoing political decentralization process.

As per my first results (table 3.5), **all treated groups have higher public employment post shock** than municipalities in non-producing departments. Producers had on average 15 percent higher post shock public employment, while NP.PP and NP.NPP had more modest increases of 8 and 9 percent, respectively. These results are roughly equal when measuring public employment as a share of the population (table 3.5, columns 5 to 8).

[33] In the future, I will not report nor discuss this coefficient, and will only focus on whether there is a differential public employment increase in mining rents rich municipalities.

Table 3.5: Municipal public employment variations, by mining producer status. Peru, 2003-2011

Ln public employment	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Total employment				Per capita employment		
Producers		-0.073	0.015	0.106		-0.059	-0.089	-0.073
		(0.15)	(0.09)	(0.21)		(0.06)	(0.06)	(0.19)
NP.PP		-0.447***	0.075	0.142		0.181***	-0.018	-0.012
		(0.08)	(0.06)	(0.21)		(0.04)	(0.04)	(0.19)
NP.NPP		-0.177**	0.073	0.132		-0.053	-0.027	
		(0.07)	(0.05)	(0.20)		(0.03)	(0.04)	(0.19)
Post reform	0.424***	0.449***	0.462***	0.540***	0.295***	0.359***	0.436***	0.713***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Producer * Post	0.144***	0.172***	0.165***	0.144***	0.136***	0.147***	0.147***	0.131***
	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)
NP.PP * Post	0.084***	0.103***	0.095***	0.072***	0.112***	0.123***	0.122***	0.105***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
NP.NPP * Post	0.091***	0.125***	0.115***	0.092***	0.090***	0.100***	0.096***	0.079***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Municip. effects	Yes	No	No	No	Yes	No	No	No
Year effects	Yes	No	No	Yes	Yes	No	No	Yes
Province effects	No	No	No	Yes	No	No	No	Yes
Controls	No	No	Yes	Yes	No	No	Yes	Yes
R^2	0.467	0.084	0.661	0.733	0.444	0.139	0.417	0.542

Note: N=14,200. All regressions have municipality clustered standard errors (in parentheses). Columns (3), (4), (7) and (8) control for total revenue (2003), population share with 1+ NBI (1993), population (2003), Gini coefficient (2007), municipality area (Km^2), latitude and longitude. Production status corresponds to year 2004. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

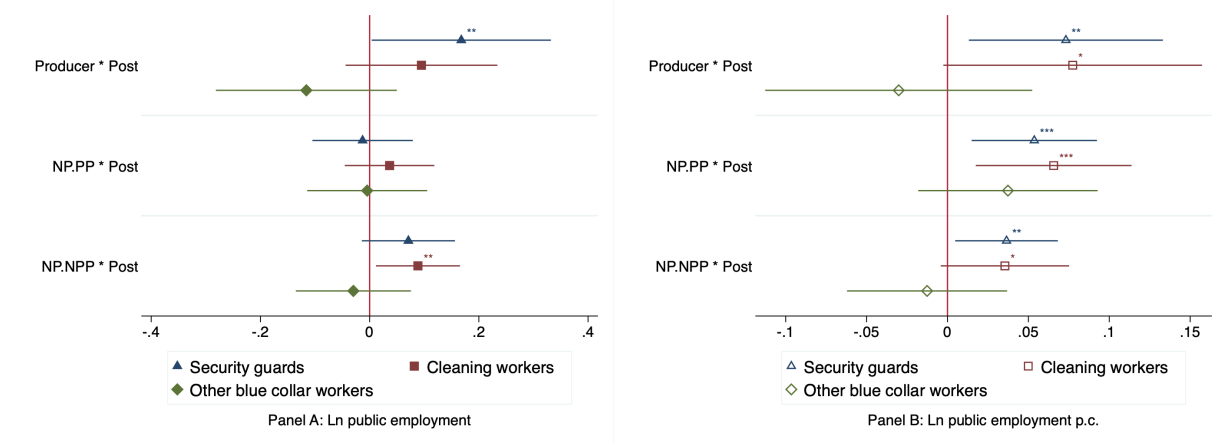
3.6.2 Larger mining rents increase the number of low skilled workers (H2), mainly under temporary contracts (H3).

My model predicts that when productivity is relatively low, public jobs are more desirable than private ones *for the low income/skills voters*; hence, incumbent politicians are likely to employ those particular clients in exchange for votes. Consequently, after the mining bonanza one would expect to see a differential increase in the **number of blue collar workers**. These new employees should be **under temporary contracts**, since this allows the incumbent politicians to hire and fire them at will, to reward their political loyalty (or otherwise punish dissent).

With regards to skill levels, figure 3.18 (panel A) confirms the expected **increase in the number of blue collar workers**, which seems to be **largely driven by more security guards**

hirings. Post shock, producing municipalities hired 17 percent more security guards than the control. Likewise, NP.NPP increased their cleaning staff by 9 percent. Other blue collar workers do not significantly increase post shock, vis-a-vis the control group.

Figure 3.18: Blue collar employees variations, by mining producer status. Peru, 2003-2011



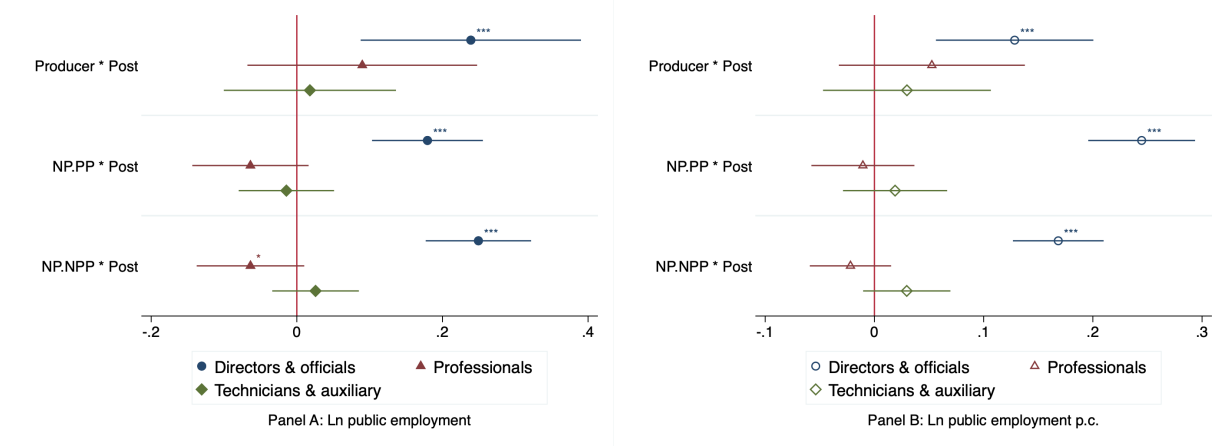
Note: N=14,200. All regressions have municipality and year dummies, as well as municipality clustered standard errors (horizontal lines). Production status corresponds to year 2004. Appendix table 3.12 presents regression coefficients. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In terms of other occupations (figure 3.19, panel A), **directors and officials consistently increased across all treatment groups** by roughly 23 percent[34]. This is not a direct prediction of my model, but it is not against it either (see section 3.3.1): these positions are usually for political appointees, and thus of free remotion.

With regards to the contractual arrangements, figure 3.20 corroborates that **the majority of the new workers are under short contracts** (H3). Post shock, producing municipalities increase their temporary contract hirings by 25 percent, vis-a-vis the control group. No other treatment group showed an increase in the number of temporary contracts. However, NP.NPP did hire 6 percent more appointed employees than the control group. As previously discussed

[34]The increase was 24, 18 and 26 percent for producers, NP.PP and NP.NPP, respectively.

Figure 3.19: Public employment composition, by occupation. DID regression by mining producer status. Peru, 2003-2011

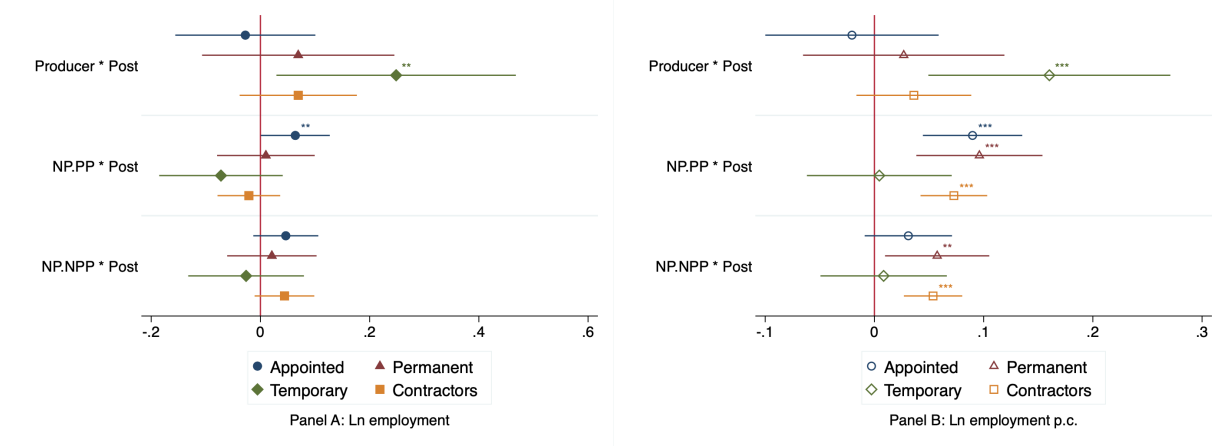


Note: N=14,200. All regressions have municipality and year dummies, as well as municipality clustered standard errors (horizontal lines). Production status corresponds to year 2004. Full results in appendix table 3.13. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

(see section 3.3.1), appointed contracts are granted to political appointees (mainly directors and officers); while these are more generous than temporary contracts, they are also inherently less stable than permanent ones. Hence, an **increase in appointed contracts also supports H3**.

In short, this section confirms hypothesis 2 and 3: larger mining rents substantially increase the number of temporary contract workers, many of which are low skilled (security guards and cleaning staff). In addition, I find that larger mining rents also bring about more directors and officers, a group of (in theory) high skill workers who come either as elected officers or as political appointees. This, although not directly predicted by my theory, is in line with clientelistic public employment logic. All findings in this section also hold for public employment per capita (panel B in figures 3.18, 3.19 and 3.20) and when using an alternative regression without municipality fixed effects and adding instead baseline controls and province dummies (see appendix figures 3.45, 3.46 and 3.47).

Figure 3.20: Public employment composition, by contract type. DID regression by mining producer status. Peru, 2003-2011



Note: N=14,200. All regressions have municipality and year dummies, as well as municipality clustered standard errors (horizontal lines). Production status corresponds to year 2004. Full results in appendix table 3.14. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

These findings provide some food for thought: higher mining rents systematically bring about workers that are the very ends of the skills distribution, and no mid skill workers nor professionals. Is it possible that municipalities receiving high MCTs, which were better off before the legal reform, had skill gaps *only* at the tails of the skills distribution? Seems rather unlikely.

3.6.3 Municipalities with high consumption inequality *do not* increase their public employment as mining revenues grow (H4 does not hold).

According to my theory, higher inequality increases pressures for redistribution, particularly so when in presence of "unconstrained" resources (such as those arising from mining rents). Consequently, in very unequal setups higher mineral rents should lead to even larger public employment than in most equal places. These new jobs will presumably be distributed among the politician's poorest clients. I test this hypothesis through a triple interaction ($ProductionStatus*$

$Post * Inequality\ quintile$), as per formula 3.2; I measure consumption inequality with the gini index [35].

I find **no evidence that municipalities with higher inequality further increase public employment as their mining income grows**. As per figure 3.21, none of the triple interactions are positive (which would have supported H4); most tend to be statistically non-significant, after robustness test. The only robust result is that, post reform, non-producing municipalities in producing provinces that are also in the third inequality quintile will have 20 percent *less* public employment than more unequal municipalities in non-producing departments.[36]

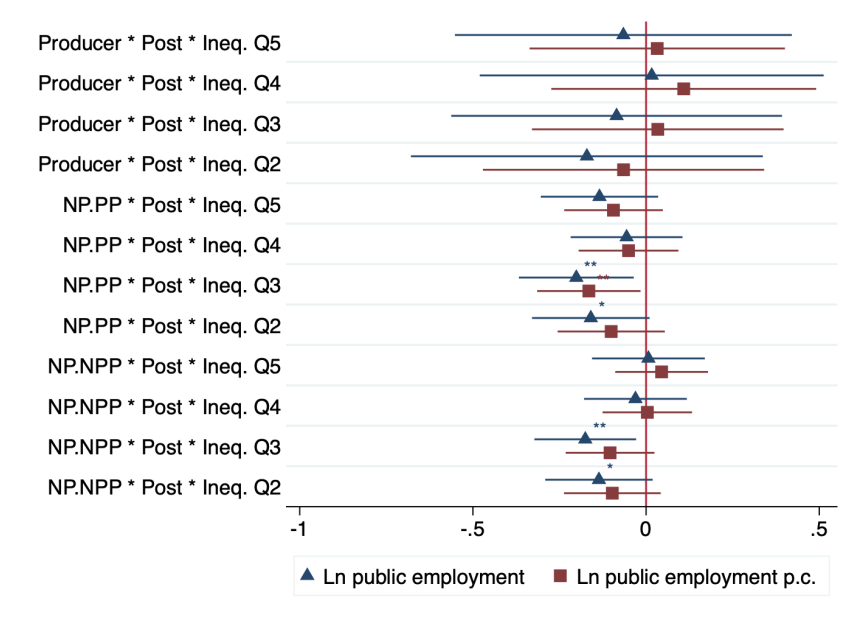
Importantly, these findings do not mean that public employment is smaller in unequal municipalities. In fact, it is quite the opposite. Figure 3.22 presents my model's predicted municipal public employment, by mining producer status and inequality quintiles. As per this figure, **municipal public employment is *always* higher in the most unequal municipalities**, regardless of the mining production status. However, in opposition to my predictions, **having larger mining rents does not exacerbate this trend**.

Post legal reform, all municipalities in consumption inequality quintiles 4 and 5 have considerably larger public employment than their peers in more equal equal municipalities. However, and against expectations, the largest increase in public employment happened in unequal municipalities in non-producing departments (the control group), followed by those in non-producing municipalities in non-producing provinces (which receive the smallest mining rents of the 3 treated groups).

[35] As discussed in section 3.5.2.1, inequality data is only available starting year 2007. It is likely that the legal change affected municipal income inequality; however, it is unlikely that consumption inequality significantly shifted in only a year. For this reason, it is relatively safe to still pursue this test, although under the assumption that the testing of H4 is exploratory, more than an actual proof.

[36] While the data suggest similar patterns for NP.PP in inequality quintile 2, and for NP.NPP in quintiles 2 and 3, these results are not robust to considering public employment in per capita terms.

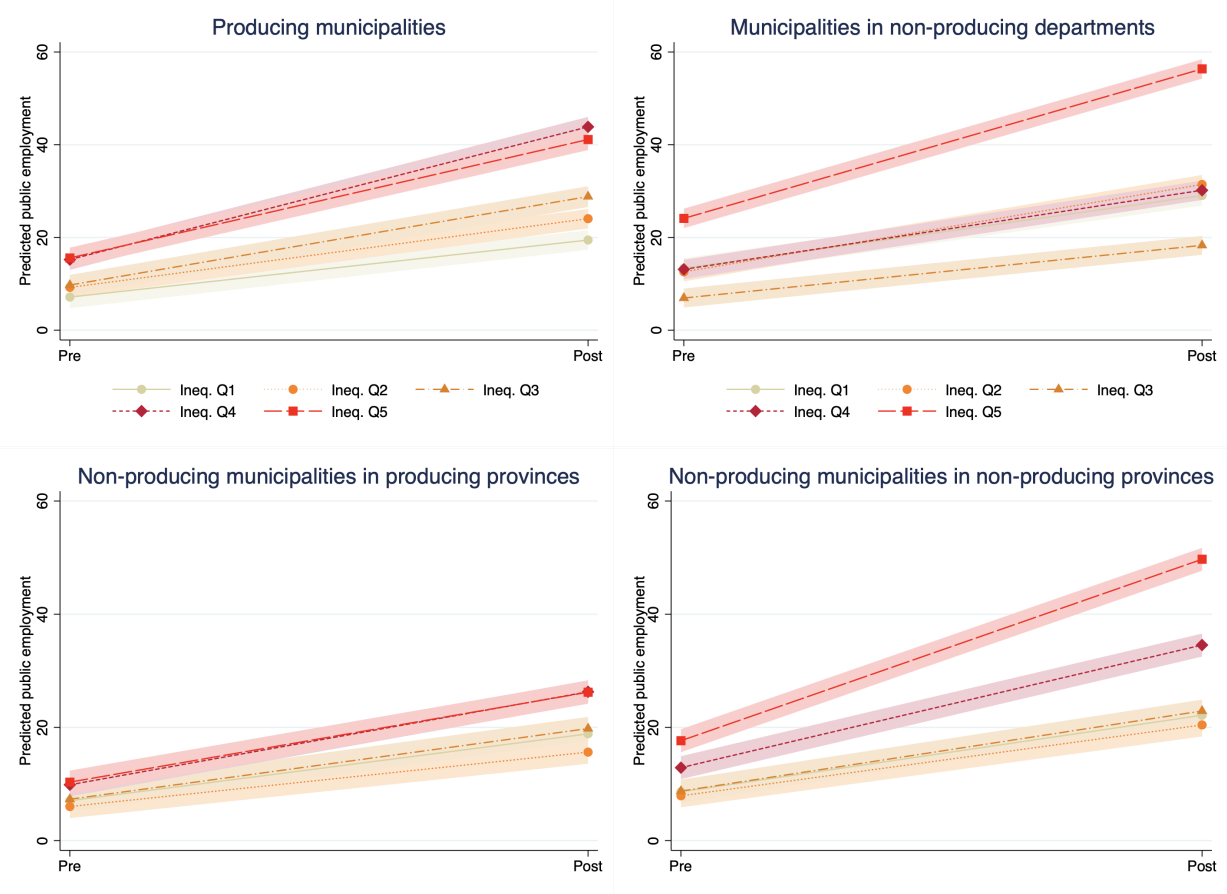
Figure 3.21: Municipal public employment variations, by mining producer status and with consumption inequality interactions. Peru, 2003-2011



Note: N=14,200. Inequality is measured with the Gini index (2007), and presented in quintiles (Q2-Q5). All regressions also include i) producer, NP.PP, NP.NPP and post reform dummies, ii) Post*Inequality dummies; iii) Producer*Inequality, NP.PP*Inequality and NP.NPP*Inequality dummies; iv) year fixed effects, and v) municipality fixed effects and clustered standard errors. Production status corresponds to year 2004. Full results in appendix table 3.15. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In summary, higher consumption inequality is certainly associated with larger municipal public employment. Mining revenues, however, are not the ones causing it. In fact, the opposite seems to be the case: if municipalities are very unequal, having higher mining revenues seems to be associated with smaller public employment increases.

Figure 3.22: Predicted public employment, by mining producer status and inequality quintiles. Peru, 2003-2011.



Note: This figure presents the marginal effects (at means) plus 95 % confidence intervals of figure 3.21's underlying regression (see appendix table 3.15, column 3).

3.7 Robustness test

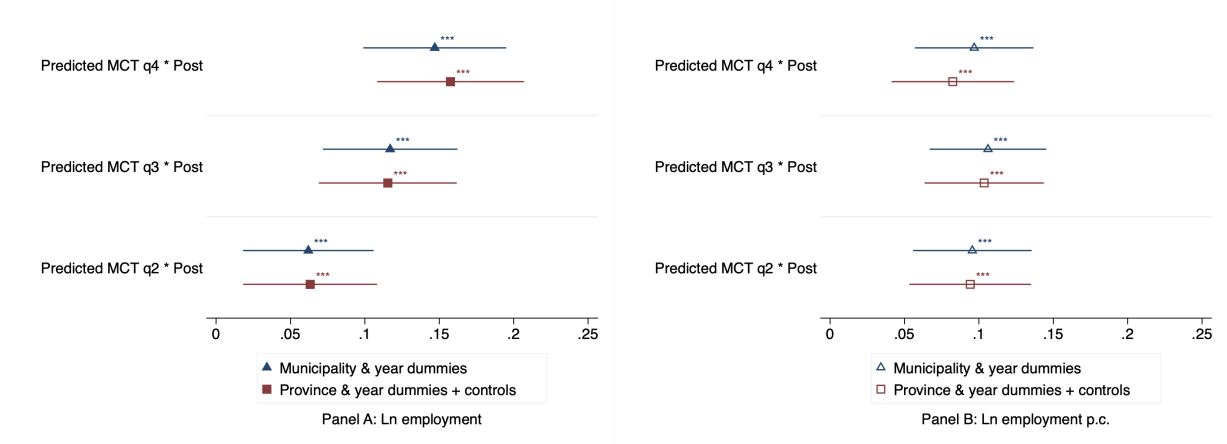
3.7.1 DID strategy II: predicted mining canon transfer quartiles

Moving forward, I consider the predicted magnitude of the MCTs as the treatment variable, regardless of a municipality's production status (see section 3.5.3). This strategy confirms that public employment increases post shock with larger mining income (H1), in favor of low skilled

workers (H2) under temporary contracts (H3). It also supports the previously observed growth in directors and officers hirings. In addition, I still do not find support for the hypothesis on larger public employment in more unequal municipalities (H4).

As depicted in figure 3.23 (panel A), the larger the transfer received by a municipality, the higher the public employment increase post shock. Municipalities in quartiles 4, 3 and 2 of total MCTs have highly significant increases in total public employment (16, 12 and 7 percent, respectively), when compared to those in the lowest transfers quartile. This will also be the case if we account for the relative importance of mining revenues in the municipal budget (appendix figure 3.48).

Figure 3.23: Municipal public employment variations, by predicted Mining Canon quartiles. Peru, 2003-2011

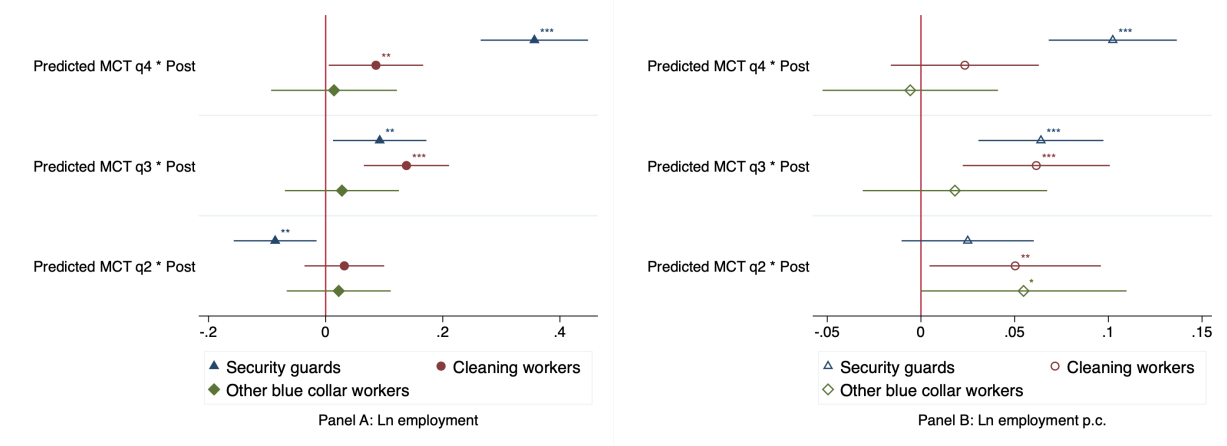


Note: N=14,200. All regressions have municipality and year dummies, as well as municipality clustered standard errors (horizontal lines). Predicted MCT quartiles correspond to year 2004. Results are available in appendix table 3.16. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The single **largest increase for any occupational group is that of security guards**. Post shock (figure 3.24, panel A), municipalities with the largest MCTs (quartile 4, q4) increased their

security guards hirings by 37 percent, followed by a 10 percent for when in MCTs q3 [37]. Cleaning workers hirings also grew by 9 and 14 percent in municipalities with MCTs quartiles 4 and 3, respectively. Both of these trends confirm my previous results.

Figure 3.24: Blue collar employees variations, by predicted Mining Canon transfers. Peru, 2003-2011



Note: N=14,200. All regressions have municipality clustered standard errors (horizontal lines), and municipality and year dummies. Predicted Mining Canon transfers correspond to year 2004. Results are also available in appendix table 3.17. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

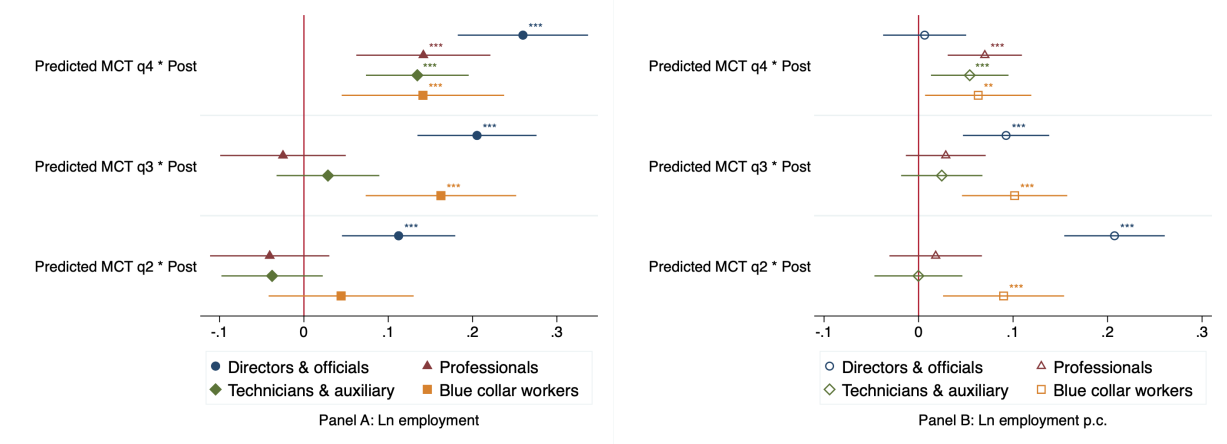
In terms of other occupations (figure 3.25, panel A), directors and officials still consistently increase across all treatment groups, by roughly 20 percent[38]. Municipalities in the 4th quartile of MCTs also increased professionals, technicians and auxiliaries hirings by 15 percent; this goes against previous findings and theoretical expectations. Nonetheless, security guards, as well as directors and officers hirings, are about twice as large as these; hence the unexpected finding should not pose a threat to my theory.

In line with expectations, figure 3.26 (panel A) corroborates that **most new workers are**

[37] Security guards numbers fell by 7 percent for MCT Q2 (figure 3.24, panel A). Nonetheless, this is not a robust finding. For instance, if the number of security guards is observed in per capita terms (figure 3.24, panel B), the coefficient becomes positive, although statistically non-significant.

[38] The increase was 27, 21 and 12 percent for municipalities with MCTs quartiles 4, 3 and 2, respectively.

Figure 3.25: Public employment composition, by occupation. DID regression by predicted Mining Canon transfers. Peru, 2003-2011



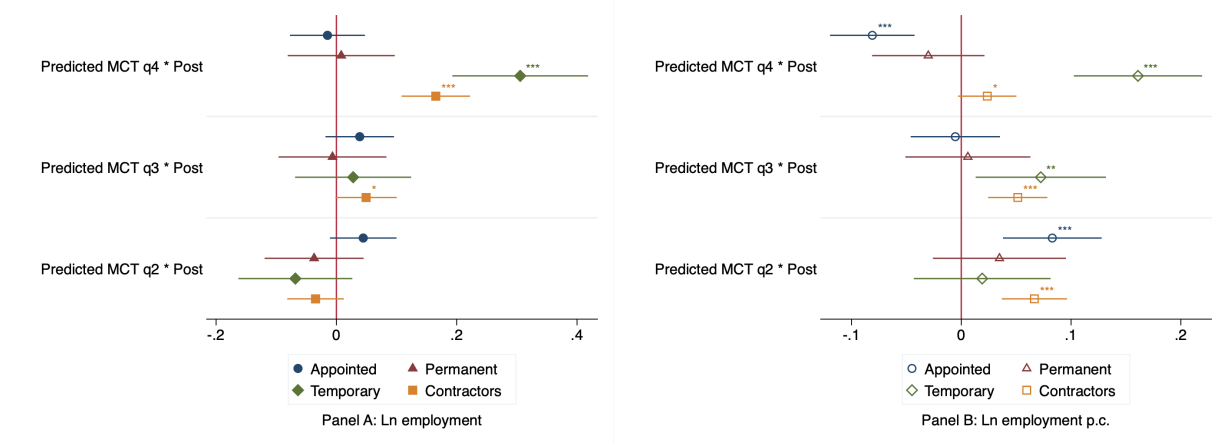
Note: N=14,200. All regressions have municipality clustered standard errors (horizontal lines), and municipality and year dummies. Predicted Mining Canon transfers correspond to year 2004. Results are also available in appendix table 3.18. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

under temporary contracts (H3). Post shock, top MCTs receiving municipalities (q4) increased their temporary contract hirings by 32 percent, vis-a-vis the control group [39]. Unlike before, the number of employees with appointed contracts only increased when the dependent variable was public employment per capita (figure 3.26, panel B). Finally, there was an unexpected 17 percent increase in employees with contractor contracts for municipalities in the 4th quartile of MCTs; this increase is robust to defining the dependent variable as public employment per capita (although the magnitude drops considerably).

Finally, with regards to hypothesis 4, I still find **no evidence that higher consumption inequality increases public employment** after year 2005, conditional on higher mining canon transfers (see figure 3.27). Post reform, only municipalities in the MCT quartile 2 that are also in the second and third inequality quintiles will have roughly 20 percent *less* public employment

[39] Previously it was also only the "top earners", producing municipalities, who increased temporary contracts.

Figure 3.26: Public employment composition, by contract type. DID regression by predicted Mining Canon transfers. Peru, 2003-2011



Note: N=14,200. N=14,200. All regressions have municipality clustered standard errors (horizontal lines), and municipality and year dummies. Predicted Mining Canon transfers correspond to year 2004. Results are also available in appendix table 3.19. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

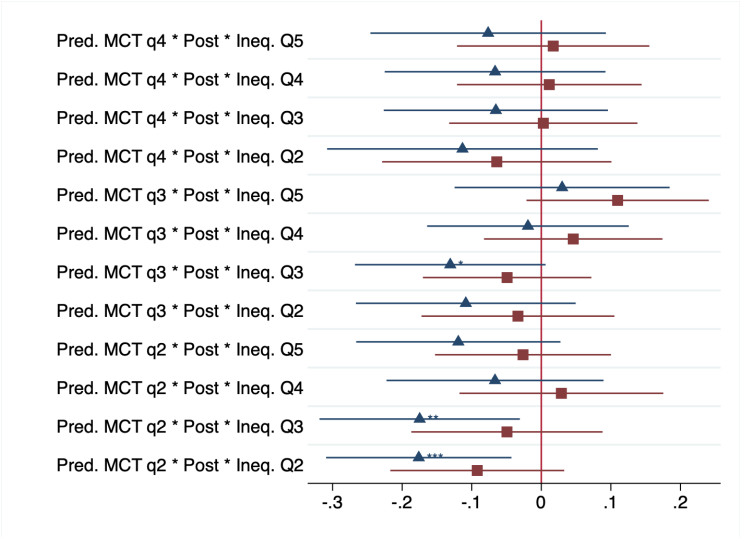
than more unequal municipalities in non-producing departments. However, these results are not robust to considering public employment in per capita terms.

3.7.2 Additional robustness test

To further assess the results' robustness, I extend the analysis along three dimensions: i) perform estimations under different samples; ii) change the treatment definition year from 2004 to years 2003 and 2005; and iii) successively reduce the post-treatment end period from 2011 to 2008. As in the main result, none of the robustness tests provide support for my H4, so I will not further discuss this hypothesis moving forward.[40]

[40]Please visit appendix 3.E.3.4 to see such results.

Figure 3.27: Municipal public employment variations, by predicted mining canon transfers and with consumption inequality interactions. Peru, 2003-2011



Note: N=14,200. Inequality is measured with the Gini index (2007), and presented in quintiles (Q2-Q5). All regressions also include i) MCT quintile and post reform dummies, ii) Post*Inequality dummies iii) MCT quintile*Inequality dummies; iii) MCT quintile*Inequality dummies; iv) year fixed effects, and v) municipality fixed effects and clustered standard errors. Production status corresponds to year 2004. Full results in appendix table 3.20. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.7.2.1 Sample exclusions

In this subsection I estimate the results using 3 different samples. I begin by excluding mining production status "switchers" from the regression: these are municipalities that either became producers or stopped producing after year 2004[41]. Next, I successively drop from the sample the Coast and Andes regions[42]. Importantly, the results excluding switchers are virtually identical to those using the full sample, so I will use them as benchmarks to see how the other two groups fare.

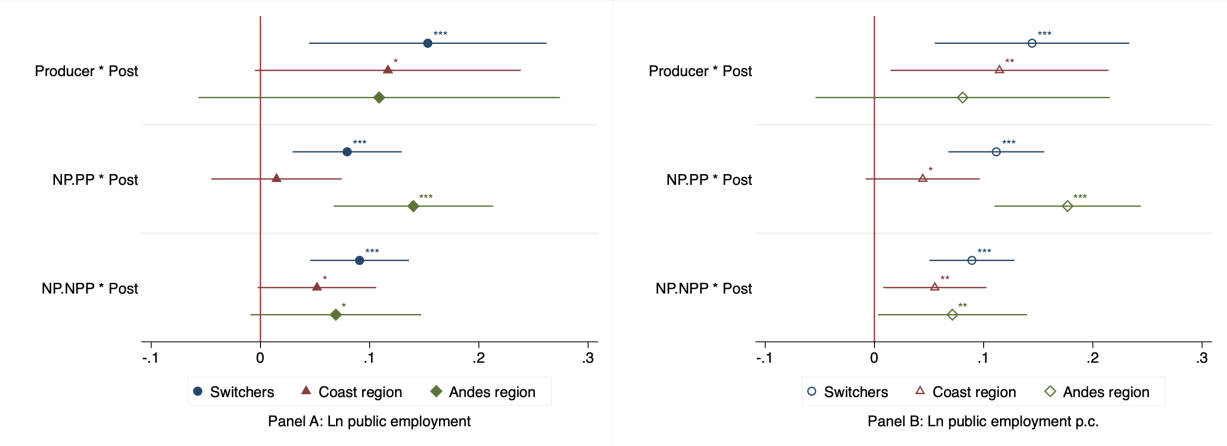
[41] 41 municipalities switched production status between years 2005-2011 (2.6% of the sample for any given year).

[42] I cannot exclude the Jungle region from the sample, because 70 percent of producing departments (the control group for DID I) is included there.

Before moving forward with the findings, I must warn that dropping the Andes region from the sample considerably increases the standard errors. This is simply because this region represents 65 percent of the full sample observations, and is also the one receiving the largest MCTs [43]. Hence, I do not necessarily consider lack of significance as a full reason for rejecting a result, as long as the coefficient size aligns with that under the main sample.

With this in mind, I generally find support for all my hypotheses across samples. There is a post shock increase in municipal employees (H1, figure 3.28), and most of the new hirings are blue collar workers (H2), together with directors and officers (figure 3.29). In addition, temporary contracts remain the most important and consistent hiring mechanism (H3, figure 3.30).

Figure 3.28: Municipal public employment variations (H1), by mining producer status. Sample exclusions. Peru, 2003-2011



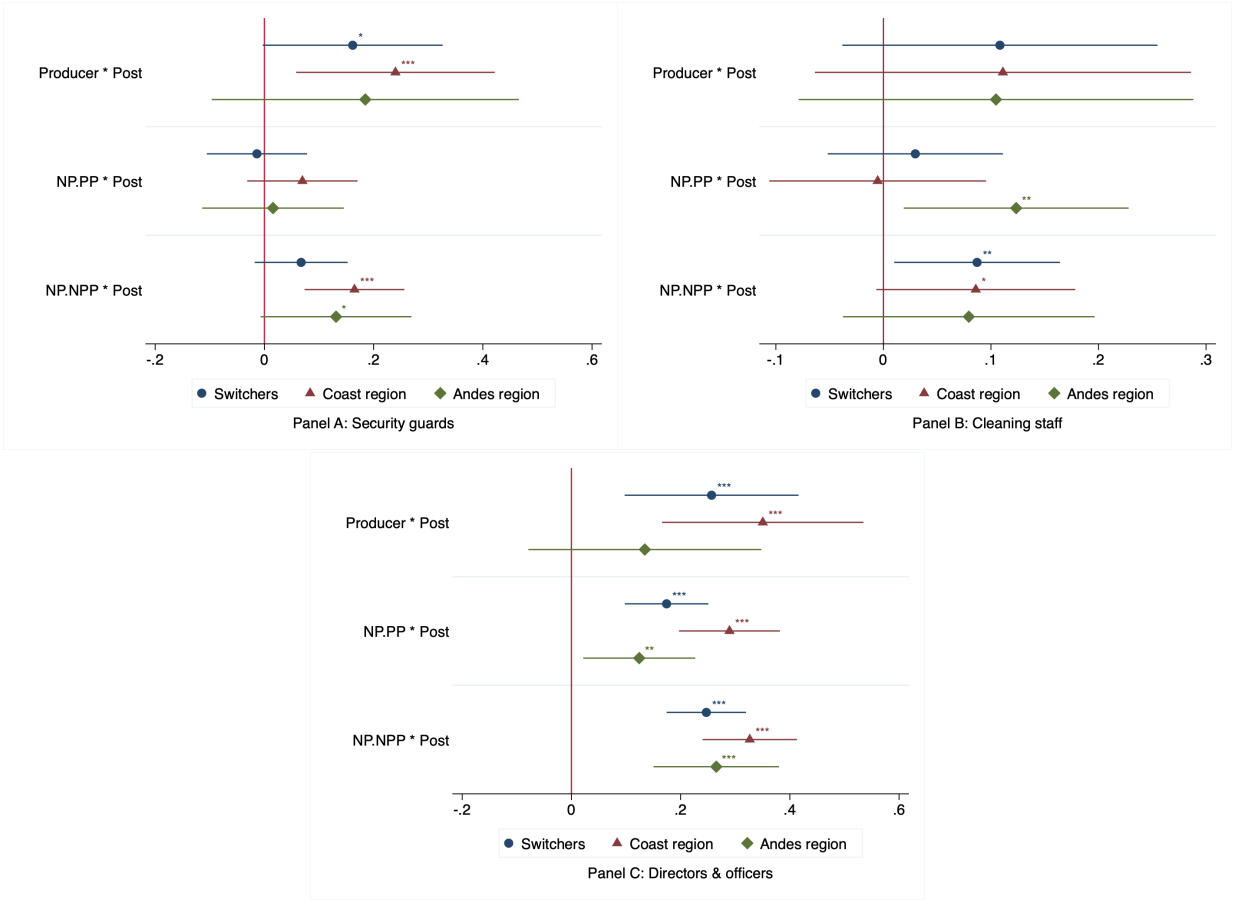
Note: Sample sizes are: N=13,812 excluding switchers, N=10,922 excluding the Coast, and N=5,016 excluding the Andes. All regressions have year and municipality dummies, as well as municipality clustered standard errors (horizontal lines). Production status corresponds to year 2004. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The hiring of security guards remains an important driver of municipal employment growth

[43] As a reference, the mean MCTs per region are 7.96, 8.27 and 0.02 million Soles (2007) for the Coast, Andes and Jungle region, respectively.

across all samples (figure 3.29, panels A). The same holds for directors and officers (figure 3.29, panels C), in which case all treatment groups consistently increase these hirings compared to the control group. Finally, cleaning staff hirings also remain robust despite sample changes. Otherwise, and as expected, there is no discernible increase in the numbers of other blue collar workers, professionals and technicians and auxiliaries (appendix figure 3.51).

Figure 3.29: Workers occupations changes (H2), by mining producer status. Sample exclusions. Peru, 2003-2011

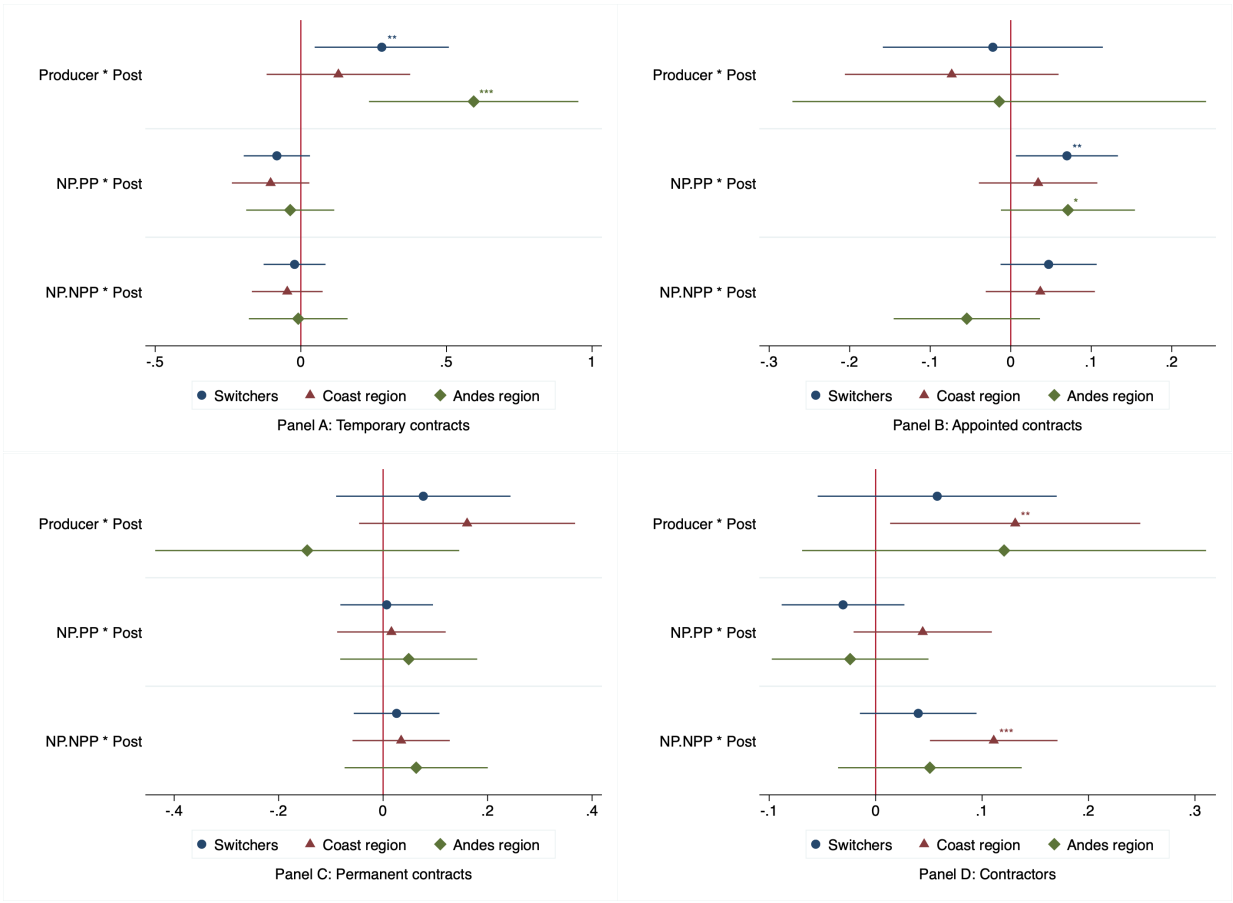


Note: Sample sizes are: N=13,812 when excluding switchers, N=10,922 when excluding the Coast, and N=5,016 when excluding the Andes region. All regressions have year and municipality dummies, as well as municipality clustered standard errors (horizontal lines). Production status corresponds to year 2004. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In terms of contract types, the increase in temporary contracts is robust to excluding the

Andes region, but not entirely to excluding the Coast (figure 3.30, panel A). Without the Coast departments, producers increase in temporary contracts remains positive, although not significant. On the contrary, the Coast exclusion does bring about a consistent increase in the number of contractor hirings (figure 3.30, panel D), for all treatment groups. Hence, this suggests that temporary contracts might be used more often in Coast region municipalities, at the expense of reducing their contractor hirings. Elsewhere, contractor hirings seem to be more popular (in all likeliness to hire more blue collar workers).

Figure 3.30: Contract type variations (H3), by mining producer status. Sample exclusions. Peru, 2003-2011



Note: Sample sizes are: N=13,812 when excluding switchers, N=10,922 when excluding the Coast, and N=5,016 when excluding the Andes region. All regressions have year and municipality dummies, as well as municipality clustered standard errors (horizontal lines). Production status corresponds to year 2004. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.7.2.2 Treatment status year change

Encouragingly, all my results are fully robust to changing the year used to define treatment status. As a reminder, a municipality is considered a producer in any given year if it has at least one mining facility with (tax paying) mineral production in such year. For the sake of brevity, I only display here the results corresponding to H1 (table 3.6). Those related to hypotheses H2 and H3 can be found in appendix tables 3.21-3.24.

Table 3.6: Municipal public employment variations (H1), by mining producer status. Changes in treatment year. Peru, 2003-2011

Ln public employment	(1)	(2)	(3)	(4)	(5)	(6)
	Production status corresponds to					
	Year 2003		Year 2004		Year 2005	
Producer * Post	0.158***	0.134***	0.157***	0.121***	0.191***	0.147***
	(0.05)	(0.05)	(0.05)	(0.04)	(0.05)	(0.04)
NP.PP * Post	0.088***	0.105***	0.082***	0.098***	0.097***	0.111***
	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)
NP.NPP * Post	0.093***	0.075***	0.096***	0.078***	0.086***	0.071***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Ln public employment	T.	P.C.	T.	P.C.	T.	P.C.
Munic. & year effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.738	0.536	0.737	0.535	0.738	0.537

Note: N=14,200. The dependent variable is expressed as a total (T.) and per capita (P.C.). All regressions have year and municipality dummies, as well as municipality clustered standard errors (horizontal lines). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.7.2.3 Post period end year

In this section, I successively shorten the post period end year from 2011 to 2008. Generally, this change translates into a decrease of the magnitude of the effects as we *shorten* the post period end year. However, I still find support for all my hypotheses, albeit with smaller effect sizes.

To start with, the total number of post-shock municipal employees remains consistently

larger for all treatment groups (table 3.7). The results are practically identical when public employment is in per capita terms (appendix table 3.25).

Table 3.7: Municipal public employment variations (H1), by mining producer status. Changes in end of post-period. Peru.

Ln public employment	(1)	(2)	(3)	(4)
	Post period ends in year			
	2011	2010	2009	2008
Producer * Post	0.157***	0.150***	0.149***	0.132**
	(0.05)	(0.05)	(0.06)	(0.06)
NP.PP * Post	0.082***	0.079***	0.074***	0.052*
	(0.03)	(0.03)	(0.03)	(0.03)
NP.NPP * Post	0.096***	0.087***	0.090***	0.062**
	(0.02)	(0.02)	(0.02)	(0.03)
N	14200	12600	11028	9452
Munic. & year effects	Yes	Yes	Yes	Yes
R^2	0.737	0.735	0.737	0.734

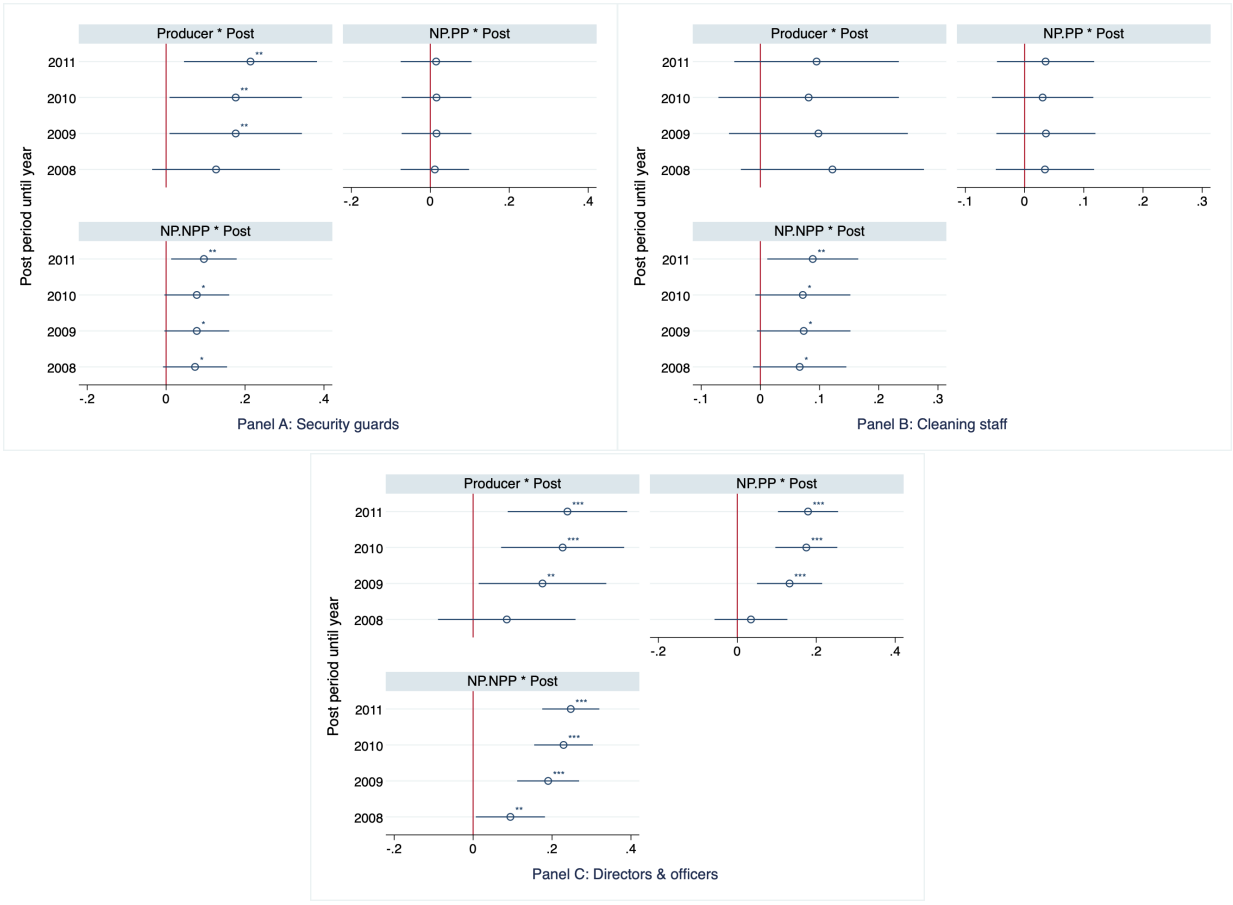
Note: N=14,200.All regressions have year and municipality dummies, as well as municipality clustered standard errors (horizontal lines). Production status corresponds to year 2004. Results with ln public employment per capita are available in appendix table 3.25. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In terms of occupation (figure 3.31), the size of the security guards hirings for producers becomes non-significant when the post-period ends in year 2008, although it remains positive. Similarly, the directors and officers hirings magnitude diminishes also; for producers and NP.PP the coefficient remains positive but becomes non-significant with a 2008 end of period. Finally, the increase in cleaning staff remains constant in magnitude and significance all along.[44]

With regards to workers contractual arrangements (figure 3.32), the number of temporary hirings always remained constant in magnitude and significance. On the opposite, the increase in appointed contracts drastically drops and becomes non significant (even negative for NP.PP) in end of period year 2008. Similarly, the previously observed increase in contractor contracts for NP.NPP quickly becomes negative and non-significant in end of period year 2010. This suggests

[44]There are no important changes in the coefficients for other blue collar workers, professionals and technicians & auxiliary workers. Results can be found in appendix figure 3.53.

Figure 3.31: Workers occupation changes (H2), by mining producer status. Changes in end of post-period. Peru, 2003-2011

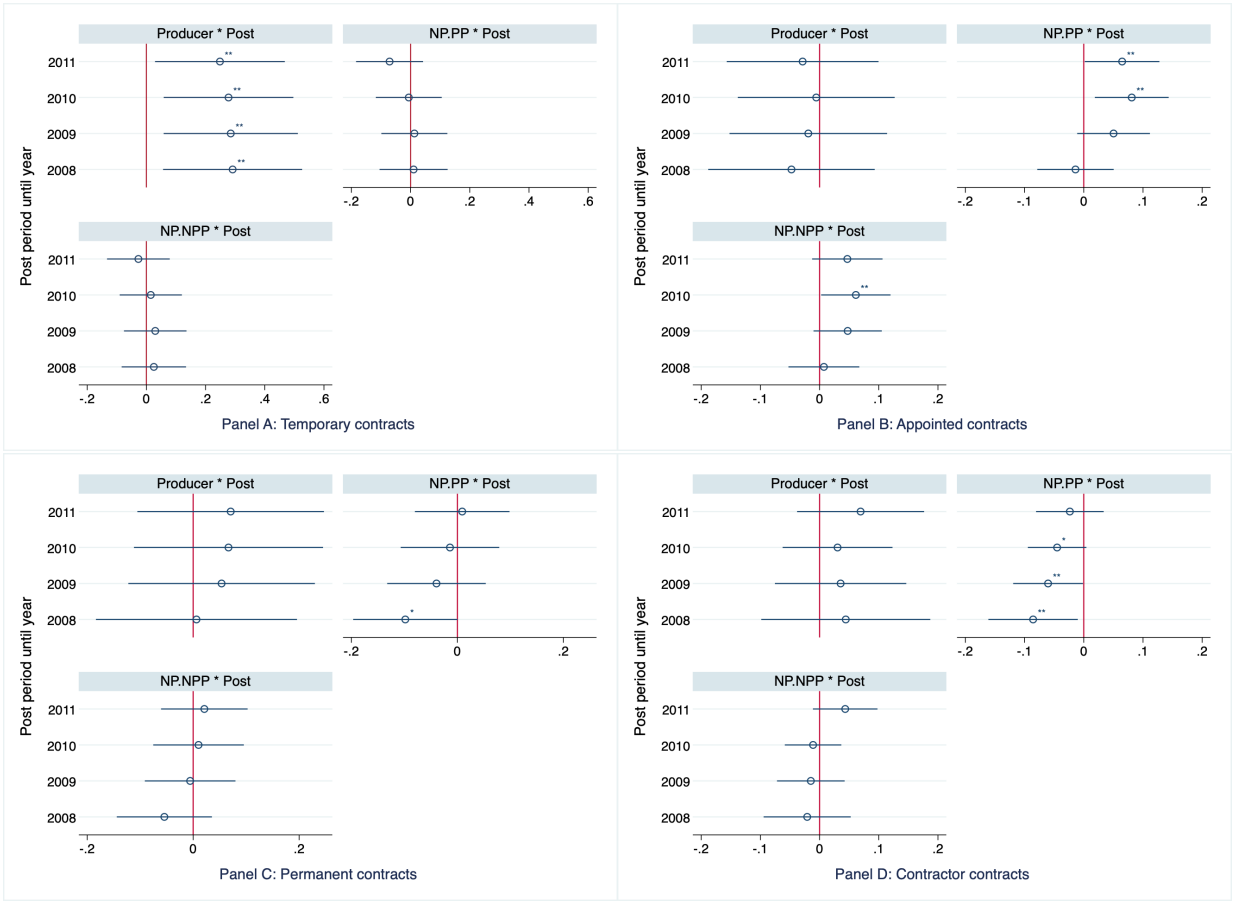


Note: N=14,200. All regressions have year and municipality dummies, as well as municipality clustered standard errors (horizontal lines). Results with other blue collar workers, professionals and technicians & auxiliary workers can be found in appendix figure 3.53. Production status corresponds to year 2004. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

that **the only fully robust contractual trend is that of the increase in temporary hirings**.

In short, this section presents several instances of smaller effect sizes with shorter time periods, particularly so in terms of the size of the security guards, directors and officers hirings. The simplest explanation for this phenomenon is that my treatment variables, which were defined for pre-shock production status, do not capture the full extend of the magnitude of MCTs, providing rather a lower bound estimate of it. Hence, as the MCTs kept increasing in time, this means that

Figure 3.32: Contract type variations (H3), by mining producer status. Changes in end of post-period. Peru, 2003-2011



Note: N=14,200. All regressions have year and municipality dummies, as well as municipality clustered standard errors (horizontal lines). Production status corresponds to year 2004. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

municipalities could further increase their number of hirings later on, as resources increased.

There is an alternative factor that could be contributing to such results. As per the law, MCTs are earmarked for capital expenditures, and may **not** be legally used for current expenditure such as wages and social contributions. Evidently, municipal governments in producing departments managed to rearrange their budgets in ways that would permit to increase their hirings, even if MCTs funds were not "directly" used for it. Such an adaptation process might have

required some time, which is why we could be seeing smaller effects in earlier years post reform.

3.8 Productive public employment?

Previously we saw that, in the period under study, larger mining rents substantially increased the number of security guards and cleaning workers, and also that of directors and officials. In this section, I observe some key municipal goods and services linked to those workers and find that treated municipalities perform marginally better than the control, despite having more labor and funds explicitly earmarked for such purposes. While these findings are merely *exploratory*, they suggest perhaps those additional workers were not as productive as desired, in line with my theoretical predictions on redistributive public employment.

Higher mining rents systematically brought about employees at the very ends of the skills distribution, and no mid skill workers nor professionals. If municipalities were indeed making use of these extra resources to grow and better staff themselves, why should they mainly increase these two types of workers? Are they providing more and better services to the municipality? Or could this differential increase be largely attributed to clientelism and/or income redistribution?

Municipal employees in Peru are core civil servants; this broad category excludes teachers, health professionals and law enforcement agents. Unlike, for instance, teachers, the outcomes linked to core civil servants' performance are inherently difficult to measure[45]. **In Peru, there are no readily available indicators to assess local government employees performance**, neither in the period under study nor recently. Consequently, as a second best strategy, I will observe

[45] Some key areas are local service delivery, financial management (including efficient and cost-effective use of resources) and human resource management.

changes in the provision of two fundamental local public services: garbage collection and basic infrastructure. While certainly imperfect, this approach is an initial step to evaluate whether there is *any* improvement in outcomes associated with the increase in certain types of public employees.

The logic to evaluate garbage collection services is straight forward. First, it is one of the core services provided by Peru's municipalities. Hence, bigger and better governments should potentially increase or improve garbage collection coverage. Second, I previously reported a mining rents induced increase in cleaning staff; if these workers were indeed hired to perform sanitation related services, then we should observe *some* improvement in this field.

With regards to infrastructure provision, Peru's law clearly establishes that Mining Canon transfers can only finance new public investment projects directly linked to public goods provision. Alternatively, up to 20 percent of MCTs can be used to maintain such projects[46]. Consequently, treated municipalities should differentially raise their public infrastructure expenditure. Furthermore, the large increase in directors and officials implies a growing body of managers that should supervise the success of these projects, starting by the disbursement of the funds intended for it.

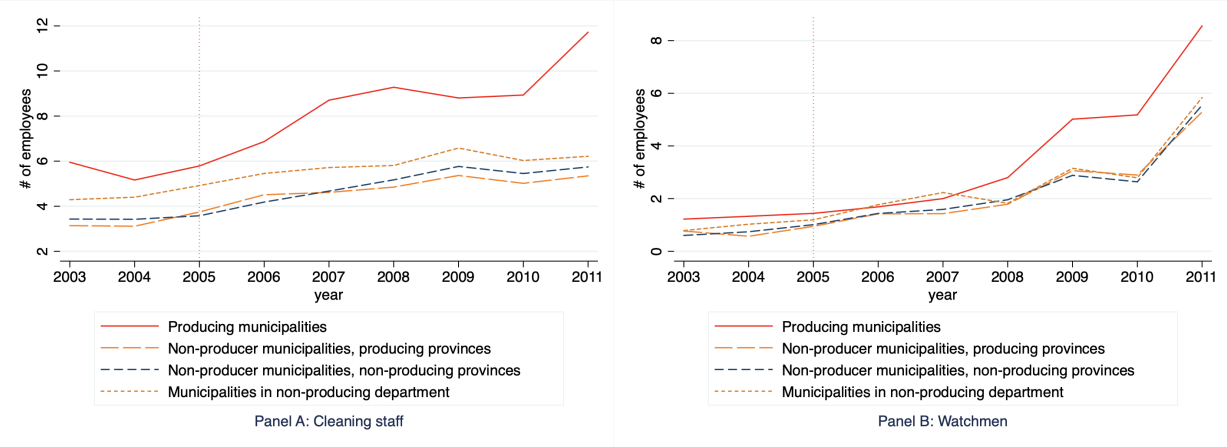
3.8.1 Blue collar workers

I will first discuss the rents driven increase in the number of cleaning workers. In the period under study, all municipalities in the sample raised their post-reform cleaning staff hirings by 26 percent, vis-a-vis the control group. In addition, NP.NPP further raised their hirings by roughly 8 percent, despite having similar cleaning staff numbers pre-shock than municipalities

[46]Fiscal Year 2006's Budget Law (*Ley de Presupuesto del Ejercicio Fiscal 2006*) and Congreso de Peru (2014).

in the control group (figure 3.33, panel A). Larger population size was not the culprit behind this increase: as seen in section 3.6.2, the cleaning staff increase, as well as that of security guards and directors and officials, held across all treated groups when public employment was measured as a share of the population (figures 3.18 and 3.19, panel B).

Figure 3.33: Number of cleaning workers and security guards, by municipality's mining production status. Peru, 2004-2011



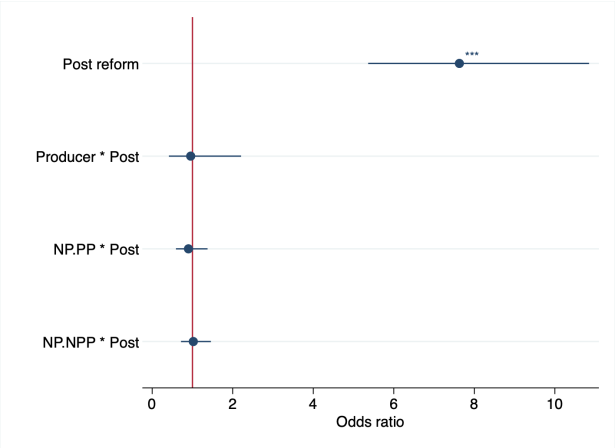
Note: Production status corresponds to year 2004.

It is certainly possible that NP.NPP municipalities need to improve their trash collection services, and hence more workers are hired for such purposes. However, my simple exploratory analysis shows no differential improvement in the trash collection and sanitation services in NP.NPP municipalities, despite the extra 8 percent growth in cleaning workers.

For starters, homes are no more likely to get their trash picked up there than in the control municipalities. As seen in figure 3.34, the odds of residential trash collection services being provided post shock is about 7 times higher for *all* municipalities. NP.NPP municipalities, nonetheless, are not more likely to have this service; they fare just as well as the control group, despite

hiring more workers.[47]

Figure 3.34: Are municipal residential trash pickup services provided? Logistic DID regression, by mining production status. Peru, 2004-2011.



Note: N=12,622. The coefficients are exponentiated, expressing odds ratios. The regression includes municipality and year fixed effects, as well as municipality clustered standard errors (in parentheses). Production status corresponds to year 2004. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The amount and frequency of trash collection in NP.NPP municipalities did not increase either (table 3.8, columns 1-2). Furthermore, they did not show improvements in the number of special cleaning and public hygiene operatives, vis-a-vis the control (table 3.8, columns 3-4).

I now briefly discuss the large and consistent rents driven increase in security guards. In the period under study, all municipalities in the sample increased their post-reform security guards hirings by 88 percent. In addition, treated municipalities further raised their hirings by roughly 12 percent[48], despite having either the same or more security guards pre-shock than municipalities in the control group (figure 3.33, panel A). Hence, it was not a "catching up" effort, nor was it induced by population size changes.

[47] Roughly 8 percent of the NP.NPP municipalities did not have residential trash collection services by year 2011. Hence, there was still room for improvement.

[48] 17 percent producers, and 7 percent NP.NPP.

Table 3.8: Changes in municipal trash collection and public hygiene services, by mining producer status. Negative binomial regression. Peru, 2004-2011.

	(1)	(2)	(3)	(4)
	Trash collection		*Special operatives*	
	Daily av. (Tons)	Frequency	Cleaning	Public hygiene
Producer * Post	1.093	1.145***	0.628	1.383
	(0.07)	(0.04)	(0.20)	(0.61)
NP.PP * Post	0.975	1.018	0.666**	0.913
	(0.03)	(0.03)	(0.13)	(0.24)
NP.NPP * Post	0.990	1.038	0.926	0.934
	(0.03)	(0.02)	(0.15)	(0.23)
Zero inflated	No	No	Yes	Yes
Munic. & year effects	Yes	Yes	Yes	Yes

Note: N=12,622. All regression coefficients are exponentiated, expressing incidence ratios. Appendix 3.C further describes these variables. Regressions (3) and (4) are zero inflated by non-province capital districts. All regressions include municipality and year fixed effects, as well as municipality clustered standard errors (in parentheses). Production status corresponds to year 2004. $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Why should the security guards demand grow when in presence of higher income? This could certainly be a response to (perceived or real) increases in local criminality. These workers, however, are *not policemen*[49], and cannot perform law enforcement activities; they simply guard the municipal government premises, which in Peru's average municipality is often enough just a small house or office building. Hence, it is not entirely unlikely either that a share of this large increase in security guards might have been driven by clientelistic politics, as per my theory. However, further evidence is required to properly test this claim.

3.8.2 Directors, officers and public infrastructure

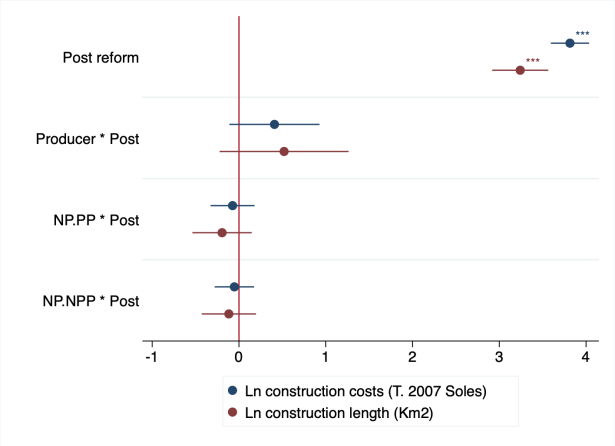
Between 2006 and 2011, treated municipalities increased their hirings of directors and officers by roughly 20 percent, compared to the control. During the same period, MCTs represented on average 23 percent of total revenues[50]; on the opposite, the control group received no MCTs.

[49] Law enforcement agents are not municipal employees, they are under the regional and national governments.
[50] 19, 22 and 26 percent of total revenues, for producers, NP.PP and NP.NPP, respectively.

As per the Mining Canon law, these funds must be spent in public infrastructure; and, presumably, municipal managers ought to oversee such process, among other tasks.

I first revise the evolution of rural and urban roads construction and repairing (figure 3.35). After year 2006 all Peruvian municipalities spent roughly 40 percent more Soles in roads construction and repairing. None of the treated groups, however, reported neither building nor spending more on roads than the control group. Hence, MCTs were not invested in more and/or better roads.

Figure 3.35: Rural and urban roads construction and repairing, by mining production status. Peru, 2004-2011.



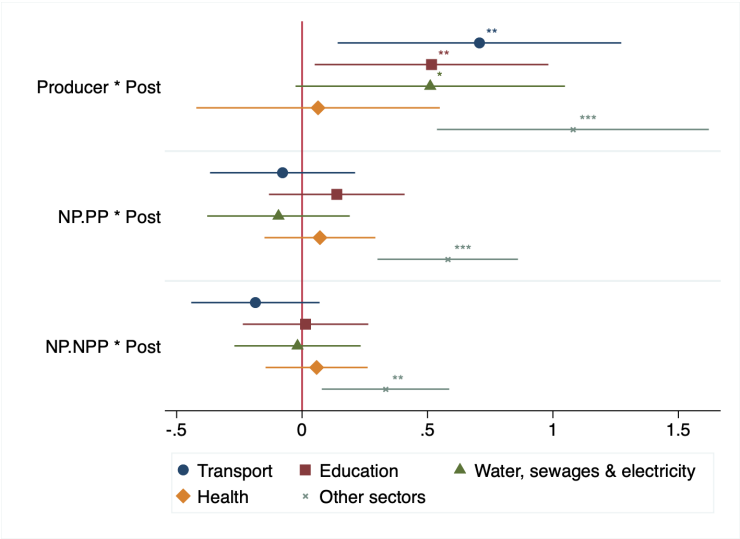
Note: N=12,622. The regressions include municipality and year fixed effects, as well as municipality clustered standard errors (in parentheses). Production status corresponds to year 2004. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Post shock infrastructure expenditure had an important increase in mining producing municipalities across sectors, but it barely did for the other two treated groups (figure 3.36). Compared to the control, producing municipalities increased their reported infrastructure expenditure in all sectors, but for health[51]. NP.PP and NP.NPP also increase infrastructure expenditure in

[51]The increases were the 72, 50, 51 and 106 percent for the areas of transport, water sewages & electricity, education and other sectors.

"other sectors" by 58 and 32 percent, respectively; the magnitude of these uncategorized expenditures, nonetheless, is rather modest. Moving forward, we will see that NP.PP and NP.NPP's total infrastructure expenditure post shock does not really differentiate from that of the control group.

Figure 3.36: Infrastructure construction expenditure (Ln), by sector and mining production status. Peru, 2004-2011.



Note: N=12,622. Amounts are in (Ln) thousand 2007 Soles. The regressions include municipality and year fixed effects, as well as municipality clustered standard errors (in parentheses). Production status corresponds to year 2004. Regression output available in appendix table 3.26. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

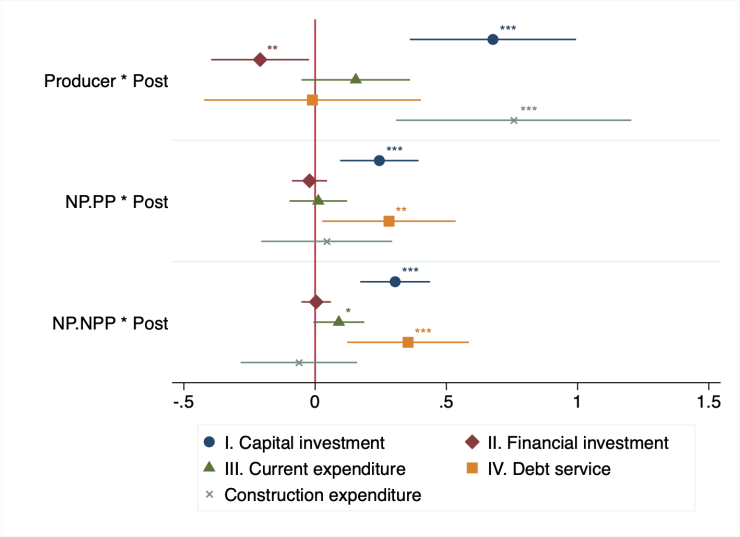
As a robustness check, I assess if there were changes in the composition of total municipal expenditure[52]. Since the MCTs ought to be spent in infrastructure, treated municipalities should present a post-shock increase in capital investments, and no major increases in financial investments, current expenditures or debt service. Importantly, capital investments include infrastructure and construction expenditures, as well as pre-investment studies and equipment

[52]Unlike the previous (self-reported) infrastructure expenditure data, this information is added to RENAMU's dataset directly by Peru's MEF, not the municipalities.

acquisition for other purposes (for instance, new computers).

As expected, all treated groups increased their post shock capital investments (Figure 3.37); however, NP.PP and NP.NPP did not increase their total public infrastructure expenditure vis-a-vis the control[53]. Hence, NP.PP and NP.NPP's roughly 26 percent increase in capital investments was predominantly driven by pre-investment studies and equipment acquisitions. Finally, and unlike producers, NP.PP and NP.NPP increased their debt service payments by roughly 40 percent, compared to the control group.

Figure 3.37: Municipal expenditures (Ln), by mining production status. Peru, 2004-2011.



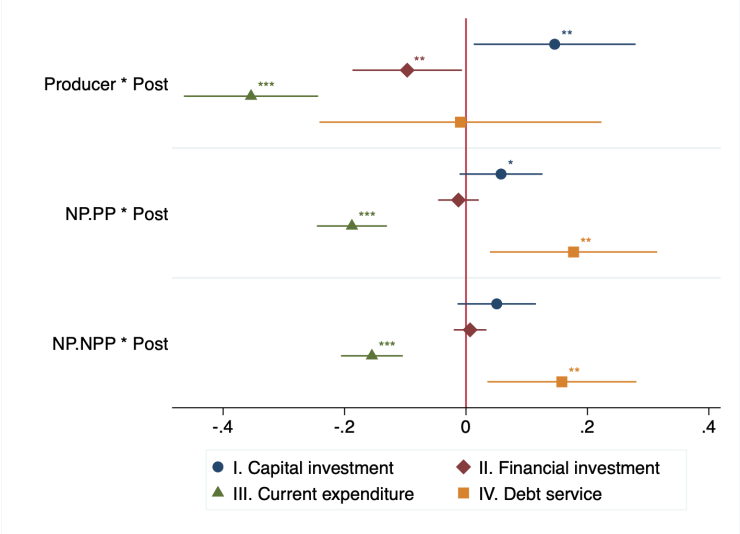
Note: N=12,622. Total municipal expenditure = I + II+ III + IV. Construction expenditure is a subset of I. All amounts are in (Ln) thousand 2007 Soles. Appendix 3.C describes the variables in detail. The regressions include municipality and year fixed effects, as well as municipality clustered standard errors (in parentheses). Production status corresponds to year 2004. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Producing municipalities post shock capital investments represented 14 percent more of their total expenditure than it did for the control group (figure 3.38), which is 5 percentage points

[53] As a reference point, producing municipalities did closely increase their construction and capital expenditures.

less than the 19 percent share that MCTs represent on total revenues. On the contrary, NP.PP and NP.NPP's post shock capital investments represented roughly the same share of total expenditures than for the control group. Furthermore, post shock debt service payments were about 18 percent a larger share of NP.PP and NP.NPP's municipal budget. This information, all together, suggests that producers seemed to have had an MCT driven public infrastructure increase, while NP.PP and NP.NPP might have spent substantial amounts of it in debt service.

Figure 3.38: Municipal expenditures as a share of total expenditure, by mining production status. Peru, 2004-2011.



Note: N=12,622. Total municipal expenditure = I + II+ III + IV. All categories represent the share of total expenditure. The regressions include municipality and year fixed effects, as well as municipality clustered standard errors (in parentheses). Production status corresponds to year 2004. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

This quick public expenditure analysis calls into question the capacities of newly hired directors and officials. For starters, producing municipalities had a gap of roughly 5 percentage points of their MCTs income. More dramatically, NP.PP and NP.NPP did not spend much (if at all) of their 23 percent MCT driven budget increase in public infrastructure; rather, they report-

edly bought machines and paid debts. If one is to assume honest behavior (although it might be reasonable not to[54]), this implies at the very least that managerial staff in treated municipalities were incapable of guaranteeing the proper disbursement of the often quite large MCTs.

These findings are broadly consistent with Neyra Araoz (2016), which observed that mining revenues decentralization had a negative and statistically significant impact on household's access to piped water and electricity wiring. These local services require both physical infrastructure expenditure, as well as proper local management and maintenance. As per my previous findings, both of these pre-conditions likely did not hold in Peruvian municipalities at the time. Hence, my findings also suggest that the **benefits from the mining revenues decentralization might have been smaller than expected** for Peru's average citizen.[55]

Summing up, this section *suggests* that treated municipalities' disproportionate increase in cleaning workers and managers might have been indeed an income redistribution exercise, even if just partially. My preliminary data shows that the many new workers did not seem to bring about a proportional increase in related goods and services, which permits questioning if such new hires were indeed driven by the need or will to provide more services. While I have no data to properly measure Peruvian municipal public employees performance, my findings certainly put into question the management capacities (hiring decisions perhaps among them?) of the many new directors and officials.

[54] Before the mining revenue shock (year 2002), 29 percent of Peruvians believed corruption was the country's main problem. By year 2010, a much larger 55 percent held the same believe, turning it into the country's main perceived issue. Furthermore, between years 2006 and 2010 about 55 percent of the population believed municipal governments were either corrupt or very corrupt. See Proetica (2006) and Proetica (2010), for Transparency International's Peru branch (Proetica) bi-annual national survey on corruption perceptions.

[55] Along the same lines, Loayza and Rigolini (2016) found a mixed impact of mining on socioeconomic outcomes at local municipalities in Peru, since it has a positive income effect but a negative distributional effect.

3.9 Conclusions

This paper explores the effect of additional natural resource revenues on municipal public employment in Peru. I exploit the variation in exogenous mining revenue shocks across municipalities, due to a Mining Canon law reform in year 2004, which sharply increased the amount of mining rents transferred to municipalities in favor of those producing minerals. This legal reform occurred within a broad national decentralization process, which made emphasis in political and economic de-concentration in favor of department and (especially) municipal governments.

The core issue in this paper is politicians' dilemma of how to spend increasing natural resource rents: should they be distributed as patronage in the form of public employment to a narrow clientele, or spent on providing broad-based public goods? As per my framework, municipalities receiving larger mining transfers will differentially increase their number of public employees (H1), particularly so in terms of low skilled workers (H2) with temporary contracts (H3). Higher inequality *should* further intensify this phenomenon (H4).

Consistent with my theory, all Mining Canon recipient municipalities have higher public employment post shock than municipalities in non-producing departments. Furthermore, larger mining rents substantially increased the number of temporary contract workers, many of which were in low skilled occupations (security guards and cleaning staff). Higher mining rents also increased directors and officials, a group of high skill workers who come either as elected officers or political appointees. Although not directly predicted by my theory, this growth in managerial staff seems in line with clientelistic public employment logic.

With regards to H4, I contribute to the literature with the first empirical test (to my knowledge) of how inequality mediates the effects of natural resource rents on public employment. In

the current context, high inequality does not further enhance resource rents driven public employment growth. Nonetheless, I do observe that municipal public employment is consistently and *considerably* higher in the most unequal municipalities, particularly if they have *small* resource rents. Hence, while I cannot support my H4, my findings do suggest a strong link between inequality and public employment size, which could be further explored in future research.

A key issue in my analysis is that hiring more public employees after an income shock does not automatically imply these workers were not needed and will not be put to a good use. My findings in terms of the skills composition of new recruits provide some initial food for thought: Is it possible that municipalities receiving high MCTs, which were already better off before the legal reform, had skill gaps only at the tails of the skills distribution? Seems rather unlikely.

Unfortunately, there is no data to properly measure Peruvian municipal public employees performance. Nonetheless, I present preliminary findings which put into question the management capacities (hiring decisions among them?) of the many new directors and officials. Importantly, non-producing municipalities in producing provinces (NP.PP) and in non-producing provinces (NP.NPP) had no increase in public infrastructure vis-a-vis the control group; this is unexpected, since MCTs are explicitly earmarked for such purposes. What where these newly hired managers supervising then, if not the proper use of the very generous mining revenues?

In year 2004, the year of the MCT legal reform, Peru's civil service was vastly politicized: municipalities had high discretion in HR decisions and little merit considerations in hiring (Iacoviello and Strazza, 2014). A key lesson from this paper then is that, in the absence of strong and meritocratic civil service rules, higher resource rents could be redistributed through unproductive public employment, likely with no proportional increase in public goods and services delivery.

Surprisingly, Larrain and Perello (2019) and Monteiro and Ferraz (2012) confirm that rents

driven redistributive public employment will *also* be found in countries with strong and meritocratic civil services, like Chile and Brazil. In line with my findings, they observe close to no impacts on municipal infrastructure investment, suggesting this additional employment was largely unproductive.

This essay also contributes to the literature on fiscal decentralization, by showing that additional resource rents can negatively affect local governments' performance. Perez-Sebastian and Raveh (2016) sustains that fiscally decentralized economies are more vulnerable to the natural resource curse than fiscally centralized ones. Furthermore, Ostrom (2001), Rodden (2004) and Kauneckis and Andersson (2009) show that decentralization yields positive results *only* if local governments have proper institutional constraints, to keep politicians accountable. Without "a minimal degree of accountability", Bardhan and Mookherjee (2006) claim, local elites will likely capture local governments and increase rent seeking, as we just observed in Peru's oversized public employment case.

My current findings add to Larrain and Perello (2019) and Monteiro and Ferraz (2012), and highlight the need cautiously designing sharing rules for natural resource revenues across levels of government. Even when in presence of strong and meritocratic civil services (as in Brazil and Chile [56]), natural resource rents revenues will not be put to optimal use by subnational governments unless strong and consistent *local* accountability mechanisms are in place.

[56] Monteiro and Ferraz (2012) and Larrain and Perello (2019) for Brazil and Chile, respectively.

3.A Peru's politico-administrative divisions

The Republic of Peru is a Unitary and Decentralized State, with 4 different levels of administrative subdivisions: departments, provinces, municipalities and small population centers[57]. By year 2005, Peru was divided in 24 departments and the Constitutional Province of El Callao, 195 provinces and 1835 municipalities.

Departments are the units of regional government, while the local government is composed by provinces and their municipalities. Local government authorities sit in municipalities which, as per the Organic Law for Municipalities (2003)[58], exist in 4 different instances:

1. In Peru's capital;
2. In provinces Capital Districts (provincial municipalities), to govern such municipality while also having an extended jurisdiction to the provincial territory;
3. In district capitals (district municipalities), with jurisdiction throughout the municipality's territory;
4. In small population centers (SPC).

In this paper the unit of analysis is the municipality. While the SPC is legally the smallest politico-administrative subdivision in Peru, I disregard them for several reasons. First and foremost, they do not directly receive Central Government Transfers, such as the Mining Canon.

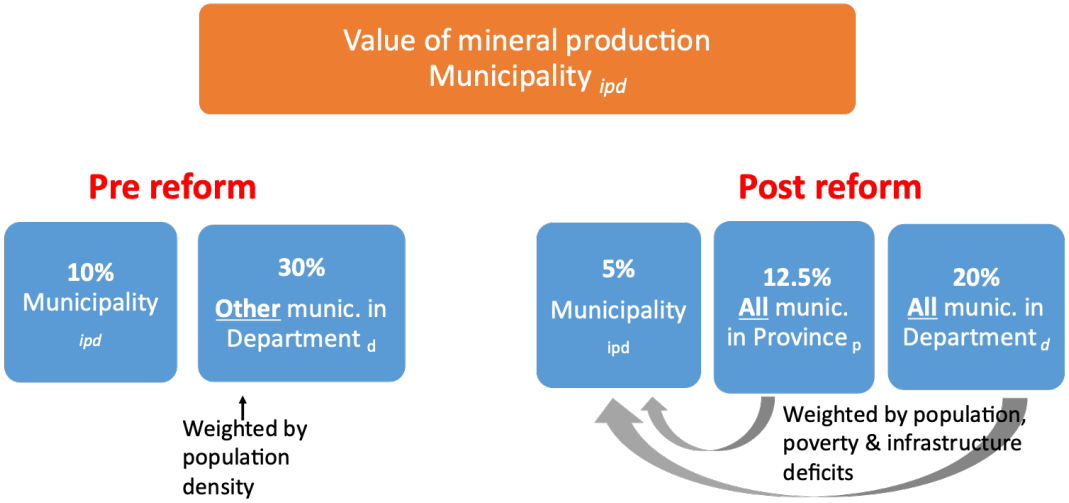
[57] These are rural conglomerates of more than 500 Peruvian citizens, with a clearly defined territory within a District. Created by decree of their corresponding municipal provincial council, these politico-administrative units comply with the functions and local public service delivery that are delegated to them on a case by case basis, with whichever budget the municipal province and district governments are voluntarily willing to assign.

[58] This law was promulgated in year 2003, within the framework of a decentralization process that made emphasis on political and economic deconcentration in favor of regional and (specially) local governments.

Furthermore, their existence is somewhat "optional": while a given province must have municipalities, no municipality ought to have SPC (these are created by citizens request, to further decentralize services provision). Finally, it is very cumbersome to get statistics at that level.

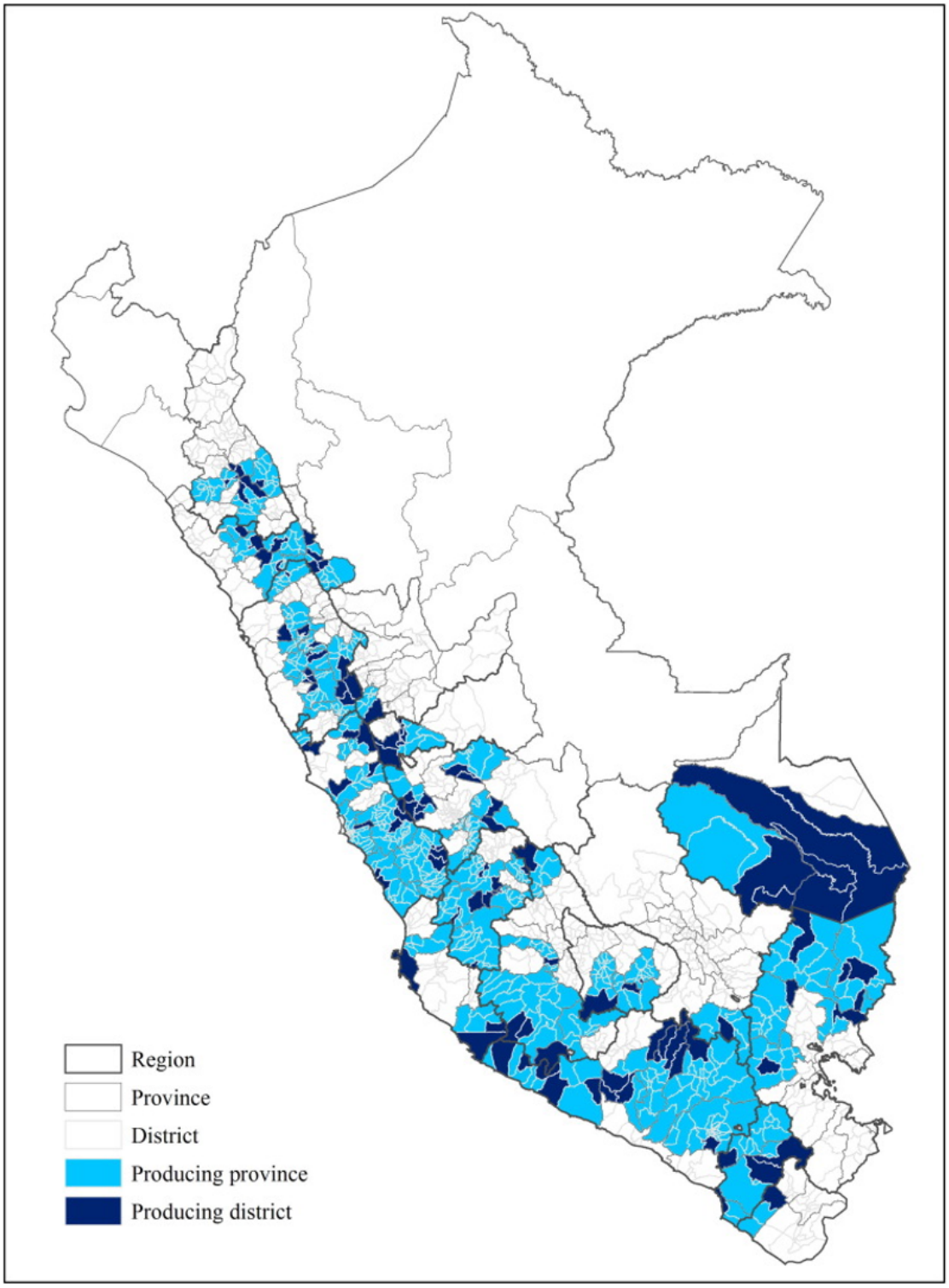
3.B Additional figures

Figure 3.39: Mining revenues redistribution, before and after Mining Canon law No. 28322



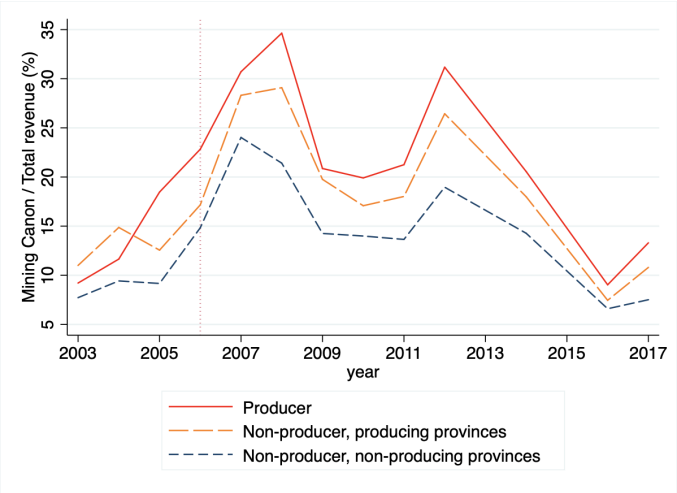
Note: This figure explains how the value of mineral production in any given municipality i is to be redistributed to municipal governments, both before and after the implementation of Mining Canon law No. 28322, adopted in August of 2004.

Figure 3.40: Mining status by Department, Province and Municipality. Peru, 2006



Source:Loayza and Rigolini (2016)

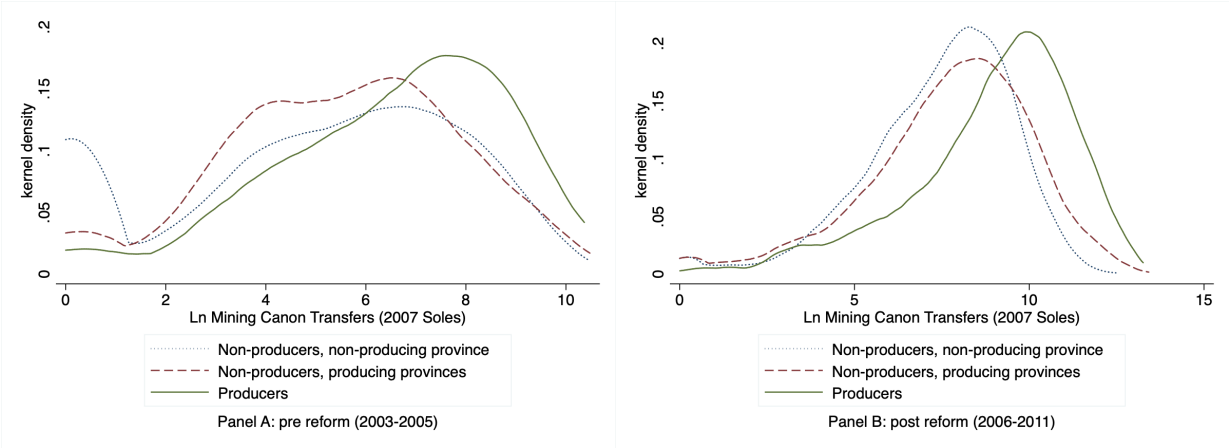
Figure 3.41: Local governments Mining Canon transfers as a share of revenues, by municipalities' mining production status. Peru, 2003 -2017



Source: own calculations, using MEF (2020c) and RENAMU (2020).

Note: Municipalities and provinces are producers if they had 1+ tax paying mining facility in year 2004.

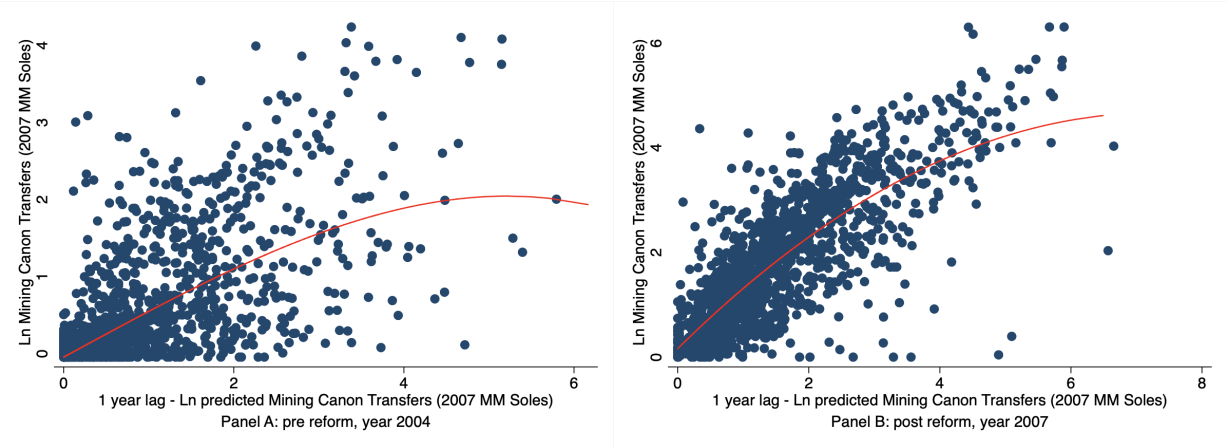
Figure 3.42: Kernel densities of Mining Canon Transfers by mining production status. Peru, 2003-2011



Source: own calculations, using MEF (2020c).

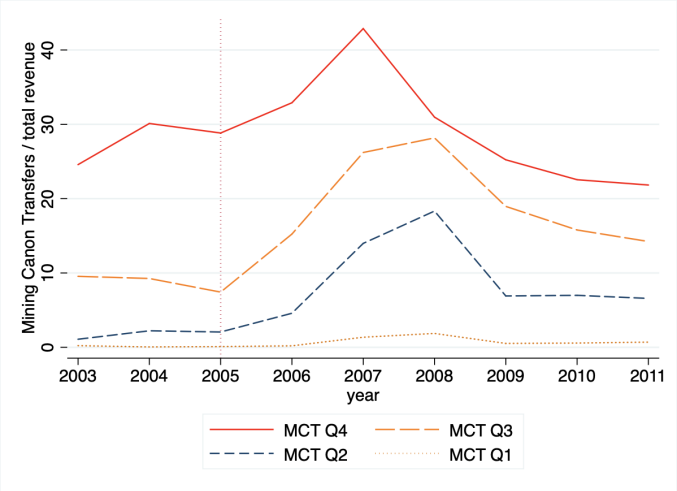
Note: Municipalities and provinces are producers if they had 1+ tax paying mining facility in year 2004.

Figure 3.43: Mining Canon Transfers, actual and predicted. Peru, 2004 & 2007



Source: own calculations, using MEF (2020c) and MINEM (2020).

Figure 3.44: Local governments Mining Canon transfers as a share of total revenue, by predicted transfers quartiles. Peru, 2003-2011



Source: own calculations, using MEF (2020c) and MINEM (2020).
Note: Transfer quartiles correspond to year 2004.

3.C Local public services, infrastructure and expenditure variables description

Trash collection, daily average: categorial variable that provides the daily average, in metric tons (Tm), of municipalities residential trash collection. The categories, as reported in RENAMU, are as follows: i) No trash is picked up; ii) 0-1 Tm; iii) 1-3 Tm; iv) 3-9 Tm ; v) 9-50 Tm; vi) 50-100 Tm; and vii) 100+ Tm. *Source*: RENAMU (2020)

Trash collection, frequency: categorial variable that reports the frequency with which residential trash collection is provided. The categories, as reported in RENAMU, are as follows: i) No trash is picked up; ii) Weekly pickup; iii) Every other day pickup; iv) Daily pickup. *Source*: RENAMU (2020)

Served waters treatment: served waters are liquids that contain human, animal or vegetable waste. This includes those originating from industrial activity. In RENAMU's questionnaire, this variable asks if these waters were treated, either with biological filter, imhoff tank, septic tank, anaerobic lagoons, among others. *Source*: RENAMU (2020)

Capital investments: expenditures on pre-investment studies, construction work execution, and machinery and equipment acquisitions. Importantly, these are non-financial investments (financial capital investments are a separate budget category). *Source*: MEF (2021).

Current expenditure: any payment done to produce goods and services. This includes wages and social benefits, consumption goods expenditure, operating expenses, etc. *Source*: MEF (2021).

Debt service: capital or interest payments, as well as commissions and any other amounts that ought to be periodically cancelled, as per the debts' terms and conditions. *Source*: MEF (2021).

3.D Additional tables

Table 3.9: Summary statistics, public employees by contract type and occupation. Peru, 2003-2005 and 2006-2008 averages.

	Variable	Mean	Std. Dev.	Mean	Std. Dev.
		Producers			
		2003-2005		2006-2008	
	Public employees (total)	37.82	68	73.18	147.36
Contract type	Appointed employees	16.86	47.04	15.89	41.23
	Contractors	0.17	1.47	3.57	15.45
	Permanent contract	11.32	34.36	24.9	106.46
	Temporary contract	10.56	18.69	30.25	69.98
Occupation	Directors & officials	2.98	5.56	6.81	19.17
	Professionals	3.94	5.86	11.23	23.23
	Technicians & auxiliary	17.58	28.83	32.66	65.75
	security guards, janitors & others	14.41	38.48	23.91	70.73
		Non-Producers, producing provinces			
		2003-2005		2006-2008	
	Public employees (total)	20.1	40.32	34.01	75.01
Contract type	Appointed employees	5.65	21.12	7.63	37.78
	Contractors	0.14	1.27	2.4	16.42
	Permanent contract	6.73	18.98	8.79	24.93
	Temporary contract	7.97	14.44	16.16	35.87
Occupation	Directors & officials	1.58	4.87	3.28	10.72
	Professionals	3.2	7.04	5.53	10.99
	Technicians & auxiliary	8.82	17.86	14.59	33.52
	security guards, janitors & others	6.26	15.15	11.59	36.61
		Non-Producers, non-producing provinces			
		2003-2005		2006-2008	
	Public employees (total)	26.71	69.78	40.93	88.10
Contract type	Appointed employees	9.41	40.52	9.75	37.58
	Contractors	0.56	11.87	3.16	23.51
	Permanent contract	6.88	16.06	8.89	21.74
	Temporary contract	10.55	26.36	19.28	46.06
Occupation	Directors & officials	2.16	7.26	3.84	12.48
	Professionals	4.03	8.39	6.59	13.48
	Technicians & auxiliary employees	11.94	28.77	18.07	38.55
	security guards, janitors & others	9.27	37.72	12.57	40.05

Note: The number of yearly observations per groups is: Producers = 65; Non-Producers in producing provinces = 408; and Non-Producers in non-producing provinces = 844.

Table 3.10: Pre-post comparisons and t-test for equality of means, by municipalities' mining production status. Peru, 2003&2007

		Producers	Non-producers		Mean difference		
			PP	NPP			
		(1)	(2)	(3)	(1)-(2)	(1)-(3)	(2)-(3)
2003	Public Employment	30.5	18.0	16.0	12.2	14.2	1.94
		(6.4)	(1.7)	(0.8)	(4.9)	(3.27)	(1.64)
					[2.48]	[4.32]	[1.18]
	Canon transfers	1,979	1,975	1,245	3.42	734	731
		(494)	(350)	(120)	(909)	(441)	(303)
					[0.00]	[1.66]	[2.40]
	Canon transfers (per capita)	0.27	0.32	0.32	-0.04	-0.05	-0.01
		(0.06)	(0.04)	(0.05)	(0.10)	(0.15)	(0.06)
					[−0.43]	[−0.36]	[−0.14]
2007	Public Employment	56.4	32.4	28.3	25.0	28.1	4.07
		(12.1)	(2.9)	(1.3)	(8.81)	(5.82)	(2.79)
					[2.83]	[4.83]	[1.5]
	Canon transfers	92,116	26,871	10,728	65,245	81,388	16,143
		(34,899)	(3,325)	(742)	(16,051)	(10,091)	(2,594)
					[4.07]	[8.07]	[6.22]
	Canon transfers (per capita)	8.94	5.96	2.32	2.98	6.63	3.65
		(2.50)	(0.67)	(0.15)	(1.89)	(0.88)	(0.50)
					[1.58]	[7.60]	[7.25]

Note: Columns (1) to (3) report averages for each group of municipalities, according to their mining production status in year 2004. The right hand side panel, reports the mean difference between groups, together with the corresponding t-test (between brackets) for the null hypothesis that the means difference is zero. Standard errors of the mean in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.11: Pre-post comparisons and t-test for equality of means, by predicted Mining Canon transfer quartiles. Peru, 2003&2007

		Quartiles				Mean difference		
		(1)	(2)	(3)	(4)	(1)-(2)	(2)-(3)	(3)-(4)
2003	Public Employment	13.79	9.28	19.87	43.44	4.50	-10.59	-23.57
		(2.45)	(0.706)	(1.55)	(4.91)	(2.55)	(1.71)	(5.15)
						[1.76]	[−6.2]	[−4.56]
						[1.20]	[−3.22]	[−2.49]
	Canon transfers	24.21	91.19	949	5072	-66.99	-857.99	-4124
		(22.89)	(22.98)	(167)	(560)	(32.43)	(168.8)	(584.6)
						[−2.06]	[−5.08]	[−7.05]
	Canon transfers (%revenue)	0.12	1.07	6.81	18.01	-0.95	-5.74	-11.20
		(0.10)	(0.20)	(0.73)	(0.98)	(0.227)	(0.753)	(1.22)
						[−4.18]	[−7.62]	[9.19]
2006	Public Employment	16.39	13.53	30.85	64.42	2.86	-17.32	-33.57
		(2.19)	(1.04)	(2.24)	(5.73)	(2.42)	(2.47)	(6.15)
						[1.18]	[−7.02]	[−5.45]
	Canon transfers	54.78	422.78	3,354	23,607	-368.00	-2,931	-20,252
		(7.38)	(28.67)	(328)	(3,132)	(29.61)	(329)	(3,15)
						[−12.43]	[−8.89]	[−6.43]
	Canon transfers (%revenue)	0.56	4.86	17.72	32.43	-4.30	-12.86	-14.72
		(0.08)	(0.36)	(0.89)	(0.89)	(0.37)	(0.96)	(1.26)
						[−11.71]	[−13.41]	[−11.71]

Note: Columns (1) to (4) report averages for each group of municipalities, according to their predicted Mining Canon transfer quartiles in year 2004. The right hand side panel, reports the mean difference between groups, together with the corresponding t-test (between brackets). Standard errors of the mean in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.E Additional results

3.E.1 DID I: mining production status

Table 3.12: Blue collar employees variations, by mining producer status. Peru, 2003-211

	(1)	(2)	(3)	(4)	(5)	(6)
	Security guards		Cleaning workers		Others	
Producer * Post	0.169**	0.074**	0.094	0.077*	-0.117	-0.030
	(0.08)	(0.03)	(0.07)	(0.04)	(0.08)	(0.04)
NP.PP * Post	-0.010	0.054***	0.036	0.065***	-0.006	0.037
	(0.05)	(0.02)	(0.04)	(0.02)	(0.06)	(0.03)
NP.NPP * Post	0.072*	0.037**	0.089**	0.035*	-0.031	-0.013
	(0.04)	(0.02)	(0.04)	(0.02)	(0.05)	(0.03)
Ln employment	T	P.C.	T	P.C.	T	P.C.
Municipality & year dummies	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.232	0.157	0.060	0.056	0.014	0.015

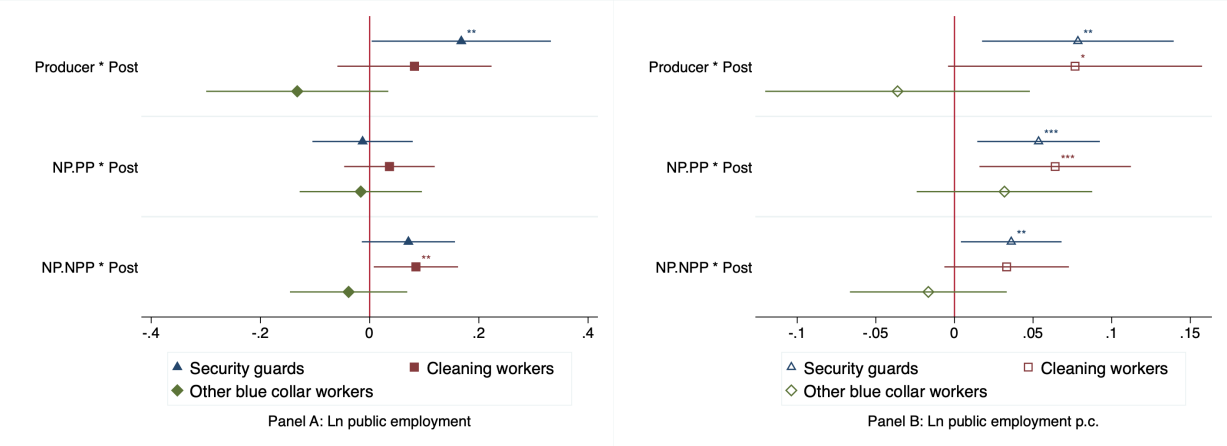
Note: N=14,200. The dependent variable is the Ln of public employment (T.) and the Ln of public employment per capita (P.C.) for each occupational category. Standard errors are clustered by municipality (in parentheses). Production status corresponds to year 2004. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.13: Public employees occupation variation, by mining producer status. Peru, 2003-211

	(1)	(2)	(3)	(4)	(5)	(6)
	Directors & Officials		Professionals		Technicians & Auxiliary	
Producer * Post	0.242***	0.130***	0.089	0.052	0.020	0.031
	(0.08)	(0.04)	(0.08)	(0.04)	(0.06)	(0.04)
NP.PP * Post	0.183***	0.246***	-0.065	-0.011	-0.013	0.020
	(0.04)	(0.02)	(0.04)	(0.02)	(0.03)	(0.02)
NP.NPP * Post	0.254***	0.170***	-0.065*	-0.022	0.029	0.031
	(0.04)	(0.02)	(0.04)	(0.02)	(0.03)	(0.02)
Ln employment	T	P.C.	T	P.C.	T	P.C.
Municipality & year dummies	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.477	0.415	0.075	0.075	0.180	0.166

Note: N=14,200. The dependent variable is the Ln of public employment (T.) and the Ln of public employment per capita (P.C.) for each occupational category. Standard errors are clustered by municipality (in parentheses). Production status corresponds to year 2004. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 3.45: Blue collar employees variations, by mining producer status and without municipality fixed effects. Peru, 2003-2011



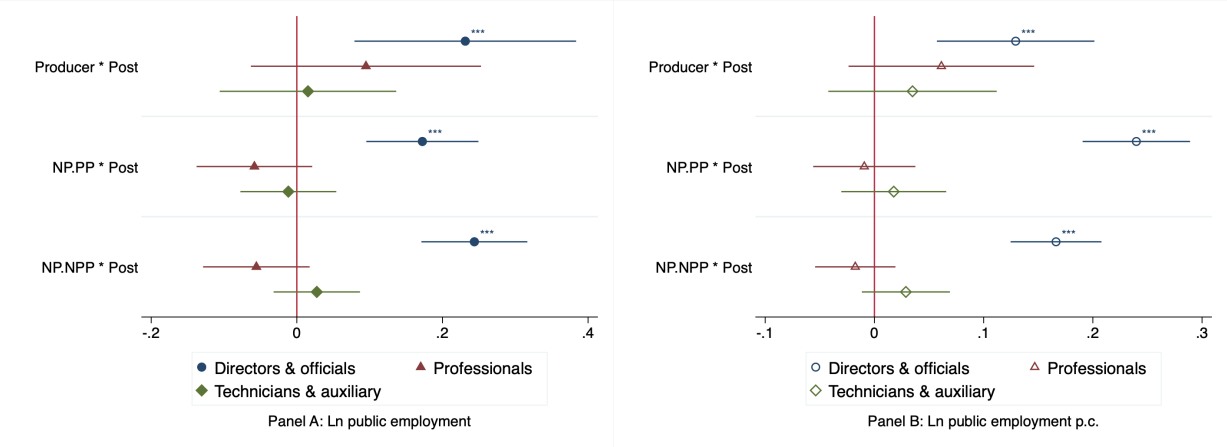
Note: N=14,200. All regressions have municipality clustered standard errors (horizontal lines), province and year dummies, and control for total revenue (2003), population share with 1+ NBI (1993), population (2003), quintiles of the population share with indigenous languages as a mother tongue (1993), municipality area (Km^2), latitude and longitude. Production status corresponds to year 2004. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.14: Public employees contract type variations, by mining producer status. Peru, 2003-2011

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Appointed		Permanent		Temporary		Contractors	
Producer * Post	-0.029	-0.021	0.071	0.028	0.253**	0.162***	0.069	0.036
	(0.07)	(0.04)	(0.09)	(0.05)	(0.11)	(0.06)	(0.05)	(0.03)
NP.PP * Post	0.063*	0.090***	0.012	0.097***	-0.068	0.006	-0.021	0.073***
	(0.03)	(0.02)	(0.05)	(0.03)	(0.06)	(0.03)	(0.03)	(0.02)
NP.NPP * Post	0.046	0.031	0.023	0.058**	-0.021	0.011	0.044	0.054***
	(0.03)	(0.02)	(0.04)	(0.02)	(0.05)	(0.03)	(0.03)	(0.01)
Ln public employment	T	P.C.	T	P.C.	T	P.C.	T	P.C.
Municipality & year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.288	0.268	0.089	0.099	0.133	0.136	0.503	0.467

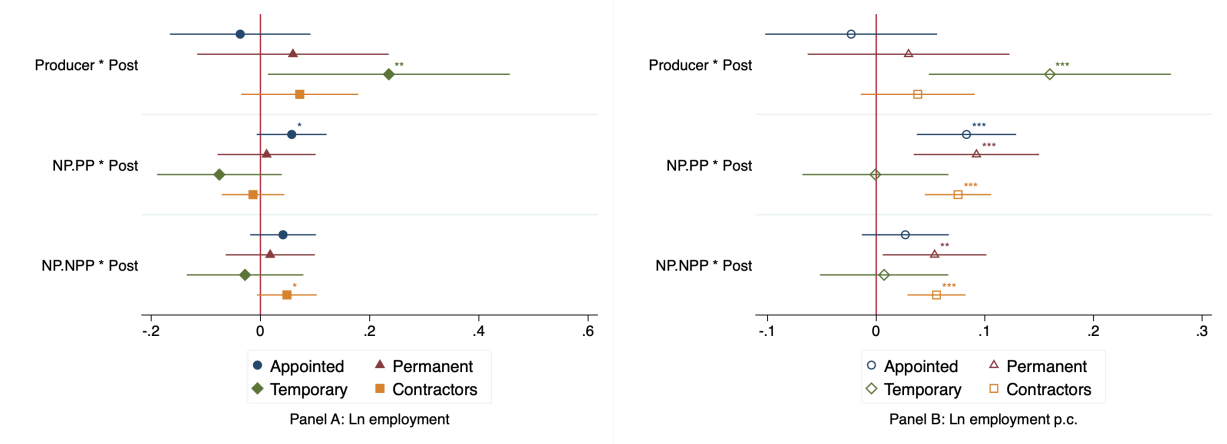
Note: N=14,200. The dependent variable is the Ln of public employment (T.) and the Ln of public employment per capita (P.C.) for each of the contract type category. Standard errors are clustered by municipality (in parentheses). Production status corresponds to year 2004. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 3.46: Public employment composition, by occupation. DID regression by mining producer status, without municipality fixed effects. Peru, 2003-2011



Note: N=14,200. All regressions have municipality clustered standard errors (horizontal lines), province and year dummies, and control for total revenue (2003), population share with 1+ NBI (1993), population (2003), quintiles of the population share with indigenous languages as a mother tongue (1993), municipality area (Km^2), latitude and longitude. Production status corresponds to year 2004. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 3.47: Public employment composition, by contract type. DID regression by mining producer status, without municipality fixed effects. Peru, 2003-2011



Note: N=14,200. All regressions have municipality clustered standard errors (horizontal lines), province and year dummies, and control for total revenue (2003), population share with 1+ NBI (1993), population (2003), quintiles of the population share with indigenous languages as a mother tongue (1993), municipality area (Km^2), latitude and longitude. Production status corresponds to year 2004. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.15: Municipal public employment variations, by mining producer status and with inequality interactions. Peru, 2003-2011

	(1)	(2)	(3)	(4)
Producer * Post	0.209	0.108	0.213	0.110
	(0.23)	(0.17)	(0.23)	(0.17)
NP.PP * Post	0.216***	0.213***	0.213***	0.211***
	(0.06)	(0.06)	(0.06)	(0.06)
NP.NPP * Post	0.179***	0.140***	0.176***	0.138***
	(0.06)	(0.05)	(0.06)	(0.05)
Producer * Post * Ineq. Q5	-0.059	0.036	-0.066	0.032
	(0.25)	(0.19)	(0.25)	(0.19)
Producer * Post * Ineq. Q4	0.024	0.113	0.016	0.109
	(0.25)	(0.19)	(0.25)	(0.19)
Producer * Post * Ineq. Q3	-0.078	0.038	-0.085	0.034
	(0.24)	(0.18)	(0.24)	(0.19)
Producer * Post * Ineq. Q2	-0.165	-0.062	-0.171	-0.065
	(0.26)	(0.21)	(0.26)	(0.21)
NP.PP * Post * Ineq. Q5	-0.137	-0.096	-0.135	-0.094
	(0.09)	(0.07)	(0.09)	(0.07)
NP.PP * Post * Ineq. Q4	-0.056	-0.051	-0.056	-0.051
	(0.08)	(0.07)	(0.08)	(0.07)
NP.PP * Post * Ineq. Q3	-0.204**	-0.167**	-0.201**	-0.165**
	(0.08)	(0.08)	(0.08)	(0.08)
NP.PP * Post * Ineq. Q2	-0.161*	-0.101	-0.160*	-0.101
	(0.09)	(0.08)	(0.09)	(0.08)
NP.NPP * Post * Ineq. Q5	0.005	0.044	0.007	0.045
	(0.08)	(0.07)	(0.08)	(0.07)
NP.NPP * Post * Ineq. Q4	-0.032	0.003	-0.031	0.004
	(0.08)	(0.07)	(0.08)	(0.07)
NP.NPP * Post * Ineq. Q3	-0.177**	-0.105	-0.176**	-0.104
	(0.07)	(0.06)	(0.07)	(0.07)
NP.NPP * Post * Ineq. Q2	-0.139*	-0.099	-0.136*	-0.097
	(0.08)	(0.07)	(0.08)	(0.07)
Ln public employment	T	P.C.	T	P.C.
Municipality dummies	Yes	Yes	Yes	Yes
Year dummies	No	No	Yes	Yes
R^2	0.333	0.305	0.467	0.444

Note: N=14,200. The dependent variable is the Ln of public employment (T.) and the Ln of public employment per capita (P.C.). All regressions include i) producer, NP.PP, NP.NPP and post reform dummies, ii) Post*Inequality quintile dummies; iii) Producer*InequalityQ, NP.PP*InequalityQ and NP.NPP*InequalityQ dummies; and iv) municipality clustered standard errors (in parenthesis). Production status corresponds to year 2004. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

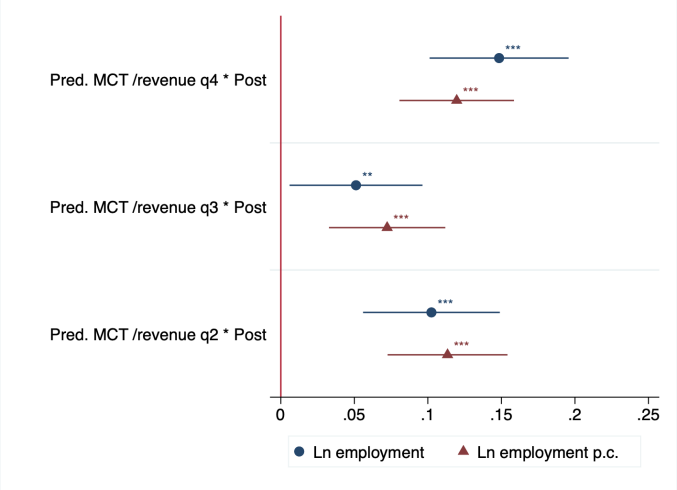
3.E.2 DID II: predicted Mining Canon transfers

Table 3.16: Municipal public employment variations, by predicted Mining Canon transfers quartiles. Peru, 2003-2011

	(1)	(2)	(3)	(4)
	Mining Canon Transfers		Mining Canon T. / total revenue	
Predicted MCT q4 * Post	0.147***	0.097***	0.148***	0.120***
	(0.02)	(0.02)	(0.02)	(0.02)
Predicted MCT q3 * Post	0.117***	0.106***	0.051**	0.072***
	(0.02)	(0.02)	(0.02)	(0.02)
Predicted MCT q2 * Post	0.062***	0.096***	0.102***	0.113***
	(0.02)	(0.02)	(0.02)	(0.02)
Ln employment	Total	P.C.	Total	P.C.
R^2	0.468	0.442	0.468	0.443

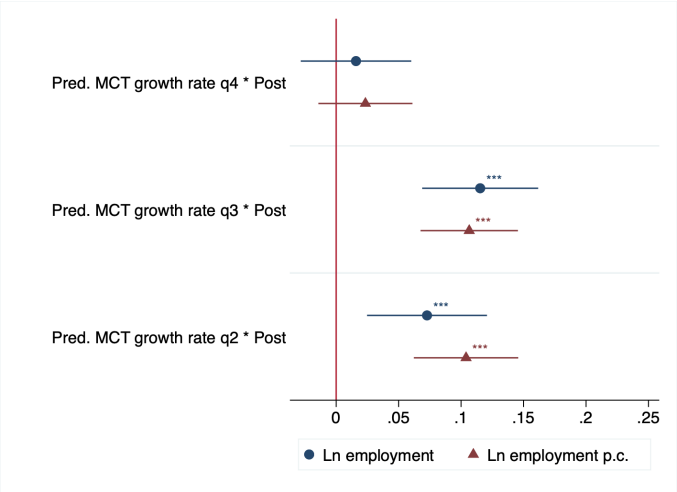
Note: N=14,200. All regressions have municipality and year dummies, as well as municipality clustered standard errors (horizontal lines). Predicted MCT quartiles correspond to year 2004 $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 3.48: Municipal public employment variations, by predicted Mining Canon transfers as a share of total revenues. Peru, 2003-2011



Note: N=14,200. All regressions have municipality and year dummies, as well as municipality clustered standard errors (horizontal lines). Predicted MCT quartiles correspond to year 2004. Results are available in appendix table 3.16. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 3.49: Municipal public employment variations, by predicted Mining Canon transfers growth rate quartiles. Peru, 2003-2011



Note: N=14,200. Mining canon growth rates are the difference between the predicted MCTs in year 2004 (pre shock) and those obtained post shock (year 2007). All regressions have municipality and year dummies, as well as municipality clustered standard errors (horizontal lines). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.17: Blue collar employees variations, by predicted Mining Canon transfers quartiles. Peru, 2003-211

	(1)	(2)	(3)	(4)	(5)	(6)
	Security guards		Cleaning workers		Others	
Predicted MCT q4 * Post	0.356***	0.102***	0.086**	0.023	0.014	-0.006
	(0.05)	(0.02)	(0.04)	(0.02)	(0.05)	(0.02)
Predicted MCT q3 * Post	0.092**	0.064***	0.138***	0.062***	0.028	0.018
	(0.04)	(0.02)	(0.04)	(0.02)	(0.05)	(0.03)
Predicted MCT q2 * Post	-0.086**	0.025	0.032	0.050**	0.022	0.055*
	(0.04)	(0.02)	(0.03)	(0.02)	(0.05)	(0.03)
Ln employment	T	P.C.	T	P.C.	T	P.C.
R^2	0.196	0.161	0.060	0.056	0.013	0.015

Note: N=14,200. The dependent variable is the Ln of public employment (T.) and the Ln of public employment per capita (P.C.) for each occupational category. All regressions have municipality and year dummies, as well as municipality clustered standard errors (horizontal lines). Predicted MCT quartiles correspond to year 2004. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.18: Public employees occupation variation, by predicted Mining Canon transfers quartiles. Peru, 2003-2011

	(1)	(2)	(3)	(4)	(5)	(6)
	Directors & Officials		Professionals		Technicians & Auxiliary	
Predicted MCT q4 * Post	0.260***	0.006	0.142***	0.070***	0.135***	0.054***
	(0.04)	(0.02)	(0.04)	(0.02)	(0.03)	(0.02)
Predicted MCT q3 * Post	0.205***	0.093***	-0.025	0.029	0.028	0.025
	(0.04)	(0.02)	(0.04)	(0.02)	(0.03)	(0.02)
Predicted MCT q2 * Post	0.112***	0.207***	-0.041	0.018	-0.038	-0.000
	(0.03)	(0.03)	(0.04)	(0.02)	(0.03)	(0.02)
Ln employment	T	P.C.	T	P.C.	T	P.C.
R^2	0.478	0.416	0.077	0.076	0.182	0.166

Note: N=14,200. All regressions have municipality and year dummies, as well as municipality clustered standard errors (horizontal lines). Predicted MCT quartiles correspond to year 2004. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.19: Public employees contract type variations, by predicted Mining Canon transfers quartiles. Peru, 2003-2011

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Appointed		Permanent		Temporary		Contractors	
Predicted MCT q4 * Post	-0.015	-0.081***	0.008	-0.030	0.306***	0.161***	0.165***	0.024*
	(0.03)	(0.02)	(0.05)	(0.03)	(0.06)	(0.03)	(0.03)	(0.01)
Predicted MCT q3 * Post	0.039	-0.005	-0.006	0.006	0.028	0.072**	0.049*	0.051***
	(0.03)	(0.02)	(0.05)	(0.03)	(0.05)	(0.03)	(0.03)	(0.01)
Predicted MCT q2 * Post	0.045	0.083***	-0.037	0.035	-0.068	0.019	-0.035	0.067***
	(0.03)	(0.02)	(0.04)	(0.03)	(0.05)	(0.03)	(0.02)	(0.02)
Ln employment	T	P.C.	T	P.C.	T	P.C.	T	P.C.
R^2 0.288	0.272	0.089	0.098	0.139	0.139	0.505	0.468	

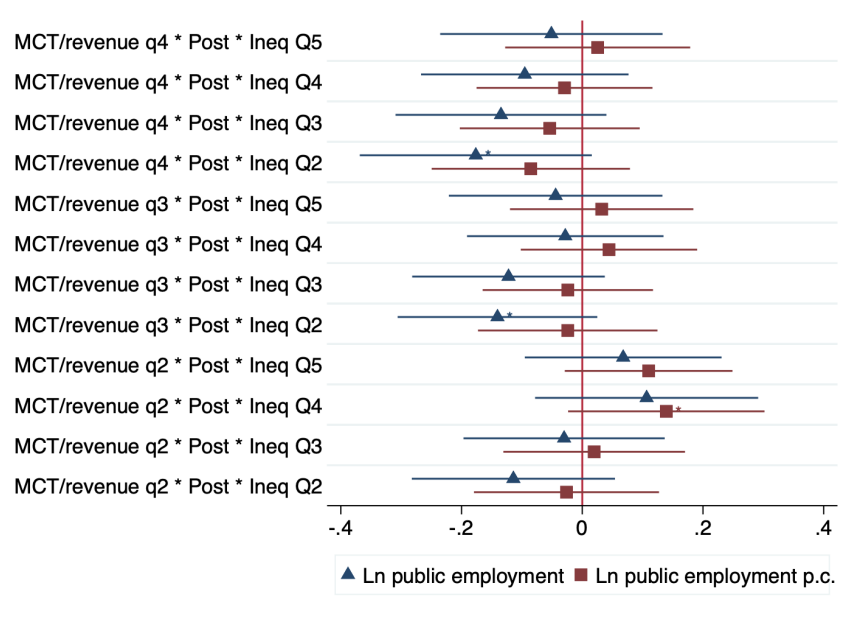
Note: N=14,200. All regressions have municipality and year dummies, as well as municipality clustered standard errors (horizontal lines). Predicted MCT quartiles correspond to year 2004. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.20: Municipal public employment variations, by Mining Canon transfers quartiles and with inequality interactions. Peru, 2003-2011

	(1)	(2)	(3)	(4)
	Mining Canon Transfers		Mining Canon T. / total revenue	
Pred. MCT q4 * Post * Ineq. Q5	-0.076	0.017	-0.076	0.017
	(0.09)	(0.07)	(0.09)	(0.07)
Pred. MCT q4 * Post * Ineq. Q4	-0.068	0.011	-0.066	0.012
	(0.08)	(0.07)	(0.08)	(0.07)
Pred. MCT q4 * Post * Ineq. Q3	-0.065	0.003	-0.065	0.003
	(0.08)	(0.07)	(0.08)	(0.07)
Pred. MCT q4 * Post * Ineq. Q2	-0.115	-0.065	-0.113	-0.064
	(0.10)	(0.08)	(0.10)	(0.08)
Pred. MCT q3 * Post * Ineq. Q5	0.030	0.110*	0.030	0.110
	(0.08)	(0.07)	(0.08)	(0.07)
Pred. MCT q3 * Post * Ineq. Q4	-0.019	0.046	-0.019	0.046
	(0.07)	(0.07)	(0.07)	(0.07)
Pred. MCT q3 * Post * Ineq. Q3	-0.132*	-0.050	-0.131*	-0.049
	(0.07)	(0.06)	(0.07)	(0.06)
Pred. MCT q3 * Post * Ineq. Q2	-0.110	-0.034	-0.109	-0.034
	(0.08)	(0.07)	(0.08)	(0.07)
Pred. MCT q2 * Post * Ineq. Q5	-0.122	-0.028	-0.119	-0.026
	(0.07)	(0.06)	(0.07)	(0.06)
Pred. MCT q2 * Post * Ineq. Q4	-0.068	0.028	-0.067	0.029
	(0.08)	(0.07)	(0.08)	(0.07)
Pred. MCT q2 * Post * Ineq. Q3	-0.177**	-0.051	-0.175**	-0.049
	(0.07)	(0.07)	(0.07)	(0.07)
Pred. MCT q2 * Post * Ineq. Q2	-0.177***	-0.093	-0.176***	-0.092
	(0.07)	(0.06)	(0.07)	(0.06)
Ln employment	Total	P.C.	Total	P.C.
R^2	0.335	0.305	0.469	0.444

Note: N=14,200. All regressions have i) MCT quartiles and post reform dummies, ii) Post*Inequality quintile dummies; iii) MCT quartiles*Inequality quintiles dummies; iv) municipality and year dummies, as well as municipality clustered standard errors (horizontal lines). Predicted MCT quartiles correspond to year 2004. $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 3.50: Municipal public employment variations, by predicted Mining Canon transfers revenue shares, and with inequality interactions. Peru, 2003-2011

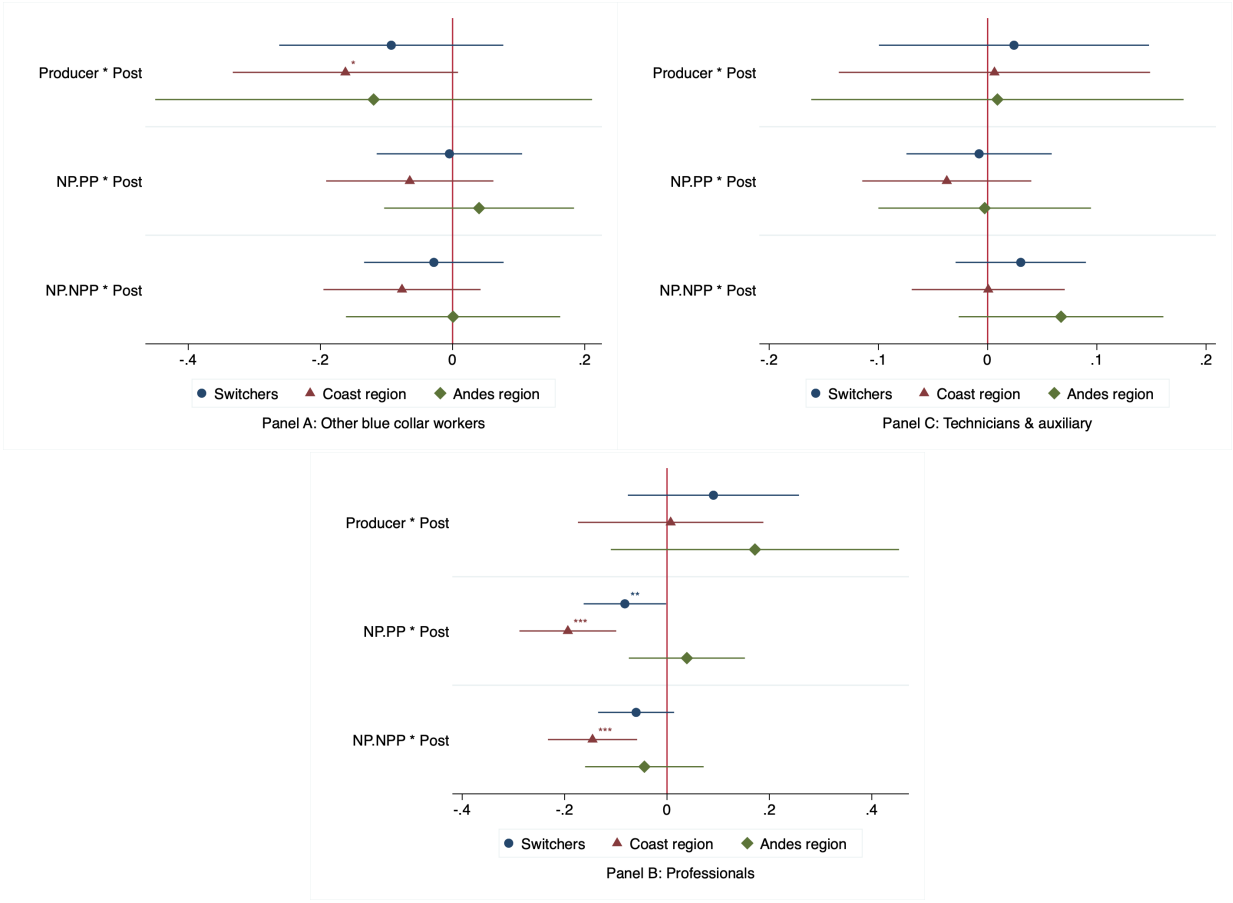


Note: N=14,200. All regressions have i) MCT quartiles and post reform dummies, ii) Post*Inequality quintile dummies; iii) MCT quartiles*Inequality quintiles dummies; iv) municipality and year dummies, as well as municipality clustered standard errors (horizontal lines). Predicted Mining Canon transfers over total revenues correspond to year 2004. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.E.3 Additional robustness test

3.E.3.1 Sample changes

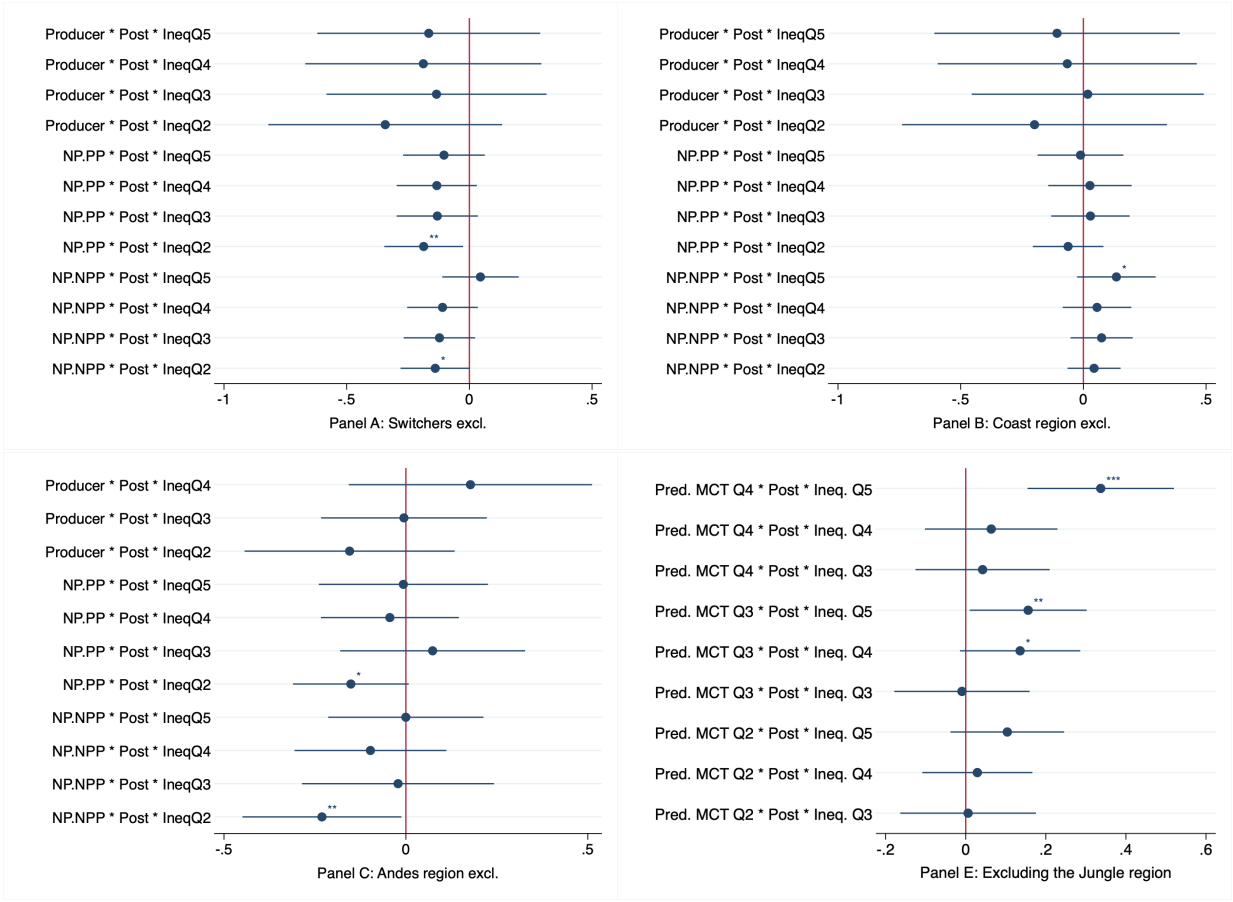
Figure 3.51: Workers occupations changes (H2), by mining producer status. Sample exclusions. Peru, 2003-2011



Note: Sample sizes are: N=13,812 excluding switchers, N=10,922 excluding the Coast, and N=5,016 excluding the Andes. All regressions have year and municipality dummies, as well as municipality clustered standard errors (horizontal lines). Production status corresponds to year 2004. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.E.3.2 Production status year changes

Figure 3.52: Municipal public employment variations (H4), by mining producer status and inequality quintiles. Sample exclussions. Peru, 2003-2011



Note: Sample sizes are: N=13,812 excluding switchers, N=10,922 excluding the Coast, and N=5,016 excluding the Andes. All regressions have year and municipality dummies, as well as municipality clustered standard errors (horizontal lines). Production status corresponds to year 2004. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.21: Workers occupation changes (H2), by mining producer status. Changes in treatment year. Peru, 2003-2011

Ln public	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
employment	Security guards			Cleaning staff			Directors & officials		
Producer * Post	0.211**	0.167**	0.226**	0.089	0.099	0.121	0.243***	0.244***	0.251***
	(0.08)	(0.08)	(0.09)	(0.07)	(0.07)	(0.07)	(0.08)	(0.08)	(0.08)
NP.PP * Post	-0.007	-0.015	0.002	0.046	0.037	0.047	0.185***	0.178***	0.169***
	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
NP.NPP * Post	0.055	0.064	0.048	0.084**	0.090**	0.082**	0.244***	0.250***	0.240***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Status year	2003	2004	2005	2003	2004	2005	2003	2004	2005
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dep. & year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.317	0.317	0.317	0.449	0.448	0.448	0.544	0.544	0.544

Note: N=14,200. All regressions control for total revenue (2003), population share with 1+ NBI (1993), population (2003), population share with indigenous languages as a mother tongue (1993), municipality area (Km^2), latitude and longitude, and include municipality clustered standard errors (in parentheses). Results with other blue collar workers, professionals and technicians & auxiliary workers are available in appendix table 3.22. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.22: Worker occupation changes (H2), by mining producer status. Changes in production status year. Peru, 2003-2011

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Worker type	Other blue collar			Professionals			Technicians & auxiliaries		
Producer * Post	-0.082	-0.122	-0.113	0.119	0.106	0.136	0.019	0.041	0.065
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.06)	(0.06)	(0.06)
NP.PP * Post	-0.031	-0.016	-0.013	-0.055	-0.060	-0.040	0.012	-0.006	0.006
	(0.06)	(0.06)	(0.06)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)
NP.NPP * Post	-0.032	-0.036	-0.037	-0.054	-0.051	-0.063*	0.031	0.040	0.031
	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)
Status year	2003	2004	2005	2003	2004	2005	2003	2004	2005
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dep. & year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.182	0.182	0.182	0.352	0.351	0.352	0.645	0.645	0.645

Note: N=14,200. All regressions control for total revenue (2003), population share with 1+ NBI (1993), population (2003), population share with indigenous languages as a mother tongue (1993), municipality area (Km^2), latitude and longitude, and include municipality clustered standard errors (in parentheses). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.23: Contract type variations (H3), by mining producer status. Changes in treatment year. Peru, 2003-2011

Ln public employment	Temporary contract (1)	(2)	(3)	Appointed contract (4)	(5)	(6)
Producer * Post	0.232**	0.255**	0.312***	0.011	-0.011	-0.012
	(0.12)	(0.11)	(0.11)	(0.07)	(0.06)	(0.07)
NP.PP * Post	-0.079	-0.077	-0.056	0.066**	0.063*	0.064*
	(0.06)	(0.06)	(0.06)	(0.03)	(0.03)	(0.03)
NP.NPP * Post	-0.023	-0.025	-0.041	0.050	0.053*	0.053*
	(0.05)	(0.05)	(0.05)	(0.03)	(0.03)	(0.03)
Production status year	2003	2004	2005	2003	2004	2005
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Dep. & year effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.334	0.335	0.335	0.632	0.632	0.633

Note: N=14,200. All regressions control for total revenue (2003), population share with 1+ NBI (1993), population (2003), population share with indigenous languages as a mother tongue (1993), municipality area (Km^2), latitude and longitude, and include municipality clustered standard errors (in parentheses). Results with permanent and contractor contracts are available in appendix table 3.24. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.24: Contract type variations (H3), by mining producer status. Changes in production status year. Peru, 2003-2011

Ln public	(1)	(2)	(3)	(4)	(5)	(6)
employment	Permanent contract			Contractor contract		
Producer * Post	0.096	0.082	0.076	0.076	0.075	0.091
	(0.09)	(0.09)	(0.09)	(0.06)	(0.05)	(0.06)
NP.PP * Post	0.040	0.019	0.011	-0.005	-0.014	-0.010
	(0.05)	(0.05)	(0.05)	(0.03)	(0.03)	(0.03)
NP.NPP * Post	0.018	0.029	0.033	0.041	0.048*	0.042
	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)
Status year	2003	2004	2005	2003	2004	2005
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Dep. & year effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.446	0.446	0.446	0.486	0.486	0.486

Note: N=14,200. All regressions control for total revenue (2003), population share with 1+ NBI (1993), population (2003), population share with indigenous languages as a mother tongue (1993), municipality area (Km^2), latitude and longitude, and include municipality clustered standard errors (in parentheses). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

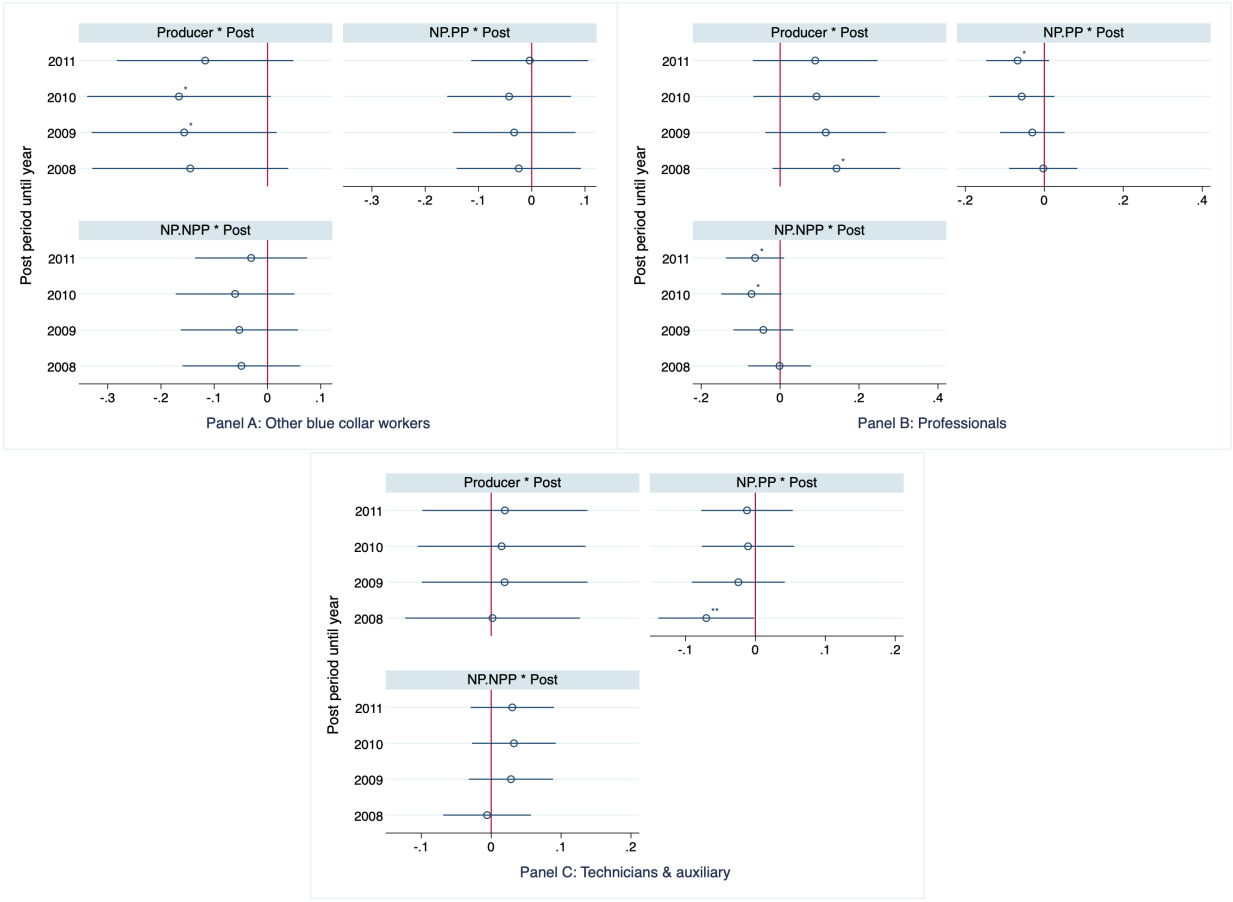
3.E.3.3 Post period end year

Table 3.25: Municipal public employment per capita variations, by mining producer status. Changes in end of post-period. Peru, 2003-2011

Ln public employment p.c.	(1)	(2)	(3)	(4)
	Post period ends in year			
	2011	2010	2009	2008
Producer * Post	0.121***	0.113**	0.112**	0.090**
	(0.04)	(0.04)	(0.04)	(0.05)
NP.PP * Post	0.098***	0.091***	0.085***	0.047**
	(0.02)	(0.02)	(0.02)	(0.02)
NP.NPP * Post	0.078***	0.068***	0.070***	0.043**
	(0.02)	(0.02)	(0.02)	(0.02)
N	14200	12600	11028	9452
Controls	Yes	Yes	Yes	Yes
Dep. & year effects	Yes	Yes	Yes	Yes
R^2	0.535	0.517	0.512	0.482

Note: All regressions control for total revenue (2003), population share with 1+ NBI (1993), population (2003), population share with indigenous languages as a mother tongue (1993), municipality area (Km^2), latitude and longitude, and include municipality clustered standard errors (in parentheses). Production status corresponds to year 2004. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 3.53: Workers occupation changes (H2), by mining producer status. Changes in end of post-period. Peru, 2003-2011

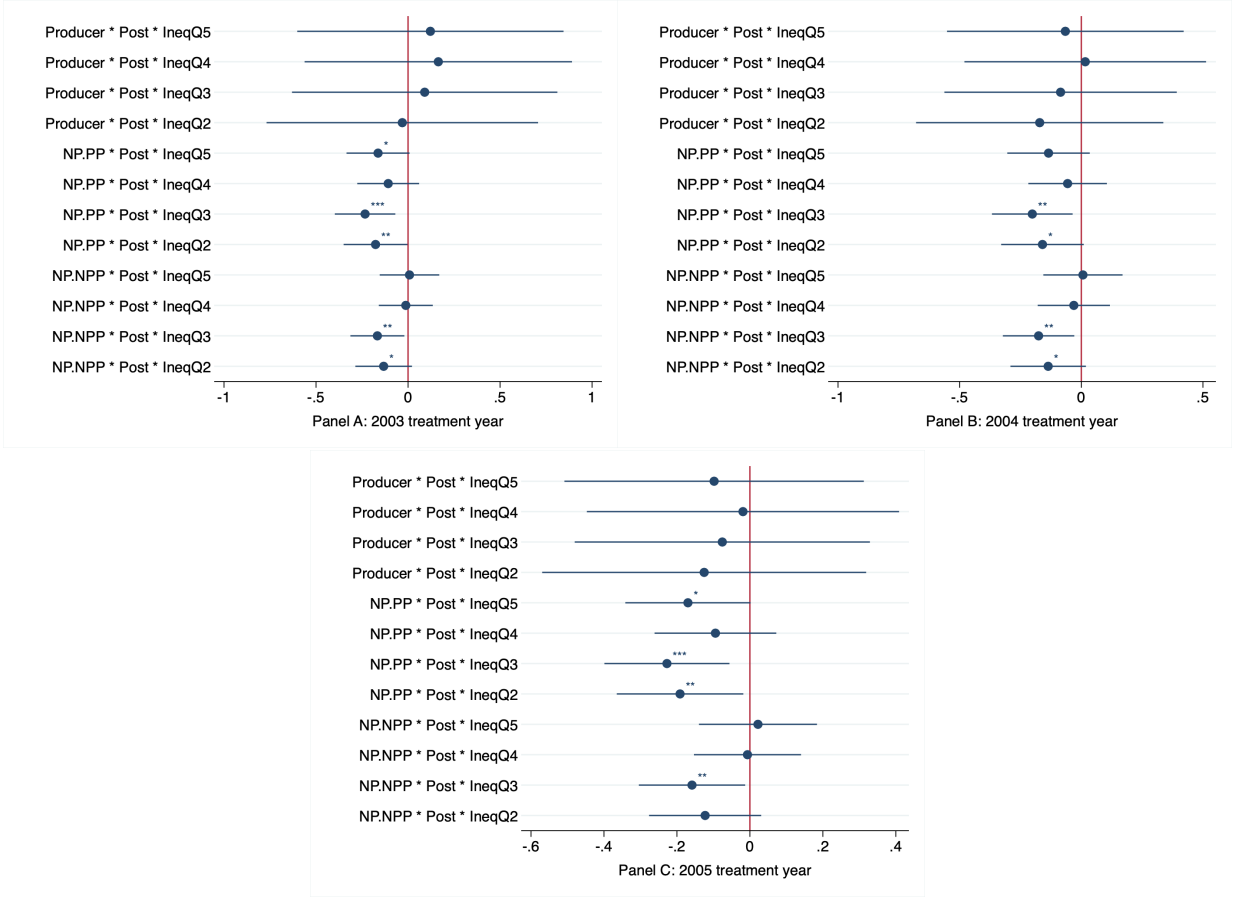


Note: All regressions have municipality clustered standard errors (horizontal lines), department and year dummies, and control for total revenue (2003), population share with 1+ NBI(1993), population (2003), quintiles of the population share with indigenous languages as a mother tongue (1993), municipality area (Km^2), latitude and longitude. Production status corresponds to year 2004. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.E.3.4 Robustness tests for H4

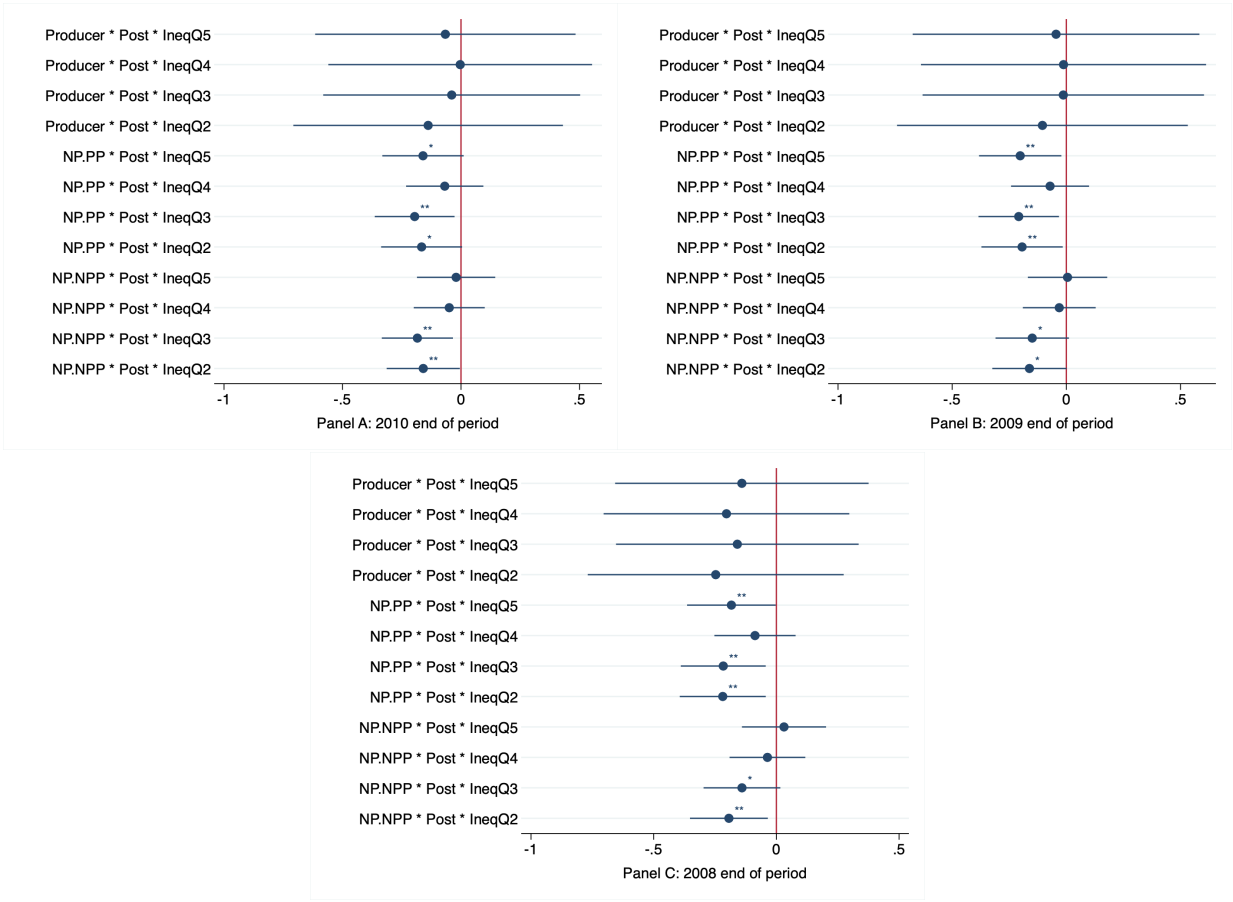
With regards to my hypothesis on the interaction between inequality and mining rents, the main results (that is, the lack of relationship between these) are robust to each of the tests performed in this section. The estimations are essentially equal if we alter the treatment year definition (from 2004 to 2003 or 2005, figure 3.54), when successively reducing the post period end year from 2011 to 2008 (figure 3.55), and under different sample exclusions (figure 3.56).

Figure 3.54: Municipal public employment variations, by mining producer status and inequality quintiles (H4). Treatment year changes. Peru, 2003-2011



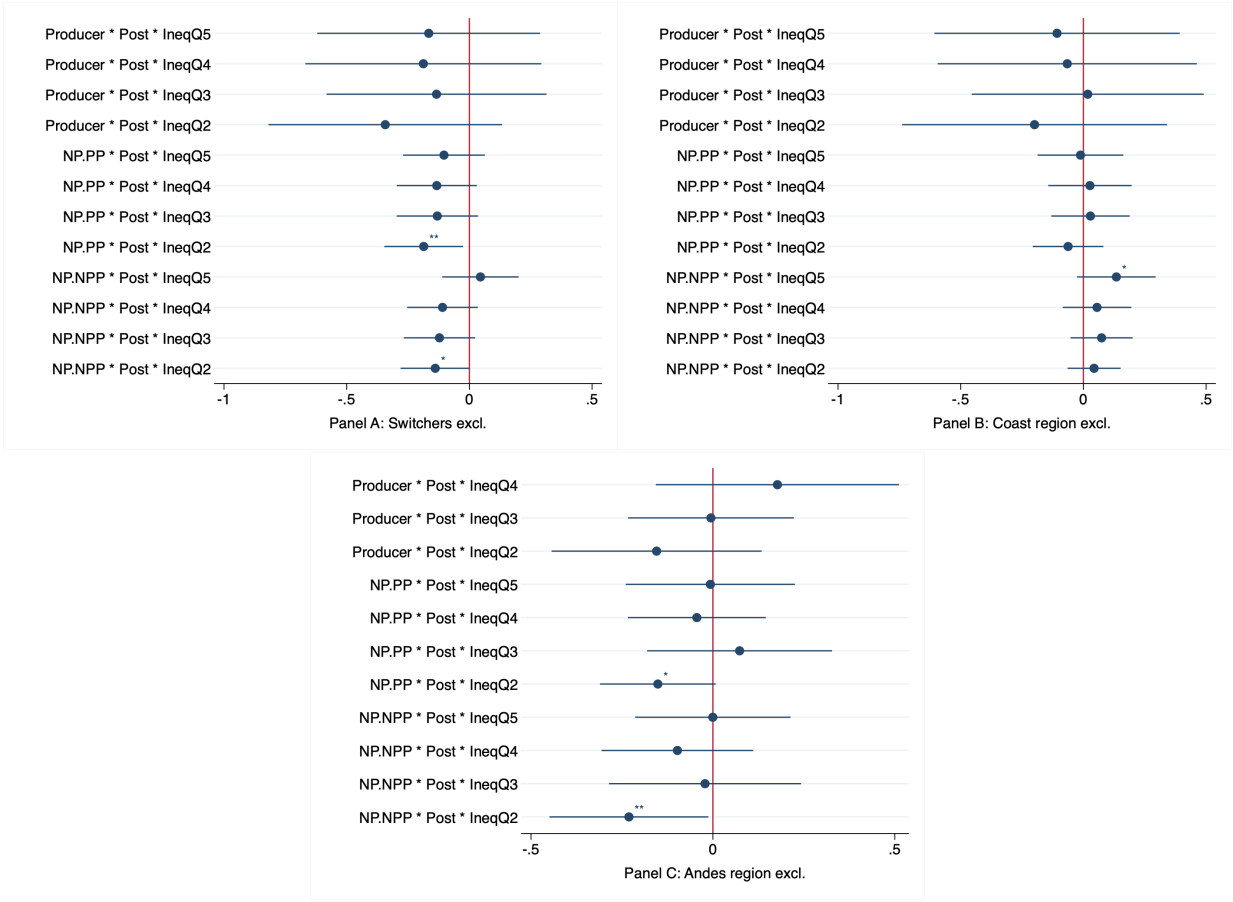
Note: N=14,200. All regressions have year and municipality dummies, as well as municipality clustered standard errors (horizontal lines). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 3.55: Municipal public employment variations (H4), by predicted Mining Canon transfers and inequality. Shortening of post-period end year. Peru, 2003-2011



Note: N=14,200. All regressions have year and municipality dummies, as well as municipality clustered standard errors (horizontal lines). Production status corresponds to year 2004. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 3.56: Municipal public employment variations, by mining producer status and inequality quintiles (H4). Sample exclusions. Peru, 2003-2011



Note: Sample sizes are: N=13,812 excluding switchers, N=10,922 excluding the Coast, and N=5,016 excluding the Andes. All regressions have year and municipality dummies, as well as municipality clustered standard errors (horizontal lines). Production status corresponds to year 2004. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.E.4 Local public services and infrastructure provision

Table 3.26: Infrastructure construction costs (Ln), by mining production status. Peru, 2004-2011.

Sector	(1) Transport	(2) Water, sewages & electricity	(3) Education	(4) Health	(5) Others	(6) Total
Post reform	0.733***	1.067***	0.141	0.066	-0.190	1.086***
	(0.13)	(0.13)	(0.13)	(0.11)	(0.13)	(0.11)
Producer * Post	0.715**	0.500*	0.508**	0.060	1.073***	0.754***
	(0.29)	(0.27)	(0.24)	(0.25)	(0.28)	(0.23)
NP.PP * Post	-0.084	-0.105	0.130	0.069	0.575***	0.041
	(0.15)	(0.14)	(0.14)	(0.11)	(0.14)	(0.13)
NP.NPP * Post	-0.177	-0.037	0.007	0.048	0.320**	-0.069
	(0.13)	(0.13)	(0.13)	(0.10)	(0.13)	(0.11)

Note: N=12,622. Amounts are in (Ln) of thousands of 2007 Soles. The regressions include municipality and year fixed effects, as well as municipality clustered standard errors (in parentheses). Production status corresponds to year 2004. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

Chapter 4: Why is reforming clientelistic public services challenging in resource-rich countries? A literature review

4.1 Overview

Countries with high natural resource rents are at particular risk of having clientelistic public services — with public sector jobs being awarded in exchange for political support, rather than based on merit. In Saudi Arabia, for example, 96 percent of Saudi nationals worked in the public sector in year 2015[1]. In the also resource rich Indonesia, the large influx of fiscal resources over the last 15 years drastically raised the number of teachers, creating one of the lowest student-teacher ratios in the developing world (Pierskalla and Sacks, 2020). Despite this, Indonesia still fares rather poorly in terms of educational outcomes[2]. In both countries, clientelism is likely the origin of an important share of those public sector jobs.

According to the Varieties of Democracy (V-Dem) expert assessments, between 2000 and 2018, public servants were *usually* appointed for personal or political reasons in 65 percent of resource-rich countries - a significantly higher share than for those with medium wealth (33 percent) and resource-poor (39 percent) countries.[3] In turn, clientelistic public services, as opposed

[1] Own calculations, based on statistics by the General Authority for Statistics of the Kingdom of Saudi Arabia (2015).

[2] Indonesia ranked 64 of the 65 countries that participated in the 2012 Program for International Student Assessment's internationally standardized tests of math and science (Chang et al., 2013).

[3] Own calculations, based on the V-Dem 2021 dataset (Coppedge et al., 2021a). Resource richness is a self-built

to meritocratic ones, have been shown to be associated with poorer development outcomes, such as worse revenue and expenditure management, and less foreign investment, growth and human development.

Why is promoting meritocracy in the public services particularly difficult in resource rich countries? To my knowledge, there is no uniform body of knowledge that *directly* answers this question. Consequently, this essay presents a review of the theoretical and empirical literature on public service reform. It identifies which political and economic contextual factors help explain public service reform trajectories generally, and in resource-rich settings particularly.

This literature review builds on the findings from my two previous empirical essays, which yield empirical support to the argument that natural resource rents increase the size of *clientelistic* public employment. I find that in democracies resource-wealth is consistently associated with larger public employment - in terms of the total number of employees (chapter 2). Both my research and that of others[4] sustain that these extra public employees yield no proportional increase in public goods and services delivery (chapter 3). This indicates that such extra employment is likely *clientelistic, rather than "productive"*: politicians provide public jobs to a selected group of people in exchange of political support. Resource rich countries hence seem more prone to have bloated and inefficient bureaucracies than less resource wealthy peers.

The present essay complements my two preceding quantitative essays with a **comprehensive literature review aimed at understanding how a resource-rich context influences public service reform likeliness**, as a mean to manage clientelistic public employment. After defining terminology (section 4.2), I proceed in three steps. First, I review the theoretical and empirical

variable, which measures the average share of natural resource rents over GDP, as per the WDI (2020). This is below 1.9 percent for resource spoor countries (quartiles 1 and 2), and below 9.5 percent for those with medium resource richness (quartile 3).

[4] Caselli and Michaels (2013); Larrain and Perello (2019); Monteiro and Ferraz (2012)

literature on public service (and management) reforms. This serves to identify contextual factors that help explain public service reform trajectories *generally, in any context* (section 4.3).[5] Second, I use theory, empirical research and data to identify (i) whether these factors are more prevalent in resource-rich than in resource-poor countries (section 4.4) and (ii) if they can hence explain more pronounced clientelism in resource-rich contexts (section 4.5). The points made in sections 4.4 and 4.5 are largely absent in the literature. Finally, section 4.6 concludes.

This review yields unsurprising findings: resource-richness, is, on average, associated with factors that make clientelistic public employment more likely, such as lower productivity, less democratic institutions and higher violent conflict probabilities, prevalence of non-programmatic political parties and more pronounced political budget cycles. This, in turn, makes meritocratic public service reform less likely in resource rich countries than in those without them.

The essay makes a small contribution to the literature on public service reform by reviewing it in view of its salience for resource-rich countries. While there is a well-developed body of knowledge on public service reform trajectories in developed countries[6], evidence is less consolidated for developing countries[7] and fairly limited for resource rich ones.

4.2 Clientelism, public service reform and resource-wealth

4.2.1 Clientelistic public services, merit and reform

The review in this paper focuses on reforms affecting the *"public service"* (PS). The PS is a group of civilian government personnel, employed under a distinctive public service em-

[5] Closely following Schuster (2016) categories, with additions.
[6] See Pollitt and Bouckaert (2017)
[7] Bertelli et al. (2020); Schuster (2016)

ployment regime which differs from that of the private sector[8]. The PS contains the ***(core) civil service*** (hereto "CS"), a group of employees at the central and subnational level, and includes those working in ministries, departments and agencies - either managing daily activities, or developing and implementing government policies and programs (Blum et al., 2019). Besides the CS, the PS also comprises service delivery agents, such teachers, health workers and the police, who typically represent the largest portion of public employment (Rao, 2013).

Importantly, the PS represents only a share of the complete public workforce[9]. It does not include the following groups of public employees: i) the military, (ii) elected officers, ministers, or political appointees[10]; (iii) State Owned Enterprises (SOE) employees, nor (iv) workers outside of government payroll, such a contractual and temporary staff, and "volunteers". These groups do not usually fall under the distinctive PS employment regime, and data on their size and remuneration is often unreliable or lacking.

A *clientelistic public service* is one in which public service jobs are predominantly awarded in exchange for political support. More generally, clientelism is a phenomenon by which politicians distribute private goods in exchange for votes or political support (Stokes, 2009). In this paper, as in my dissertation overall, I focus exclusively on ***clientelistic public service employment***, and leave aside other types of private goods that could also be used as rewards in clientelistic exchanges. My narrow usage of the term coincides with the definition of ***patronage***, which is simply the exchange of public sector jobs for political support (Hicken, 2011)[11]. I will here-

[8]PS employment is usually governed by a Public Service law. Unlike other public sector workers, public servants are in the government's official payroll and hold distinctive permanent-like employment status. (Blum et al., 2019)

[9]Figure 4.11 (appendix 4.A) provides a more detailed picture of the structure of public employment.

[10]Different administrative traditions give more room for political appointments within the public sector, resulting in a heated discussion on the trade-off between building capacity and performance (often associated to meritocracy), and political responsiveness (see Box 1).

[11] As per Hicken (2011), patronage is one specific type of clientelist exchange.

after use the terms "clientelistic public service employment" and "patronage" interchangeably, as others often do (i.e., Kitschelt and Wilkinson (2007)).

It is less straightforward to define the opposite of a clientelistic PS, and the objective of a ***"public service reform"***. This is primarily because public services serve different, and conflicting objectives, as detailed further below (see Box 1). Protecting ***"merit"*** - where bureaucrats are recruited on the base of qualifications and can advance in the organization as per predictable and objective criteria (Cornell et al., 2020) - is typically seen as crucial for building capable and performing public services. Protecting "merit" can, however, come at the expense of other objectives, such as (i) ensuring that the PS is responsive to political priorities, (ii) that its composition is representative of the broader population or of (iii) ensuring political stability.

For this paper, I rely on "merit" (defined above) as a clear and simple opposite of "clientelistic public service employment" or "patronage". When I talk about ***"public service reforms"***, this hence references reform activities aimed at ***strengthening merit*** in the PS [12]. Intuitively, one expects that eliminating much of the discretional hiring should improve public services delivery. Under this logic, most of public sector personnel policies are defined precisely to isolate employment decisions from political criteria (Finan et al., 2015).

[12]Meritocracy is only a subset of broader PS characteristics commonly referred to as "Weberian". Weberian PS systems emphasize a stark separation between appointed public administrators and (politics and) elected politicians (McCourt, 2013). Weberian reforms, hence, replace discretionary power in the areas of i) recruitment (via competitive exams) and promotion; ii) clear wage progressions; and iii) job stability, granting tenure to avoid politically motivated dismissals (Schuster, 2016).

Box 1: Public services' conflicting objectives

Public services operate under varied, often conflicting, objectives, and current institutional arrangements reflect which values are prioritized. In most cases, PS institutional choices serve the following broad objectives: i) building capacity and performance (often associated to meritocratic practices); ii) granting political stability; iii) being responsive to political priorities; and iv) mirroring the population, via representativeness and inclusiveness. A fundamental tradeoff is that meritocracy does not always guarantee capability and performance (objective i), and it may also be at odds with objectives (ii)-(iv) above. In fact, patronage can also enhance performance, while perhaps ensuring objectives (ii)-(iv).

Patronage can and has been used by reform-oriented leaders to quickly bring in highly qualified technocrats, hence pushing for policy reform and strengthening government performance[a]. Furthermore, patronage can guarantee political stability and governability, and increasingly does so as a country becomes more unstable. For example, in post-conflict states political elites often hand out public jobs to political supporters and former combatants, as a mean to ensure that these will not return to combat (Blum and Rogger, 2020). Despite these potential advantages, patronage can be rather "capricious" (Grindle, 2012), so it is no guarantee for long term success.

An important feature of public services is that they are **often used for political stability purposes** (objective ii). As per the "politician's dilemma" (Geddes, 1994), political elites usually prioritize political survival vis-à-vis long-term objectives, which they achieve through the coalitions formed via patronage. This, in turn, implies that public service institution design often responds to politician's need of political stability. Service delivery could also ensure the population's support (hence, survival); however, this strategy is unfeasible in places with low public services delivery capability (Blum and Rogger, 2020).

Civil servants **responsiveness** to elected officials policy agendas (objective iii) can be an important way to be responsible to the citizens electing such officials (Hood and Peters, 2004). Recently, developed countries like the United States (US) and United Kingdom (UK) are strongly pushing for greater flexibility in hiring and removing public employees, claiming that traditional public service rules turn bureaucracies too unresponsive to citizens' needs (Moreira and Pérez, 2021).[b]

Finally, there is the question of whether **public employment should be representative and inclusive** (objective iv). For example, in countries with unequal access to education, health, and jobs, both by gender and ethnic group: Should new appointments be purely based on credentials, even if this excludes women and most ethnicities (but the economically/politically dominant one)? This ethical and political conundrum can be, and often is, at odds with objectives (i)-(iii).

[a] For instance, Grindle (2012) has extensively document this phenomenon in Brazil and Mexico's cases.

[b] See Ujhelyi (2014) for recent US legislation to traditional PS protections, and Cabinet Office (2012) the UK's case.

This narrow definition has several key advantages. For starters, (1) "merit" stands in clear

contraposition to "clientelistic PS employment" or "patronage", the practice of exchanging public sector jobs for political support. Horn (1995), for instance, claims that the "distinguishing feature of the merit system" is to restrict politicians' control over their administrative agents (the PS). Furthermore, merit, narrowly defined (2), avoids a risk flagged by Schuster (2016): the PS reform literature can show contradictory findings because it bundles up, under the term "Weberianism"[13], weakly correlated PS reform strategies[14].

Most importantly, there is strong evidence that (3) meritocratic reforms are associated with improved development outcomes. On average, merit-based civil services are associated with outcomes as diverse as improved bureaucratic performance (Rauch and Evans, 2000), reduced corruption[15], higher tax revenue mobilization and better public finance management[16], and in general more democratic stability[17].

I further narrow the notions of "merit" and "PS reform" to reference the **adoption** of such reforms, **rather than their actual implementation**. This choice primarily reflects the state of the literature, which predominantly discusses when PS reform is more likely to *begin*; however, there is currently no consensus on which factors predict its *implementation success*[18]. Furthermore, actual reform implementation is hard to observe empirically. While necessary for practical reasons, this choice is problematic, since a significant strand of the literature highlights that PS reforms tend to get undermined in implementation, once adopted[19].

[13] While the essence of Weberian reforms is to promote meritocracy, Weberian systems also promote tenure and depoliticized pay in the public sector - and, as Schuster (2016) explains, these need not be correlated to one another.

[14] In the PS reform literature, reform is often equated to "Weberianness". However, the distinct features of Weberian systems (merit, pay structures and tenure) can be weakly correlated between each other (Schuster, 2016).

[15] Charron et al. (2017); Dahlstrom et al. (2012).

[16] Arezki et al. (2012); Xu (2018).

[17] Cornell and Lapuente (2012); Oliveros and Schuster (2018).

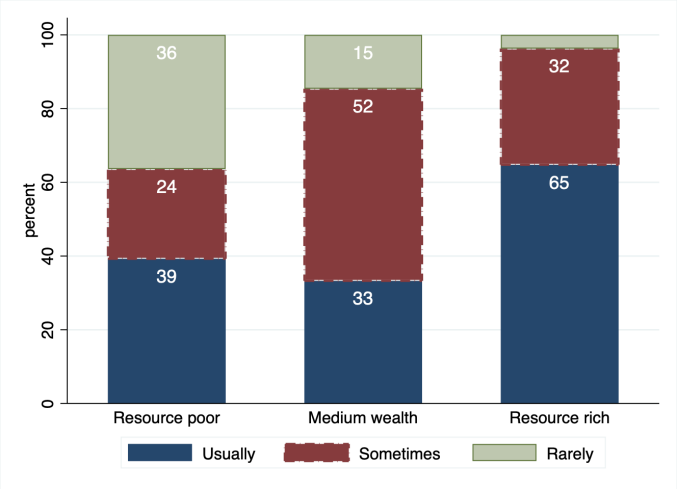
[18] Kurtz (2013); Moreira and Pérez (2021).

[19] Andrews (2013b); Blum et al. (2019); Grindle (2012).

4.2.2 Clientelistic public services in resource rich countries

This literature review is motivated by the fact that **clientelist PS are more prevalent in resource-rich countries**[20] than in others. Several sources show this association empirically. As per the Varieties of Democracy (V-Dem) expert assessments, in 65 percent of resource-rich countries, public administration officers were *usually* appointed for personal or political reasons (Figure 4.1). By contrast, this number ranges between 33 and 39 percent for the medium and resource-poor countries.

Figure 4.1: Are public administration appointments based on personal or political criteria? By natural resource rents level. Years 2000-2018.



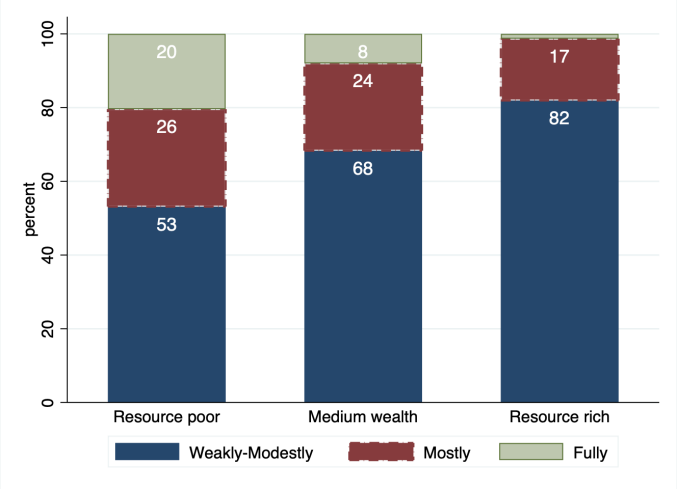
Source: own elaboration, based on the V-Dem 2021 dataset (Coppedge et al., 2021a) and WDI (2020).
Note: Resource richness measures the average share of natural resource rents over GDP. This is below 1.9% for resource poor countries, and below 9.5% for medium wealth ones (quartiles 2 and 3, respectively).

Furthermore, bureaucrats are perceived as either weakly or modestly impartial in 82 percent

[20] I arbitrarily define resource rich countries as those were 9.5 percent or more of countries' GDP comes from natural resources rents. This is the lower boundary of the 4th quartile of resource rents over GDP in my first paper's sample. I classify the remaining countries as having medium resource riches if these rents are between 1.9 and 9.5 percent of their GDP (quartile 3), while the resource poor are those with rents below 1.9 percent of their GDP (quartiles 1 and 2).

of resource-rich countries (Figure 4.2), while for their medium and resource poor counterparts it would be 68 and 53 percent, respectively. Hence, the average citizen in resource rich countries likely faces a less capable and impartial bureaucracy than peers in resource poor countries.

Figure 4.2: Are public administration rigorous and impartial? By natural resource rents level. Years 2000-2018.

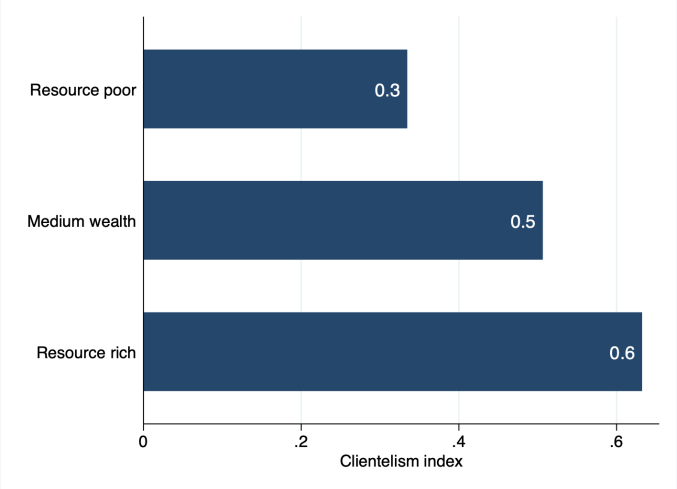


Source: own elaboration, based on the V-Dem 2021 dataset (Coppedge et al., 2021a) and WDI (2020).
Note: Resource richness measures the average share of natural resource rents over GDP. This is below 1.9% for resource poor countries, and below 9.5% for medium wealth ones (quartiles 2 and 3, respectively).

This finding is underpinned by the fact that resource-rich countries are generally more clientelistic than others, beyond the public service. Expert assessments from the V-Dem project indicate that, between years 2000 and 2018, clientelism was *twice as likely* in resource rich countries compared to resource poor ones (Figure 4.3). While not all clientelistic exchanges lead to patronage (the former is just a subset of the later), this joint evidence suggests that patronage strongly correlates with natural resource riches.

In my previous research, I observed a simultaneous growth of public employment and natural resources rents, consistent with my theoretical predictions that such public employment

Figure 4.3: Clientelism prevalence by natural resource rents level. Years 2000-2018.



Source: own elaboration, based on the V-Dem 2021 dataset (Coppedge et al., 2021a) and WDI (2020). *Note*: Resource richness measures the average share of natural resource rents over GDP. This is below 1.9% for resource poor countries, and below 9.5% for medium wealth ones (quartiles 2 and 3, respectively). The clientelism index ranges from 0 to 1, with higher scores indicating a normatively worse situation (more clientelism).

growth was partly driven by redistribution concerns and hence was potentially clientelistic. In my first paper, highly unequal resource-rich democracies showed on average 20 percent higher public employment than more equal countries in the lowest quintile of natural resource rents. In my second paper, mining producing municipalities in Peru hired 15 percent more public employees [21] after a sharp increase in mining revenues. Similar research by Monteiro and Ferraz (2012) and Caselli and Michaels (2013) (for Brazil) and Larrain and Perello (2019) (for Chile) report comparable findings, which they also attribute to clientelism. Consequently, while the empirical literature on the topic is limited, it confirms a positive association between patronage and natural resource rents.

This raises the question: Why is this the case? This is not fully surprising, given the

[21] Predominantly temporary workers, and either blue collar or political appointees.

natural resource curse literature's robust findings of the negative institutional consequences of resource riches, with worse governance, higher corruption and rent seeking among them (see section 4.4.1). Moving forward, I review the literature on the contextual factors affecting PS reform, to then discuss how this distinctively affects countries with high natural resources wealth.

4.3 How does context matter for public service reform?

Which contextual factors help explain public service reform trajectories *generally, in any context*? This section reviews the broader theoretical and empirical literature on PS reform, beyond resource rich settings. The objective is to identify i) which major contextual factors affect PS reform trajectories; and ii) through which mechanisms/ channels do they affect it.

Context dependency, and generally whether policy results from one setting can be easily extrapolated to another, is being hotly debated in the economics literature. Recent high level theoretical and empirical assessments on external validity and context include work by Deaton and Cartwright (2018), Ravallion (2020), Allcott (2015), Dehejia et al. (2021), Pritchett and Sandefur (2015), Banerjee et al. (2017) and Andrews and Oster (2019). The big picture message from this research is that policymakers who wish to learn about the potential impact of a given reform should assess the literature with a critical eye, focusing on differences in program design and context, rather than simply on impact size (Pritchett and Sandefur, 2015).

Vivalt (2020) studies 20 different types of international development interventions and finds a large heterogeneity in treatment effects for any given program type[22]. These differences are largely explained by contextual factors. The author observes that if context is ignored, "an in-

[22]Her sample was compiled via meta-analysis and includes 15,024 estimates from 635 papers on 20 types of interventions in international development.

ference about another study will have the correct *sign* about 61% of the time"(Vivalt, 2020, p. 3). Along the same lines, Gechter (2016) describes that (for microcredit programs) causal effects from one place may be only partially informative about effects elsewhere, and extrapolation possibilities largely depend on the two contexts being similar. Furthermore, Pritchett and Sandefur (2014) and Dehejia et al. (2021) find that economic and institutional level characteristics are on average more informative than individual level variables when it comes to extrapolating treatment effects. [23]

There are also a few recent efforts to systematically conceptualize how context matters for public sector management (PSM) reforms, of which PS reforms are a subset. These identify diverse socioeconomic and political-institutional factors which affect PSM reform likeliness and paths. For developed countries, Pollitt and Bouckaert (2017) highlight the importance of a country's dominant administrative culture – the system of values, beliefs and procedures defining what is "normal" or "acceptable" within a bureaucracy. The authors explain that PS are embedded in these cultures, and as such promote either i) highly legalistic and omnipresent states (the *Rechtsstaat* model[24]), ii) smaller bureaucracies where public officials are pragmatic and flexible, seeking the public's approval of policies designed for their general interest (the Anglo-Saxon "public interest" model[25]), or iii) other more moderate approaches in the middle [26].

[23] At least for the authors studies on i) class size effects and the gains from private schooling, and ii) fertility and labor supply, respectively.

[24] The *Rechtsstaat* model originates in Germany and considers the state as a central integrating force in society. Its main concern is to prepare, promulgate and enforce laws. "The typical values of this approach will include respect for the authority of the law as a socially necessary and integrating force, attention to precedent, and a concern with ... equality before the law." (Pollitt and Bouckaert, 2017, p. 60)

[25] The 'public interest' model is based upon Anglo-Saxon states culture (Australia, New Zealand, and the UK), and seeks to minimize the role of the state, which is seen as a necessary evil. To govern, bureaucrats are expected to constantly seek the public's consent on the policies designed for the public interest. "Fairness and independence ... are therefore key values, with pragmatism and flexibility as qualities which may be prized above technical expertise (or even above strict legality)."(Pollitt and Bouckaert, 2017, p. 61)

[26] For instance, the Continental European-Napoleonic (France, Italy, Greece, Portugal and Spain) and Nordic (Finland, Sweden, Norway and Denmark) systems. These differ from the Rechtsstaat model in the strength of

For developing countries, Bunse and Fritz (2012) review which factors can trigger or facilitate PSM reforms. The authors find that many governments do attempt to initiate PSM reform but tend to abandon it at the implementation stage because of their high political costs. In the same order of ideas, Blum (2014) studies a large sample of World Bank PSM projects, and finds that political context factors[27] have a greater impact on the PSM projects performance than it does for other types of WB projects.

On the particular issue of patronage reform, Schuster (2016) conducts a comprehensive review of 34 articles, and grossly categorizes context factors into socioeconomic (demands from organized interests and voters) and political-institutional explanations (electoral competition, party organization, political institutions and budget constraints). The author concludes that while studies identify numerous relevant contextual factors, scholars largely disagree on their importance, so one cannot expect "robust inferences about their explanatory weight, mechanisms and signs of effects". (Schuster, 2016, p. 1095)

Building on these previous efforts, and on newly reviewed literature, this paper examines the role of 8 contextual factors in PS reforms (summarized in table 4.1). I build upon Schuster (2016) broad categorization of socioeconomic and political-institutional factors, and expand his initial list to consider violent conflict, low productivity and the role of foreign donors and agencies. I will separately discuss how natural resources rents influence the context for PS reforms in section 4.4.

My findings come from assessing over 70 studies published since the 1990's, in a four-step literature search. First, I obtained a large set of papers from an online search with the

the state/society separation, the prominence of patronage, and whether the state is centralized or not. (Pollitt and Bouckaert, 2017)

[27] Whether regimes are democratic, political parties programmatic and countries more aid dependent.

Table 4.1: Overview. How does context matter for meritocratic public service (PS) reform?

Factor	Definition	PS reform likelihood	Conditional on	Mechanism
A. Political-institutional factors				
Democratic regime $*$	Democracy, per the polity IV score	Increases	-	When accountability matters, governments must cater to larger population shares to keep power. The larger this share, the less gains in offering public jobs in exchange of support.
Electoral competition	Small margin in electoral results	Decreases	Clientelism - common	Since programmatic politics are not credible, politicians increase patronage to keep office.
		Increases	Clientelism - uncommon	Politicians who fear being replaced promote merit to i) ensure that "friendly" bureaucrats and party appointees keep their post, and ii) block patronage possibilities to hostile successors.
Political budget cycles	Budget shifts in electoral years	Decreases	Democracy	In electoral years, public expenditure shifts to visible current expenditures (like wages), to increase electoral advantages
Programmatic political parties	Clear policy platforms and/or ideologies	Increases	Democracy	Performance enhancing reforms are credible and grant more political advantage than patronage.
Violent conflict	High probability of armed conflict	Decreases	-	Clientelism in the public service is key for peace keeping.
B. Socioeconomic factors				
Low productivity	Output per worker	Decreases	-	A small formal private sector offers less outside employment options to job seekers. Thus, they are more dependent on (clientelistic) PS jobs.
External pressures	High foreign aid intake	Increases	-	Donor conditionalities increase pressures to *initiate* reforms; however, these are often abandoned at the implementation stage.
Voters & organized interests	Voters and organized groups pressure	No effect	-	Socioeconomic changes that change voter preferences towards public goods usually do not promote patronage reform.

Note: $*$ Conversely, less democratic regimes (mixed regimes and autocracies) have lower PS reforms likelihood.

keywords ‘civil service’, ‘public service’, ‘patronage’, ‘downsizing’, ‘Weberian’ and ‘reform’. Second, I repeated the previous search while adding ‘public employment’ and ‘resource rich’ or each individual resource rich country name[28]. Third, I excluded studies which did not focus on patronage or clientelism more broadly[29], *causes* of reform (not outcomes nor processes), or that contained no substantive quantitative evidence. Finally, I reviewed the bibliographies of the selected papers, in search of other relevant ones.

4.3.1 Political and institutional factors

4.3.1.1 Theory

This section provides evidence on the broad political economy frameworks of PS reform. We observe that patronage emerges as a politically rational form of rent-seeking, which increases both the benefits of being in office and politicians short term survival probabilities. Because of the myriad of benefits it affords, patronage systems are very resilient and often survive and readapt formal reform attempts. The literature concludes that merit-based PS reform is a rare occurrence because it is not compatible with politicians’ short-term incentives. PS reform will only materialize when its short-term gains are outweighed by the long-term ones.

A. PS reforms are difficult

Despite their importance to successfully implement public policies, professionalized and meritocratic PS are far from being the norm, and most countries are somewhere in the range

[28] As previously mentioned, I define resource rich countries following my first paper’s criteria: countries for which 9.5 percent or more of their GDP comes from natural resources rents.

[29] I explicitly excluded papers on New Public Management, capacity building, decentralization and central management bodies reforms.

between fully meritocratic and entirely partisan bureaucracies[30]. This elucidates the fact that **reforming bureaucracies to eliminate patronage is an extremely difficult endeavor**, since: 1) patronage systems are very sticky (they confer many benefits to those in them), and 2) sometimes patronage also helps getting things done (by fighting the bureaucracy's rigidities).

CS and PS reforms are long and bumpy roads; they require a long-time horizon to succeed. Take the case of the United States (US). In 1883, the US Congress passed the Pendleton Civil Service Act, considered the first step towards a professionalized civil service in the US. This Act introduced competitive exams for the selection of certain federal government employees, such as those on charge of tax collection. According to Grindle (2012), the US federal government patronage system finally gave way to reform 50 years post Pendleton Act: it was only by the 1930's that the federal civil service incorporated about 80 percent of bureaucrats.

In the developing world, PS reforms attempts are frequent, but often stagnate or are only partially implemented (Bunse and Fritz, 2012). Between 2000 and 2006 only, the World Bank lent USD 422 million per year for 71 projects related to CS and administrative reform, but less than 45 percent of borrowers showed any improvement in the target areas. In particular, i) salary adjustments were not carried out; and ii) downsizing attempts either did not take place or were soon reversed (usually rehiring the same people)(Webb et al., 2008).

Experience shows that **patronage systems are "sticky", unyielding to reforms, because of the benefits they provide to those in them**. Electoral advantages are broadly considered the main reason behind patronage politics: giving public jobs away can ensure more votes for candidates (Robinson and Verdier, 2013) or be used to pay political favors (Grindle, 2012). Politically loyal public workers can feed the clientelistic cycle by using public services and procedures to

[30](Brierley, 2020; Grindle, 2012)

favor political supporters, be them voters, interest groups or elites (Geddes, 1994).

B. Political economy of redistributive politics

Bueno De Mesquita and Smith (2010) selectorate model explains that **policy choices are always made to promote the interests of elite groups** (the "selectorate"). Political leaders seek above all things to survive in office; hence, they choose the public and private goods provision that maximize their chances for staying in power. The main distinction between democracies and autocracies is the size of the elites relative to the population, with autocracies having small elites or influence groups. Autocrats can efficiently redistribute the rents from being in power through private goods (such as public jobs), as these can be easily targeted to a small elite. By contrast, politicians in democracies have incentives to provide public goods, which can effectively reach a large "elite". This, in turn, implies that democratic leaders have more incentives than autocrats to promote meritocratic PS reform, all else equal.

Robinson and Verdier (2013) specifically discuss the role of public employment (as a particular type of rent) in redistributive politics: **when politicians' stakes of being in power are high, so will clientelistic public employment**. Their model's key prediction is that politicians can maximize the expected returns from being in office when public goods investment is low, as this maximizes their expected rents from public employment. Less public goods, and less aggregate efficiency, make public employment offers more attractive to voters. This in turn increases voters' dependency on a paternalistic state, making it easier to continue buying their support with job offers.

C. Political economy of public service reform

The political economy literature that focuses on explaining PS reform is quite limited: Blum and Rogger (2020); Geddes (1994); Grindle (2012); Horn (1995); Robinson and Verdier (2013); Schuster (2016). Its main message is that incentive compatibility is required for reform: **patronage can only be reduced with reforms that are incentive-compatible for incumbent politicians** (and sometimes bureaucrats). In other words, meritocracy-based reforms go through *only* when their political costs are smaller than the electoral and governability benefits from patronage. This, nonetheless, requires the confluence of multiple factors, rather than simply a predominant one (Schuster, 2016).

Politicians have a conflict between immediate political survival and long-term prospects, and immediate political survival pressures usually prevail. According to Blum and Rogger (2020), historically, all public service reforms (or lack of) have been shaped by a major political trade-off: should public appointments be based on merit or patronage? Geddes (1994) famously coined the term *"politicians' dilemma"* to describe the straining conflict politicians face, between using public appointments either "as a political or economic investment" (Geddes, 1994, p.18).

This "dilemma" explains that **merit-based PS reform is a rare occurrence, because it is not compatible with politicians' short-term incentives**. When appointing an individual for office, politicians are confronted with their immediate need of political survival, often in conflict with longer-term collective interests. In the short run, politicians prioritize patronage appointments to those individuals whose support is key to remain in office (the "political investment"). However, to serve common long-term goals of economic development and public goods provision, forward looking politicians should instead appoint capable technocrats, based on merit (the

"economic investment").

Nonetheless, in some cases **PS design can be shaped by the wish to protect current laws, policies and appointments from future legislators and/or politicians** (Horn, 1995). Electoral competition generally means that current leaders will eventually be replaced by others with different policy preferences and interests. This, in turn, implies that future leaders may attempt to i) reduce the scope or alter the intended effects of previous laws, and/or ii) remove "sympathetic appointments in the public service". Hence, meritocratic PS systems come handy as a mean to restricts politicians' power over their administrative agents, protecting both the benefits of legislation and the appointment of capable bureaucrats.

Horn (1995) indicates that politicians will support merit based reforms to offer their constituents a credible and "durable solution to the problem of the 'corruption' inherent in patronage" (Horn, 1995, p.101). This, nonetheless, will only happen if the gains in support from those loosing from patronage are larger than loss in political support by those gaining from patronage. Hence, as Blum and Rogger (2020, p.17) put it, pushing for merit institutions becomes incentive compatible if *"political elites have long time horizons and significant pressure from a broad electorate"*. This, nonetheless, is not frequent.

Patronage systems are very resilient and often survive and readapt legislations for *formal* public service systems. According to Grindle (2012), there is a dissociation between legislation and implementation, the latter being what de facto shapes emergent civil services. The creation of stable and meritocratic PS, she explains, is forever ongoing, "constantly threatened by deconstruction and reconstruction" (Grindle, 2010, p.2). Hence, political actors often repeal legislations during the implementation period, via a plethora of "reform avoidance" strategies. The slow accumulation of these reform avoidance "legacies" is what ultimately determines PS

Table 4.2: Mechanisms for resisting implementation of PS systems

Resistance strategy	**Definition**
Opting out	Resist incorporation to a new PS regime (at the agency level).
Disestablishment	Legally eliminate the agency in charge of the PS system management.
Starvation	Reduce implementing agencies budgets, to stop them. Cancel out entry examinations, to cut recruitment opportunities.
Re-claiming	Decentralize personnel decisions to ministry or agency level, to avoid "system-wide" rules.
Re-definition	Legally reduce coverage of the PS system.
Re-engineering	Inventing ways to hire public officials outside the PS system regulation (i.e., via short contracts).

Source: Grindle (2010)

characteristics.

Grindle describes several mechanisms through which bureaucrats resist the implementation of central PS rules (see Table 4.2). The most common one is to hire workers outside the system's established categories, either through temporary contracts or by creating new positions that fall out of the PS legislation structures ("re-engineering"). Those resisting change can also kill the organization implementing the reform, either by legally elimitating it ("disestablishment"), or reducing their budget and opportunities to hold entry examinations for recruitment ("starvation"). In addition, they can also legally reduce the PS system coverage ("re-definition") or circunvent system-wide rules by decentralizing personnel decisions to ministry or agency level ("re-claiming"). Finally, some agencies manage to escape new public service regimes all together ("opting out"), possibly claiming their agency requires a special status, out of the system, to properly function.

However, despite the continuity of patronage, many PS have become increasingly more

professional, and patronage has at times been used "to infuse government with more professional and competent management." (Grindle, 2010, p.21). This has been the case even in highly politicized and clientelistic cases, such as Mexico and Brazil.

4.3.1.2 Political regimes: Democracies versus autocracies

In democracies, where accountability and popular support truly matter, governments must cater to larger population shares to remain in power. Targeted transfers (such as public employment) are relatively unattractive, because of the large size of the group whose support must be won; thus, rational politicians take advantage of the economies of scale of large public goods provision[31]. This, naturally, is generally consistent with promoting a meritocratic CS.

As per the previous logic, clientelism should be more common the further away we are from democracy, when political influence is concentrated, and leaders need to please mainly some small influential groups. In such cases, income redistribution via large, targeted transfers becomes preferred to public goods provision, as the latter also spreads out towards non-influential outsiders [32]. Hence, **meritocratic PS reform is *more* likely as countries become *more* democratic**.

Blum (2014) provides empirical support to this statement, showing that World Bank financed public sector management (PSM) reforms (PS among those) perform better when political rights and civil liberties are less restricted. This suggests that, compared to other policy areas, "PSM reforms are distinctly vulnerable to the political economy logic of rent-seeking", likely because of the difficulty to protect these reforms from antagonistic political incentives. (Blum, 2014, p.31)

[31] (Bueno De Mesquita et al., 2005; Deacon, 2009)
[32] (Bueno De Mesquita et al., 2005; Deacon, 2009)

A small strain of literature proposes that PSM reforms implementation, PS included, might be easier in autocracies than in democracies, since the organized opposition is weaker and there is less dependence on electoral outcomes [33] (Bunse and Fritz, 2012). In addition, transparent and systematic personnel policies could be useful for autocrats because they increase the credibility of future rewards to those public officials who contribute to the regime goals (Gehlbach and Keefer, 2012).

Despite these arguments, history shows that **most authoritarian and hybrid regimes have either not promoted any meritocratic PS reform** or have largely engaged in start-stop exercises (Bunse and Fritz, 2012). It is certainly possible to find some non-democratic regimes such as Vietnam[34], China[35], and Singapore[36] that successfully carried significant PS reforms. These countries, nonetheless, already had long histories of some degree of meritocracy in the public sector[37], hence standing as outliers vis-à-vis other non-democracies.

Bunse and Fritz (2012) broad literature review finds that **recent democratization can be a PSM reform *trigger***[38], as this is often begins/renews electoral competition (see section's 4.3.1.3 discussion), promotes civil society participation, and brings demand for the fight against corruption and better public services. We can find examples of it in post-Communist Central and East Europe[39], Latin America[40] and South-East Asia[41]. However, in most Africa, democratization did not come with significant PSM and PS reforms[42]. Ultimately, more research is needed to

[33] Conversely, Heredia and Schneider (2003) explain that PSM reforms are difficult in democracies because they are misaligned with electoral cycles, as they come with clear short-term costs and unclear long-term benefits.
[34] (Dinh, 2002; Painter, 2006)
[35] (Burns, 2008; Gehlbach and Keefer, 2012; Shirk, 1993; Tong et al., 1999)
[36] (Jones, 2008; Quah, 1996)
[37] Bunse and Fritz (2012), referencing Fukuyama (2011).
[38] Whether such reforms are ultimately successful is a different issue.
[39] (Kotchegura, 2008; Liebert et al., 2013)
[40] See Grindle (2012) for a recount of Brazil and Mexico's case.
[41] See Pierskalla et al. (2021) for Indonesia.
[42] The democratic transition in Africa has been largely unsuccessful in terms of PS reform. Most countries have

disentangle the circumstances under which democratization could indeed help pushing reform forward.

4.3.1.3 Electoral competition

Electoral competition *can* promote meritocracy in the PS. Competition, by definition, increases politicians' probability of being replaced by others from different coalitions, interests, and preferences. Consequently, both politicians and bureaucrats may wish to promote meritocracy to ensure the continuity of employment for: i) party appointees that can later protect their interests (Ruhil and Camões, 2003) and/or ii) sympathetic bureaucrats that guarantee the durability of legislative decisions (Horn, 1995). Furthermore, meritocracy also serves to potentially block patronage possibilities to hostile successors.[43]

However, **electoral competition can also disincentivize meritocratic reforms and even further increase clientelism**. For starters, electoral competition reduces politicians time horizons, hence diminishing the likeliness to benefit from reforms such as PSM, which yield mainly long-term benefits (Lapuente and Nistotskaya, 2009). Pierskalla and Sacks (2020, p.1) lay the burden on the **lack of credibility of programmatic politics**[44]: when not credible, institutional reforms that "move politics to mass-focused competitive environments" will increase patronage, particularly around election years. Their study of Indonesia's highly decentralized education

experience setbacks in terms of wage containments and personnel reductions (Ayee, 2008). Furthermore, overall public services provision had declined in most countries (Crook, 2010), but for Botswana and Eritrea (Olowu, 2010).

[43](Grzymala-Busse, 2007; Ting et al., 2013).

[44]Political systems are programmatic when most political parties i) mobilize support, ii) design policies, and iii) govern, following coherent and consistent ideological positions. This stands in contrasts with non-programmatic systems, were parties i) connect with their voters with charismatic or clientelistic appeals (rather than clear policies); ii) do not have internal structures to commit party leaders to their policies; and/or iii) do not implement their proposed programs when in government (Cheeseman et al., 2014).

sector confirms such findings [45].

Brierley (2020) research reconciles what seems to be opposing points, allowing for electoral competition to both increase and decrease patronage hiring. Her theoretical and empirical work in Ghana confirm that **electoral competition can discourage politicians from hiring *professionals*[46] in non-meritocratic ways**, because it can impact state performance. However, this same electoral competition will also encourage politicians to recruit partisans in low ranked positions (clerics and technicians), to ensure political support. Brierley (2020) hypothesis helps explain the phenomenon of public administration's "missing middle": not enough professionals to guarantee high quality service provision, coexisting with overstaffed (often unqualified) lower public service positions (Blum and Rogger, 2020).

Schuster (2020) points that it is **not electoral competition, but rather *uncertainty*, what disincentivizes PS reform**[47]. He explains that electoral competition affects PS reform incentives depending on whether the electoral loses are *potential* or *certain*. When exit from office is *certain*, competition can incentivize reforms as insurance (as discussed above). However, if elections are contested and exiting is a possibility, incumbents resist reforms that limit patronage, since these diminish their electoral advantage.

[45] Pierskalla and Sacks (2020) explain that higher electoral competition (at the local level) lead to higher numbers of contract teachers, as well as dramatically salary increases, with no corresponding improvement in education outcomes.

[46] For instance, budget analysts, engineers, planning officers and so on.

[47] The author supports his theory with a study of the Dominican Republic's (unsuccessful) CS tenure reform process.

A. Political budget cycles

Political budget cycles discourage PS reforms[48]. There is a well-established literature on political business and budget cycles[49], which asserts the importance of budget manipulation for electoral purposes[50]. These cycles are induced by both programmatic and patronage politics, with a certain **increase in patronage "in the wake of elections**." (Remmer, 2007, p.374). As a result, during electoral years public expenditure shifts away from public goods provision and capital expenditures, and towards more visible current expenditures like wages and subsidies.[51]

Patronage business cycles are almost exclusive to developing countries[52], and become more prevalent when i) political parties are on average younger (Hanusch and Keefer, 2013), and ii) as politicians' rents of remaining in power increase (Shi and Svensson, 2006). Furthermore, **these cycles will accentuate in countries with competitive elections *and* higher natural resources rents** (Vergne, 2009). For instance, recent quasi-experimental research on resource rich Brazil and Indonesia concretely shows that, during election years, local governments considerably increase their teachers hiring, without any related improvement in education outcomes.[53]

B. Programmatic political parties, and party systems

Cruz and Keefer (2015) confirm that **stronger and more programmatic parties incentivize reform**, because their public goods promises are more credible. After studying PSM reform *attempts* (CS among them) in 109 countries, they conclude that reforms are more likely to

[48] As explained in section 3.2.1.3, PS reform comes with clear short-term costs and unclear long-term benefits (Heredia and Schneider, 2003). Hence, they are misaligned with political budget cycles.
[49] Starting with Rogoff (1990) seminal theoretical account of electoral budget manipulation.
[50] For a thorough review on the subject, see Pierskalla and Sacks (2018).
[51] Finan and Mazzocco (2016); Khemani (2004); Saez and Sinha (2010); Vergne (2009).
[52] (Brender and Drazen, 2004; Shi and Svensson, 2006).
[53] Akhtari et al. (2017); Monteiro and Ferraz (2012); Pierskalla and Sacks (2020); Teso et al. (2018).

fail in countries with non-programmatic political parties (without clear policy platforms/ideologies)[54]. In such cases, politicians have no incentives to promote performance enhancing reforms, since the electorate would not consider such promises credible and, in consequence, they would not grant any electoral advantage. Hence, incumbent politicians drop reforms and continue luring voters with clientelistic practices.

In a related manner, Huber and Ting (2021) indicate that merit-based PS reform probabilities depend on **the polarization of the party system, together with the opposition characteristics**. In places where political opponents have "extreme" preferences, incumbent politicians have no incentive to engage in PS reforms when there is political competition. This is the case because, should the incumbent loose elections, they would have brunt all the costs and none of the benefits of reform. Hence, electoral competition is only expected to encourage PS reform when the opposition has non-extreme preferences (a thus values public goods provision).

It should be noticed that political parties are just one of the many networks that facilitate patronage exchange. In fact, patronage can flourish even in the absence of a party system[55], as clientelistic exchanges can occur within plenty of other systems, "from bureaucratic networks and kinship groups and clans to religious organizations and ethnic communities."[56] (Aspinall and Berenschot, 2019, p.30)

[54] Blum (2014) findings echoes these results.

[55] For instance, between 1986 and 2006 Uganda functioned as a "no-party democracy", where all political organizations were banned and individuals running for elections were meant to do so based on "individual merit" (Carbone, 2008). Despite the lack of formal parties, in such period Ugandan politics were highly clientelistic, aided by a "political economy of favouritism and corruption"(Bogaards, 2010, p.422). These exchanges just happened outside a traditional political party structure.

[56] There is plenty of qualitative evidence (often ethnographic) on the matter for Indonesia (Aspinall and Berenschot, 2019), Thailand (Arghiros, 2001), Central Asia (Collins, 2006), Uganda (Carbone, 2008) and Senegal (Beck, 2008).

4.3.1.4 Violent conflict

Blum and Rogger (2020) add to this literature with an analytical framework on the dynamics of PS structure and reform in **post-conflict states**[57]. Their model parts from the notion that public jobs and their associated wages are a key mechanism for **rents redistribution and peace keeping**. The PS structure is defined by an *ongoing* bargaining between the centralized elites[58] (in control of the civil service) and the decentralized government organizations[59]. Donor agencies[60] can also participate in the process, by offering the centralized elites a reform program, with associated funds.

By this logic, **PS dynamics ultimately depend on whether decentralized bodies have any leverage over the executive** and credibly threat its stability. Consequently, merit-based PS reform seems more likely under two scenarios. In the first, the decentralized agents are relatively weak (with little access to violence or other means of disruption) and the center elites have incentives for public service delivery (due to citizen accountability or their own internalized preferences). In the second, the decentralized agents are relatively strong *and* have preferences for public service delivery.

4.3.2 Socioeconomic factors

In terms of socioeconomic context, the evidence is clear in that low productivity diminishes incentives for meritocratic PS reforms. With regards to budget composition, the literature

[57] This model, however, is also informative to understand public service reform in resource rich countries, many of which are also politically unstable and prone to conflict.

[58] The chief executive and its reporting central agencies, such as the Ministry of Finance and the Ministry of Civil Service (or other centralized human resources agency).

[59] Line-ministries, departments and agencies.

[60] IMF, World Bank and others.

suggests that high non-tax-based revenues (both foreign aid and natural resource rents) decrease PS reform likeliness; however, more empirical evidence is needed to fully confirm it.

4.3.2.1 Low productivity

Meritocratic PS reforms are less likely in low productivity setups. Low productivity incentivizes clientelism through 3 distinct channels: i) raising public employment attractiveness for low-income/skills voters, ii) diminishing outside options for those already holding public jobs, and iii) by increasing political parties' monetary dependency on bureaucrats.

When clientelistic politics are already in place and productivity is low, public service reform is not incentive compatible for politicians. As amply discussed in my previous 2 papers, clientelistic public employment is more likely to emerge in low productivity countries (Robinson and Verdier, 2013). Intuitively, **when productivity is low, low-income voters alliance can be easily "bought" with public sector jobs**, which grants wages above those they could achieve in the private sector. Politicians also benefit from clientelistic public employment, as they can gain more rents from public employment than they would from private one . Moreover, politicians have incentives to further decrease productivity by under-providing public goods, as this increases public employment offers attractiveness.

The second mechanism to consider is that low productivity implies there are less job opportunities and smaller relative wages in the private sector, which in turn **decreases outside options** for those already working in the public sector. As per Blum and Rogger (2020) analytical framework (see section 4.3.1.4), less outside options render PS reform unattractive.

A third relevant mechanism is that the **formal private sector size affects party financing**

strategies, and hence clientelistic practices. Sigman (2015) explains that when the formal private sector is large, parties often raise funds by granting contracts to businesses in exchange for monetary support. On the opposite, with a less developed formal private sector, parties tend to rely on rent-seeking bureaucrats for funds. Consequently, when productivity is low, clientelism is directed to political supporters, the most loyal of which get placed in public jobs with direct access to state revenue and rents.

4.3.2.2 Aid-dependency and other non-resource rents

The literature, albeit limited, suggests that **high non-tax based revenues decrease PS reform likeliness**. On the one hand, Geddes (1994) claims that larger budgets may enable merit-based reform, since more resources provide a buffer for patronage concessions needed to secure governability and, consequently, professional recruitment. On the opposite, Mwenda and Tangri (2005) explain that aid relaxes patronage budget constraints and disincentivizes the professionalization of public service bodies. These contradictory accounts suggest that the *source* of budget largesse likely matters: if an important share of revenues does not come from taxes (like foreign aid or natural resources rents), governments have less incentives to promote reforms needed to tax their populations[61], which in turn implies that PS reforms are less likely.

In the particular case of **foreign aid**, there is large N size evidence that World Bank PSM reform projects (PS reform included) perform better in countries that depend more on aid[62]. Bunse and Fritz (2012) defend this by claiming that fiscal crises may induce reform, because they diminish the political power of those actors that have patronage networks. However, Heredia

[61] (Bräutigam and Knack, 2004; Moore et al., 2008)
[62] (Blum, 2014; Bunse and Fritz, 2012).

and Schneider (2003) indicate that **fiscally induced reforms are not sustainable and *likely to be reversed soon***, as they usually focus on containing payrolls rather than undoing political discretion[63].

A. External pressure: Donor agencies and symbolic reform

Governments in developing countries frequently face internal and external pressures to initiate public sector reforms, but tend to abandon them at the implementation stage, when the political costs outshine the potential gains[64]. Development assistance organizations ("donors") are at the front of promoting institutional reform in developing countries, since countries who wish to borrow from them ought to comply with certain good governance conditionalities[65]. Nonetheless, the evidence on the effectiveness of these efforts is not very encouraging.

For instance, public sector institutional reforms are a key component of the World Bank's program: 65 percent of all operations between 2000 and 2010 include it. Between 2000 and 2006 only, the World Bank lent USD 422 million *per year* for 71 projects related to public service and administrative reform; however, less than 45 percent of borrowers showed any improvement in the target areas. In the particular case of PS reforms, i) salary adjustments were not carried out ; and ii) downsizing attempts either did not take place or were soon reversed (usually rehiring the same people) (Webb et al., 2008). Hence, despite good intentions and expertise, such donor driven reforms bring rather modest improvements (if at all).

[63] Olowu (2010) and Mukandala and Kiragu (2005) summarize Africa's experience with such fiscally induced reforms, most of which focused on containing the size and cost of the CS. As a result, and against expectations, real pay levels declined in most countries, diminishing governments possibilities to attract and retain critical skills. Consequently, overall service provision has declined through the region (but for Botswana and Eritrea) and bureaucratic quality remains low (except in Botswana, Tunisia, Namibia, Mauritius and Eritrea).

[64] (Bunse and Fritz, 2012; Webb et al., 2008).

[65] (Blum, 2014; Goetz, 2001)

DiMaggio and Powell (1983) **isomorphic change theory** sheds light into these findings. According to the authors, leaders symbolically "mimic" public service institutions sponsored by development partners, to get legitimacy or funds from them. Nonetheless, despite formally accepting such reforms, leaders try to avoid them during the implementation period, hence dodging the domestic political price.

In the same spirit, Andrews (2013b) explains that reforms in developing countries are often introduced as short-term signals, to ensure external support and legitimacy. "Signal" reforms typically i) overlook contextual factors, ii) propose "impressive-looking but hard-to-reproduce best practice interventions" (Andrews, 2013b, p.3) and iii) are driven by a narrow group of high-level agents that outsiders consider champions. These reforms often generate new forms (like laws) which nonetheless do not become operational, often due to lack of ownership from the implementing agents. Hence, governments might look better in the short term, but in the long term are functionally the same.

Reforms aiming at requiring performance data are a simple way to look better and be "perceived as battling government waste" (Moynihan, 2008, p.192), while cutting down domestic political costs[66]. The "creation, selection, interpretation, and presentation of performance information" (Moynihan, 2008, p.2) is not an entirely objective process, but rather one that reflects the values of the actors involved in the political process. Consequently, politicians can use performance information to claim the success of their PSM reforms, without them bringing significant change.

Johnson (2015) explains that the compatibility with domestic political forces is key to donor

[66] According to Moynihan (2008), this so-called reform of requiring performance information should "succeed" domestically if it does not i) offend key players (like unions), nor ii) blatantly benefit any political party over the other.

aided reform success (not simply form changes). When donors' incentives are incompatible with those of domestic players, reform is unlikely to succeed. On the opposite, donors can support reform if national governments are the real impetus behind it, leading both the design and implementation (Repucci, 2014).

4.3.2.3 Voter preferences and organized interests

A. Voter preferences

Economic development is as a key factor contributing to the profitability of clientelism[67]. A strand of literature suggests that patronage reform could be more likely after socioeconomic shifts that change voter preferences towards public goods, such as: i) greater income, education, private employment alternatives and exposure to the international economy[68]; ii) the formalization of finance and banking[69]; iii) urbanization[70] and a large middle class[71]; and other important demographic shifts like iv) the decline of the fraction of foreign-born citizens (for whom patronage is more appealing)[72] .

Brierley (2020) reviews substantial empirical evidence suggesting that even in relatively clientelistic countries, some citizens still weight in public service delivery and general economic performance when voting. For instance, some literature suggests that macroeconomic stability can be an important determinant of vote choice in Africa[73], Latin America[74], and Eastern Eu-

[67](Bustikova and Corduneanu-Huci, 2017; Hicken, 2011; Kitschelt and Kselman, 2013; Wilkinson, 2007)
[68](Calvo and Murillo, 2004; Hicken, 2011; Kitschelt and Kselman, 2013)
[69](Hite-Rubin, 2015)
[70](Larreguy et al., 2016, 2015)
[71](Weitz Shapiro, 2012)
[72](Ruhil, 2003)
[73]Bratton et al. (2012) provide aggregate evidence on Benin, Botswana, Ghana, Kenya, Lesotho, Madagascar, Malawi, Mali, Mozambique, Namibia, Nigeria, Senegal, South Africa, Tanzania, Uganda, and Zambia.
[74](Murillo and Visconti, 2017)

rope[75].

Despite these claims, there is **little evidence claiming that socioeconomic changes *successfully lead* to patronage reform demands** (Schuster, 2016). Most countries, seem to remain stuck in a "clientelist trap", were elected politicians cater to the demands of *just about enough* voters to keep them in power, but undermine bureaucratic autonomy in the process (Shefter, 1977). **Clientelistic politics** necessarily imply the under-provision of public goods, which consequently **prevents the socioeconomic changes that would lead to patronage reform**. Furthermore, when clientelism and patronage are already common, public goods-oriented politicians are rarely elected because their promises are not considered credible[76].

B. Organized interests

In the overall, **organized interests' mobilization does not come up as an important driver of PS reform**. As Blum et al. (2019) explain, in Europe and the United States civil society coalitions mobilized reformist legislators against patronage because this became a politically salient issue among voters (bringing the costs of reform down). Nonetheless, it is an anomaly for citizens to organize and successfully push for this type of merit-based reforms; this was the case mainly for the United States [77].

The private sector is rather unlikely to engage in these issues. Business leaders only demand reforms of institutions that directly affect business transactions, like regulatory agencies or the central bank (Heredia and Schneider, 2003). In fact, in clientelistic regimes, big businesses (the ones that could have an influence) can benefiting from (illicit) state contracts, so they might not

[75](Roberts, 2008)
[76](Keefer and Knack, 2007; Remmer, 2007; Robinson and Verdier, 2013)
[77](Grindle, 2012; Schuster, 2016)

be in favor of merit based reforms[78](Schuster, 2016).

4.4 How do resource riches relate to context?

In this section, I provide both theoretical (section 4.4.1) and empirical evidence (section 4.4.2) on the association between resource richness and the above-mentioned contextual factors. First, I review the new theories (not previously discussed in this paper) on how resource richness affect context, based on the natural resource curse literature. Second, I discuss which contextual factors are more prevalent in resource rich countries, building on a combination of empirical literature and simple correlations.

This section shows that resource riches are associated to lower productivity, a higher probability to be non-democratic and engaged in violent conflict, worse institutions (higher corruption in particular), more pronounced political budget cycles, and less programmatic political parties (Table 4.3 summarizes these findings). As exposed in section 4.3, these contextual factors increase the likeliness of clientelistic public employment and turn meritocratic PS reform less likely in resource rich countries than in those without (to be discussed in section 4.5).

4.4.1 Theory: the natural resource curse

There is a well-nourished body of theory on the relationship between natural resources and long-term economic growth. Traditionally, this literature discusses what is known as the "*natural resource curse*", whereby resource abundance diminishes total factor productivity (TFP) and hence long-term growth. The "economic" strain of this literature [79] highlights the role of real ex-

[78]Once more, except for the US during the early years of their PS reform. See Miller (2000)
[79]See Frankel (2011) for a comprehensive survey.

Table 4.3: Overview. How do natural resource rents (NRR) relate to context?

Factor	NRR correlation	Conditional on	Mechanism
Low productivity	Positive	Extractive institutions	Resource windfalls appreciate currencies and promote rent-seeking, leading to de-industrialization and reduced entrepreneurship
Political regime	Negative	Democracy	Lower taxation and higher politically motivated expenditure ultimately weaken accountability and checks and balances
	Positive	Autocracy	Prevents democratization
Electoral competition	Negative	Democracy	Decreases subnational electoral competition and worsens politician's quality
Political budget cycles	Negative	Young democracy	Higher discretionary budget for politically motivated expenditures during elections
Programmatic political parties	Negative	Young democracy	-
Violent conflict	Positive	-	No consensus on which mechanism prevails

change rate appreciation and consequent de-industrialization. The "political" stream emphasizes the negative institutional consequences of resource riches[80], which ultimately also worsen long term economic prospects.

4.4.1.1 Economic resource curse

The "*natural resource curse*" theory poses that **natural resource windfalls crowd out other sectors of the economy**. Such windfalls appreciate the real exchange rate, thus increasing export prices while decreasing those of imports. Hence, local non-resource tradable industries become less competitive, leading to an overall smaller private sector and higher private unemployment in the long term. This creates a theoretically well developed[81], and extensively empiri-

[80] Worse governance, higher corruption and rent seeking, and more conflict. See Ross (2015) for a summary.

[81] For early theories, see Auty (1993, 2001); Gelb (1988); Krugman (1987); Sacks and Warner (1997); Van Wijnbergen (1984)

cally documented[82], negative relationship between resource riches and smaller TFP growth, also known as the "Dutch Disease".

Baland and Francois (2000) and Torvik (2002) models discuss an alternative channel to that of currency appreciation: **natural resource abundance increases rent-seeking activities**[83], which in turn lowers productivity and aggregate welfare. Large natural resource rents affect entrepreneurs' behavior by: i) increasing the number of those engaged in rent-seeking and ii) reducing the share which runs productive firms. This, naturally, translates into lower productivity (although, according to Baland and Francois (2000), the initial equilibrium will determine the extent to which this happens).

These theories feed the notion of a **rentier state**[84], which relies on substantial external rents (with the government as the main recipient) that can sustain the economy without a strong productive domestic sector. In these, only a small share of society creates wealth, while the rest engages mainly in its distribution and utilization. Importantly, the government becomes larger to redistribute the natural resource wealth, through a combination of public goods and private transfers. Furthermore, it tries to compensate the lack of private jobs by becoming a major employer, which increases wages for lower skilled workers and creates expectations among certain groups to become government employees[85].

Recent revisions of the natural resource curse literature question the veracity of the so-

[82]For important empirical documentation, see Auty (2001); Collier (2007); Gelb (1988); Harding and Venables (2016); Humphreys et al. (2007); Ismail (2010); Sachs and Warner (2001)

[83]In the political economy literature, rent-seeking refers to activities that increase wealth for a given individual or group without producing any new wealth. For example, a government may create some "prize" (be it a subsidy, import license or monopoly) that diverse interest groups compete for, via lobbying, media campaigns or even bribery. Noticeably, only those earning the "prize" benefit from it, while the society at large either does not benefit or is explicitly worse off because of it (i.e., having to pay monopoly prices). (Mazzuca, 2019)

[84]Beblawi (1987) and Luciani (1987) are among the early proponents of this hypothesis.

[85]These expectations are often known as the "rentier mentality". According to Hertog (2010b, p.1), rentier states citizen's reliance on patronage "breaks the link between effort and reward, leads to low achievement orientation in economic life and makes citizens politically passive".

called curse, since empirical evidence shows that the effect of natural resources on growth is **not** unequivocally negative, as the theory predicts. After all, "for every Venezuela and Nigeria, there is a Norway or a Botswana" (Robinson et al., 2006, p.7).

A new generation of theory shows that the overall economic impact of natural resource booms depends on pre-resource shock institutions[86]. **Countries with "extractive institutions"**[87], which enable mainly small elites to funnel economic rewards, **will likely have the natural resource curse**. On the opposite, resource riches will *benefit* countries with "inclusive institutions", such as property rights, unbiased law systems, and wide-spread public services. These promote accountability and state competence, by keeping in track "the perverse political incentives that such booms create" (Robinson et al., 2006, p.6). Furthermore, they attract entrepreneurs into production, which in turn promotes higher growth (Mehlum et al., 2006). Hence, the economic resource curse is contingent on institutional features.

4.4.1.2 Political resource curse

Theories on the political resource curse focus on how natural resource rents (oil predominantly) affect the political process and incentives of diverse interest groups. This research line proposes that resource revenues degrades institutional quality in democracies, stabilize authoritarian regimes, and trigger violent conflict (Ross, 2015). [88]

[86](Boschini et al., 2013; Mehlum et al., 2006; Robinson et al., 2006, 2014; Van Der Ploeg and Poelhekke, 2010)

[87]I take the term from Acemoglu and Robinson (2013), which define it in contraposition to inclusive institutions (those that permit most people to reap the gains of their own efforts).

[88]The upcoming three sections lean heavily on Ross (2015) comprehensive review on the issue of the political resource curse.

A. Resource riches hinder institutional quality in democracies

The theoretical literature champions the "rentier effect" as the lead explanation on how resource wealth degrades democracies' institutional quality (and that of autocracies too). **Natural resource rents increase both the stakes and the probabilities of keeping office**[89], due to the readiness of larger funds to influence election outcomes (and often for politicians' personal purposes). Therefore, politicians in resource rich countries seek to remain in power through a combination of low tax rates and high public spending, which ultimately inhibits democratic reform pressures (Andersen and Aslaksen, 2013).

Models on the rentier effect distinguish between two related effects on taxation and expenditure[90]. On the one hand, the "**taxation effect**" implies that governments with abundant resource revenues **tend to tax their populations less heavily**; in turn, citizens are less likely to demand accountability from the government (Ross, 2001a). Consequently, resource riches curb down incentives to invest in tax extraction capacities (Besley et al., 2010), which then increases rent seeking vulnerability and further weakens state competence[91].

On the other hand, the resource rents "**spending effect**" describes a pattern of **greater expenditure in clientelistic redistribution**, which also reduces democratization pressures (Ross, 2001a). Indeed, Wiens (2014) model predicts that resource riches curb down incentives to limit how resource wealth is spent, and Ross (2001b) shows that politicians can go as far as to undo well-functioning fiscal institutions in order to gain access to the rents.

[89] Caselli and Cunningham (2009); Morrison (2007); Robinson et al. (2006, 2014); Robinson and Verdier (2013)

[90] Ross (2001a) also discusses the group formation effect, according to which the government uses its generous budget to discourage the formation of social groups that are independent from the state, as these often push for greater political rights. Nonetheless, the evidence on this mechanism is predominantly empirical, so I do not discuss it in this section.

[91] Beblawi (1987); Karl (1997); Luciani (1987).

Deepening into the mechanisms, Collier and Hoeffler (2005) explain that **resource rents can undermine democracy by weakening i) electoral competition and ii) checks and balances** (such as press freedom). These are often referred to as "voice", and it is an important mean for citizens to discipline governments into providing public goods. Collier and Hoeffler (2005) predict that natural resource rents undermine voice (and hence democracy) because, unlike taxation, resource rents do not incite citizen scrutiny[92]. This, in turn, facilitates clientelistic politics and reduces public goods provision.

Finally, Brollo et al. (2013) propose an alternative mechanism: **natural resources can deteriorate political candidates' quality and increase corruption**. In this model, larger resource rents driven budgets affect the political process through 2 related mechanisms. First, through moral hazard: the incumbent politician's corruption increases with budget size, since they can seize political rents without upsetting imperfectly informed voters. Second, there is a selection effect: the average quality of new politicians declines with budget size, due to moral hazard (political rents increase with the budget) and because political candidates with lower ability are more attracted to political rents. The interaction between these two effects explains corruption growth: if the incumbent faces less able opponents, (s)he can seize more rents without decreasing their reelection probabilities.

[92]Citizens pay taxes out of their pocket. On the opposite, regular citizens do not participate in natural resources extraction, so these rents are "hidden" from their eyes (Klomp and de Haan, 2016). Accordingly, tax payments are perceived as foregone income, while natural resources are generally not. This is why citizens hold the government accountable for the use of the former, but not as much for the latter (Huntington, 1991).

B. Resource riches support autocracies

The connection between autocracies and *oil* riches[93] has been extensively described by resource-rich countries scholars, with particular emphasis in the Middle East region[94]. The theoretical literature is less developed, but diverse models seek to explain whether oil wealth supports autocratic regime survival by reducing the chances of either i) transitioning to another dictatorship, or ii) becoming a democracy (Wright et al., 2015). The literature is ambiguous in terms of whether resource rents prevent other autocrats from taking over (i), but clearly concludes that these diminish democratization chances (ii), even with perfect institutional safeguards in place (Morrison, 2007).

Within this family of models, a first group predicts that **natural resource rents prevent autocrats from being deposed, because they allow to buy off opponents**. This is the essence behind Acemoglu et al. (2004) kleptocracy model, where corrupt rulers can keep power without broad base support if they effectively break society's cooperation efforts to depose them. Resource riches aid in the process by giving the autocrats a large purse for their "divide-and-rule" game. Along the same lines, Gandhi and Przeworski (2007) sustain that autocrats can make use of natural resource rents to buy the legislatures, hence using "democratic structures" to increase their survival chances.

Bueno De Mesquita and Smith (2010) propose an alternative mechanism: **resource rich autocrats can suppress "coordination goods", hence diminishing the probability of *coup d'états***. Coordination goods are those that help people organize themselves, and include public

[93] Oil is the king in this literature. Other natural resources are not frequently studied.

[94] Ross (2015) provides a rich summary of this literature, discussing seminal research on Iran (Mahdavy, 1970), Kuwait and Qatar (Crystal, 1990), Tunisia (Bellin, 1994), Libya (Vandewalle, 1998), the Gulf States (Chaudhry, 1989; Gause, 1994) and generally in the Arab world (Beblawi, 1987).

goods such as free press, transparency and communication means. These goods help organizing production, but also political activities which could be detrimental for the government. Rulers of countries with large resource riches can afford to suppress "coordination goods" (and hence rebellions), because they do not greatly depend on general productivity for income. This finding is consistent with Egorov et al. (2009), who predict that resource riches increase autocracies stability by reducing media freedom.

In addition, autocrats can, and often do, **use natural resources revenues to finance repression**. Acemoglu and Robinson (2001) model predicts that in very unequal societies the elite may lose too much if democratization occurs; hence, they prefer to repress revolutions than to permit democratization. However, Ali and Elbadawi (2016) show that it will only be optimal for dictators to set up a repressive security apparatus **when the natural resources *per capita* are small**. If resources are high relative to the population size, repression is more costly than redistributing the resource rents to the population.

The non-monotonic relationship between repression and natural resources can be due to the fact that powerful militaries are "a double-edged sword for the elite" (Acemoglu et al., 2010, p.2). On the one hand, autocrats strongly prefer to prevent democratic transitions, and will hence use natural resources to power up and buy the military. However, a more powerful military also raises the risk of a coup against the current regime. Consequently, while higher resource rents to seem to help against democratization, it is unclear whether they block other autocratic challengers.

C. Resource riches fuel conflict

Natural resources abundance is often pointed as a key culprit for conflict. Empirically, this relationship has been studied intensely[95], but theoretical models are scarce, particularly when it comes to the role of natural resources in inter-state conflict. Acemoglu et al. (2012) developed a dynamic model between a resource-rich and a resource-poor country, to explain the effect of resource scarcity when countries' have limited commitment to future policies and transfers. Here, **war incentives decrease with the elasticity of natural resources' demand**. When demand is inelastic, the value of the resources' stock increases as they become scarcer, which in turn raises the resource-poor country's incentives to invade the resource-rich one. In a slightly different fashion, Caselli et al. (2014) put the emphasis on the geographic location of the resources: **the likelihood of conflict between two countries increases when at least one of them has natural resources *near their common border***[96].

A second strain of literature models how **natural resource rents induce fighting between rival groups within the same country**. Most theories emphasize the **key role of ethnic fragmentation**, explaining that ethnic coalitions can be formed to compete for the economy's resources [97]. Consequently, civil wars should be more frequent when a minority group's territory is particularly resource rich, especially in low-capacity states (Morelli and Rohner, 2015). Nonetheless, the relationship between resource rents and conflict intensity is not monotonic, and can be affected by the resource abundance (Reuveny et al., 2011), the economy's long run growth[98] and

[95] Parting from Collier and Hoeffler (1998)seminal paper.

[96] When both countries have natural resources, conflict is more likely if the resource is unevenly distributed around the border. The emphasis on asymmetry of resources *around the border* makes intuitive sense: if an area in foreign country is far from the border, it is more difficult to reach it and seize whichever resources are in it.

[97] (Caselli and Coleman, 2006; Hodler, 2006)

[98] (Caselli and Coleman, 2006; Wick, 2008)

third-party threats such as future economic sanctions[99].

4.4.2 Empirical evidence

4.4.2.1 RRC have lower productivity, conditional on extractive institutions

As discussed in section 4.4.1.1, there is an extensively empirically documented negative relationship between resource riches and lower productivity [100]. Most recent empirical literature focuses on discussing the non-linearity of this relationship, explaining under which conditions it holds.

A first group of authors claim that the main mechanism behind the "curse" is not the real exchange rate appreciation, but rather the fact that natural resource prices are volatile and hence create excessive macroeconomic instability[101]. Hence, countries which succeed controlling such instability[102] should be *less* vulnerable to the "curse", as did Norway through its State Petroleum Fund[103] or Chile via its Copper Stabilization Fund and Rules for the budget deficit[104]. These cases, nonetheless, are exceptions.

A second group of authors indicates that the type of natural resources is a determinant factor. Boschini et al. (2013) find **evidence of the resource curse in minerals rich countries, conditional on low-quality institutions**. Likewise, Isham et al. (2005) propose that countries dependent on "point source" natural resources (hydrocarbons and minerals) and plantation crops

[99] (Hasan, 2016; Hasan and Lahiri, 2017)

[100] For important empirical documentation, see Auty (2001); Collier (2007); Gelb (1988); Harding and Venables (2016); Humphreys et al. (2007); Ismail (2010); Sachs and Warner (2001)

[101] Blattman et al. (2007); Hausmann and Rigobon (2003); Van der Ploeg and Poelhekke (2009); Van Der Ploeg and Poelhekke (2010)

[102] See Frankel (2011, p.27-34) for a summary of successful institutions, in the context of resource rich countries, for the promotion of monetary policy and pro-cyclical national savings.

[103] Created to smooth oil income consumption and invest savings in the future (Hannesson, 2001; Ovald et al., 2019).

[104] Frankel (2011, p.30-31)

are prone to economic and social instability. On the opposite, countries with "diffuse" natural resource exports (livestock and agricultural produce from small farms) do not show these effects.

Finally, a third group provides empirical support to the theoretical work claiming that **the "curse" only applies to countries with extractive institutions**. The same authors developing such models[105] accompanied their work with empirical sections which naturally supported their claims, as does other recent purely empirical research on the matter [106].

The newest strain of the economic resource curse literature focuses on the local effects of natural resource rents, with mixed results[107]. In developing countries, resource rent shocks can bring about real income increases and lower poverty for households near *mining* areas[108], but also higher inequality[109], negative traded sector employment effects [110] and increases in municipal government spending without corresponding extra public goods and services[111]. While there are positive effects, the magnitude is rather small and does not come without negative side effects as the ones mentioned above (Cust and Poelhekke, 2015); therefore, it is **unlikely that these minor local effects could induce productivity gains in the aggregate economy**.

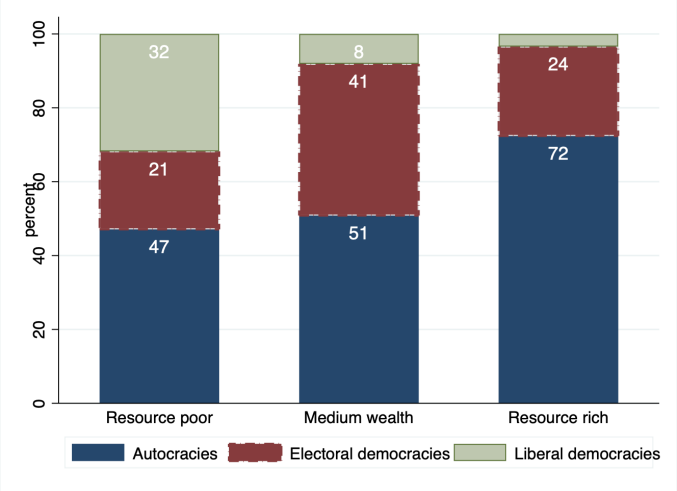
4.4.2.2 RRC are more likely to be autocracies

72 percent of RCC are autocracies, which stands in sharp contrast with the 51 and 47 percent of the medium resource riches and resource poor counterparts, respectively (Figure 4.4).

[105] Boschini et al. (2007); Mehlum et al. (2006); Robinson et al. (2006); Van Der Ploeg and Poelhekke (2010)
[106] Bhattacharyya and Hodler (2014); Boschini et al. (2013); Farhadi et al. (2015); Gelb et al. (2012); Kim and Lin (2017)
[107] See Cust and Poelhekke (2015); Pelzl and Poelhekke (2018); Van Der Ploeg and Poelhekke (2017) for a summary.
[108] See Aragon and Rud (2013); Loayza and Rigolini (2016) in Peru, Toscani (2017) in Bolivia, and Pelzl and Poelhekke (2018) in Indonesia.
[109] Loayza and Rigolini (2016) found a decrease in poverty that, however, also came with higher inequality.
[110] See De Haas and Poelhekke (2019).
[111] See Caselli and Michaels (2013) for evidence on Brazil, and Larrain and Perello (2019) for Chile.

Even when they are democratic, RCC and medium resource rich countries are far more likely to be electoral democracies[112] than liberal democracies[113], meaning that they often fail in terms of equality before the law and individual liberties, and lack proper judicial or legislative constraints on the executive.

Figure 4.4: Political regime by natural resource rents level. Years 2000-2018.



Source: own elaboration, based on the V-Dem 2021 dataset (Coppedge et al., 2021a) and WDI (2020).
Note: Resource richness measures the average share of natural resource rents over GDP. This is below 1.9% for resource poor countries, and below 9.5% for medium wealth ones (quartiles 2 and 3, respectively). Electoral democracies i) have free and fair multiparty elections (unlike autocracies), but ii) do not guarantee equality before the law, individual liberties nor proper judicial or legislative constraints on the executive (unlike liberal democracies).

The empirical literature provides strong cross-country support for the claim that *oil* rents can increase authoritarian leaders term duration [114], prevent democratic transitions [115], or a combi-

[112]Electoral democracies have free and fair multiparty elections (de facto), and a minimum level of institutions that guarantees the participation of a plurality of actors, such as freedom of expression and alternative sources of information, freedom of association, free and clean elections, universal suffrage, equality before the law and respect to individual liberties. Nonetheless, electoral democracies are deficient in terms of equality before the law and individual liberty (access to justice, transparent law enforcement, liberal principles of respect for personal liberties, rule of law and lack proper judicial or legislative constraints on the executive (Coppedge et al., 2021b).

[113]Liberal democracies are electoral democracies that also guarantee equality before the law and individual liberty, and have proper judicial or legislative constraints on the executive (Coppedge et al., 2021b, p.283).

[114]Andersen and Aslaksen (2013); Cuaresma Crespo et al. (2011); Gandhi and Przeworski (2007)

[115]Gassebner et al. (2013); Ulfelder (2007)

nation of both[116]. In terms of the resource type, Prichard et al. (2018) and Andersen and Aslaksen (2013) discuss that autocratic persistence is driven primarily by oil wealth, rather than mineral wealth. In fact, Andersen and Aslaksen (2013) found that alluvial diamonds and other types of minerals can potentially *reduce* authoritarian leaders terms. Alternatively, Morrison (2009, 2015) posits that any increase in resource rents, and non-tax revenue generally, should stabilize autocracies.

4.4.2.3 RRC democracies have weaker institutions

Two empirical literature strains study whether natural resources are associated with i) "less democracy", and/or ii) worse institutions[117]. The literature generally supports a negative relationship between natural resource wealth and the deterioration of democracies and their institutions, particularly for oil producers. This has been the case after the nationalization of oil industries in the 1980s, when developing countries' governments seized massive oil revenues that were previously managed by foreign-owned firms.[118]

In terms of the first question, a wide set of cross-country studies supports the **negative relationship between natural resource wealth and democracy levels**. These results have been replicated with variety of datasets and methods, with recent estimates being robust to distinct definitions of democracy levels[119], the use of country fixed effects[120], instrumental variables[121],

[116] Bueno De Mesquita and Smith (2010); Morrison (2009); Prichard et al. (2018); Wright et al. (2015)

[117] In this context, the definition of "institutions" is rather blurry, but economists generally refer to it as the rules of the game, which ultimately drive economic development and growth (Rodrik et al., 2002).

[118] A revolutionary (at the time) study by Haber and Menaldo (2011) discussed that such relationship did not exist. However, as pointed by Andersen and Ross (2014), Haber and Menaldo (2011) study only runs until the 1970's, after which the political resource course has been (robustly) shown to exist, as per Ahmadov (2014); Andersen and Ross (2014); Lall (2017); Ross (2001a, 2012b) findings.

[119] The definition of "democracy level" changes by study, but it is always measured in terms of the Polity score plus other indicator(s) on civil liberties and political rights.

[120] Andersen and Aslaksen (2013); Andersen and Ross (2014); Tsui (2011)

[121] Ramsay (2011); Tsui (2011)

considering missing data issues (Lall, 2017), performing an extreme bounds analysis of regime type determinants (Gassebner et al., 2013), and even a statistical metanalysis of 29 studies and 246 empirical estimates (Ahmadov, 2014). However, there is still no consensus on whether more resource rents simply "pushes a country's democracy level down" (Tsui, 2011, p.1), while still remaining a democracy, or if it actively promotes a transition towards autocracy, as suggested by Prichard et al. (2018).

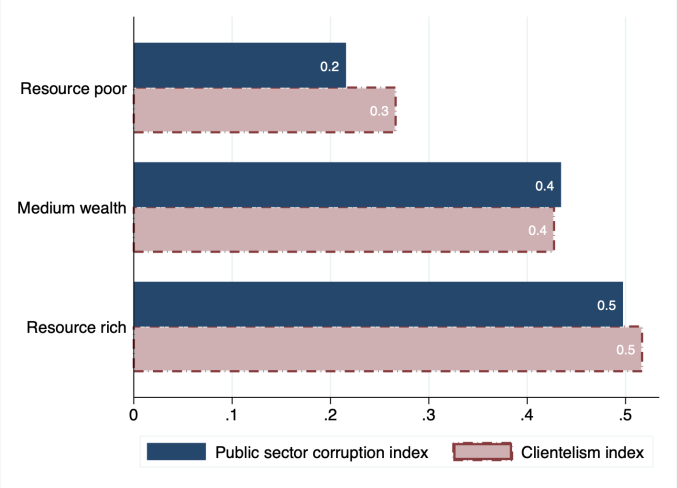
With regards to the second question, the empirical literature is divided in terms of how natural resource wealth affect institutional quality. The most studied issue is whether resource rents promote corruption, and there is strong cross-country evidence supporting a **causal link from oil rents to corruption**[122]. And corruption in the public sector is almost always equatable to clientelism (Figure 4.5). More recently, a set of subnational studies also supports this causal link from natural resources to corruption, for countries as diverse as Brazil (Brollo et al., 2013; Caselli and Michaels, 2013), China (Zhu and Wu, 2014), Nigeria (Subramanian and Sala-i Martin, 2003) and Sao Tome and Principe (Vicente, 2010).

A second set of studies by Anthonsen et al. (2012); Bulte et al. (2005); Isham et al. (2005) finds that resource riches negatively affect other institutional quality measures, such as the rule of law, property rights and legal impartiality. Nonetheless, Brunnschweiler and Bulte (2009); Busse and Groning (2013) do not find such link. In fact, Brunnschweiler and Bulte (2009) find that, after controls, resource abundance positively affects growth and institutional quality.

A third set of papers supports that resource rents diminish tax collection (Morrison, 2009) and weakens investments in fiscal capacity (Knack, 2009; Masi et al., 2018). According to theory

[122] Andersen and Aslaksen (2013); Anthonsen et al. (2012); Arezki and Bruckner (2009); Arezki and Gylfason (2013); Busse and Groning (2013); Isham et al. (2005)

Figure 4.5: Public sector corruption and clientelism in democracies, by natural resource rents level. Years 2000-2018.



Source: own elaboration, based on the V-Dem 2021 dataset (Coppedge et al., 2021a) and WDI (2020).
Note: Resource richness measures the average share of natural resource rents over GDP. This is below 1.9% for resource poor countries, and below 9.5% for medium wealth ones (quartiles 2 and 3, respectively).

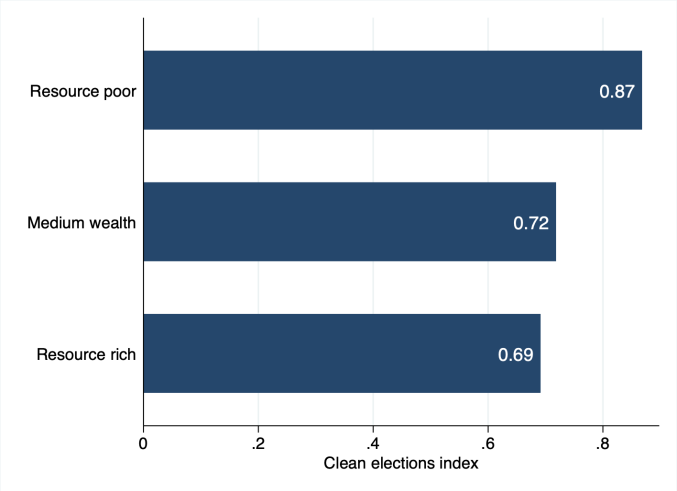
(section 4.4.1.1), this should increase rent seeking vulnerability and further weaken state competence and bureaucratic quality. The findings, once more, are inconclusive: Anthonsen et al. (2012); Isham et al. (2005) do find the expected negative link between resource rents and bureaucratic quality or government effectiveness, but Brunnschweiler and Bulte (2009) do not.

Ultimately, the empirical literature offers no consensus on the effect of natural resource rents in institutional quality, except for the strong causal link from oil rents to corruption. This could well be due to difficulties in measuring institutional quality and simultaneity problems, which do not permit to disentangle the real relationship between resource wealth and institutional quality (Ross, 2015). More empirical work remains to be done for the sake of clarity, but the very strong and consistent relationship between oil rents and corruption suggest natural resource rents generally undermine institutional quality.

4.4.2.4 RRC have less electoral competition, at the subnational level

Elections are generally less free and fair in democracies with high and medium resource riches than in their resource poor counterparts (Figure 4.6). This suggest that electoral competition is likely lower in the former than in the latter. However, as I will discuss below, the **negative correlation between electoral competition and resource riches is more pronounced for regional and local elections than in central government ones**. This does not permit to establish clear predictions with regards to the link between electoral competition and PS reform likeliness in RCC.

Figure 4.6: How free and fair are elections in democracies? Years 2000-2018.

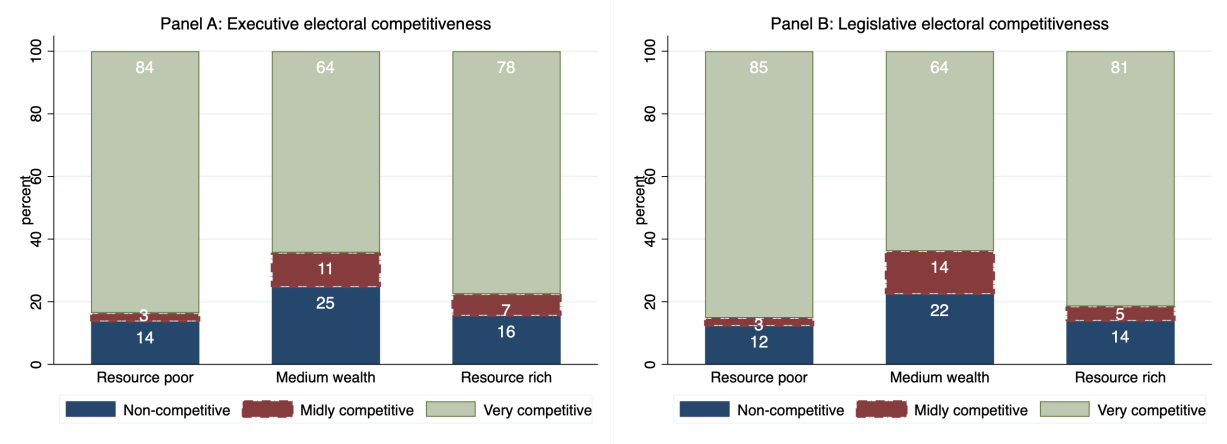


Source: own elaboration, based on the V-Dem 2021 dataset (Coppedge et al., 2021a) and WDI (2020).
Note: Resource richness measures the average share of natural resource rents over GDP. This is below 1.9% for resource poor countries, and below 9.5% for medium wealth ones (quartiles 2 and 3, respectively). Non-democracies are excluded. The index increases with the absence of registration fraud, vote buying, election violence, government intimidation and systematic irregularities.

In the overall, resource poor democracies have more competitive elections for the executive

and legislative bodies (Figure 4.7)[123]. However, these same elections are also more competitive in resource rich democracies than in those with medium resource riches. Hence, this suggests a non-monotonic relationship between natural resource rents and central government's election competitiveness.

Figure 4.7: Executive and legislative electoral competitiveness in democracies, by natural resource rents level. Years 2000-2018.

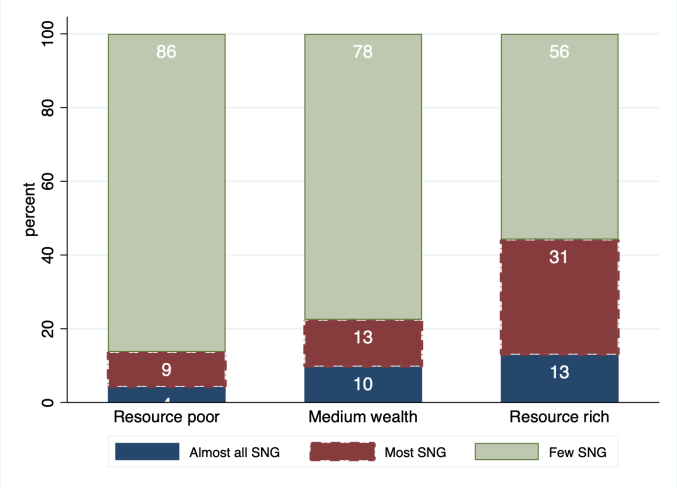


Source: own elaboration, based on the DPI 2017 dataset (Cruz et al., 2018) and WDI (2020).
Note: Non-democracies are excluded. The figure shows how competitive are executive and legislative elections (Appendix 4.B for details). Resource richness measures the average share of natural resource rents over GDP. This is below 1.9% for resource poor countries, and below 9.5% for medium wealth ones (quartiles 2 and 3, respectively).

When it comes to **subnational governments (SNG), higher natural resources clearly correlate with less electoral competition**. In resource poor democracies, the main political party rarely has a high extent of control of important policymaking bodies across regional and local governments: in 86 percent of cases, they control less than 60 percent of them[124] (Figure 4.8). However, SNGs control is more concentrated when resource riches are large. In resource rich democracies, a single political party controls most SNGs in 45 percent of instances[125].

[123] Although the difference between resource poor and resource rich is minor when it comes to legislative elections.
[124] "A few", as per the Database of Political Institutions (DPI) expert assessments (Cruz et al., 2018)
[125] Over 90% of SNGs in 13 percent of cases, and between 60 and 90% of SNGs in 32 percent of cases.

Figure 4.8: Single party control of subnational governments in democracies, by natural resource rents level. Years 2000-2018.



Source: own elaboration, based on the DPI 2017 dataset (Cruz et al., 2018) and WDI (2020).
Note: Non-democracies are excluded.The figure shows how prevalent is single party control over important policymaking bodies, across regional and local governments (SNG). Control can range from almost all SNG's (90% +), most SNG's (60-90%) and few SNG's (less than 60%). Resource richness measures the average share of natural resource rents over GDP. This is below 1.9% for resource poor countries, and below 9.5% for medium wealth ones (quartiles 2 and 3, respectively).

The empirical literature on the relationship between natural resource rents and electoral competition is scarce, and predominantly discusses **the negative impact of resource booms on *local* elections**, for individual countries[126]. These effects can be attributed to two separate mechanisms: i) higher re-election probabilities and wider vote margins, and ii) a decrease in the quality of candidates.

With regards to the first channel, Carreri and Dube (2016) find that positive oil price shocks reduce the number of candidates running for office in Colombian municipalities, and widens the vote margin for winners. In Peru, Maldonado (2014) shows that local politicians' re-election

[126]I only found one cross country study on the matter. For 15 sub-Saharan countries, McGuirk (2013) found that an increase in natural resource rents decreases perceived tax enforcement, and hence reduces the demand for transparent and open elections.

probabilities increase with mining revenue transfers, conditional on high levels of mining revenues[127]. In Brazil, Brollo et al. (2013) observes that incumbents who receive larger oil revenues are also more likely to be reelected. Monteiro and Ferraz (2012) also observe that oil royalties create a large incumbency advantage in Brazil's municipalities, but this effect disappears in the short-run.

In terms of the second channel, Carreri and Dube (2016) observe that positive oil price shocks increase the number of legislators affiliated with right-wing paramilitary groups, thus creating far more radicalized legislatures. Brollo et al. (2013) finds that larger oil revenue transfers (by 10 percent) decrease the fraction of opponents with at least a college degree by 3 percentage points (about 7 percent). Even more shockingly, Asher and Novosad (2018) show that mineral rent shocks raises the number of elected local politicians (in India) with criminal records.

4.4.2.5 RCC have more pronounced political budgets cycles, conditional on being a young democracy

There is scarce, but clear, large N evidence showing that **natural resources rents can intensify electoral cycles and increase clientelism**, conditional on there being some political competition[128] (which somewhat excludes autocracies) and on being a young democracy[129] (and hence more clientelistic)[130]. Furthermore, recent quasi-experimental research in resource wealthy settings shows an increase clientelistic public employment during election years: in Brazil[131] and

[127] On the opposite, for the average municipality (with low mining revenues) the re-election probability is negative.
[128] Vergne (2009)
[129] Klomp and de Haan (2016)
[130] As per Keefer and Vlaicu (2005), politicians in young democracies are less credible, which in turn leads to more clientelism (Keefer and Vlaicu, 2008).
[131] Akhtari et al. (2017); Monteiro and Ferraz (2012); Teso et al. (2018)

Indonesia[132], teachers' hiring substantially increases around election periods, without any related improvement in education outcomes.

Although the direct empirical evidence is limited, it is intuitively clear that natural resource rents should intensify political budget cycles. Natural resources rents are somewhat "hidden" from citizens eyes, since they are extracted mainly by government enterprises or big private multinationals (Klomp and de Haan, 2016). Consequently, and unlike tax payments, natural resource rents are generally not considered forgone income, so citizens do not hold the government as accountable for how they are used (Huntington, 1991). This, de facto, softens governments budget constraints[133]. Hence, politicians in resource rich countries have every incentive to use the fiscal largess to enhance their reelection probabilities.

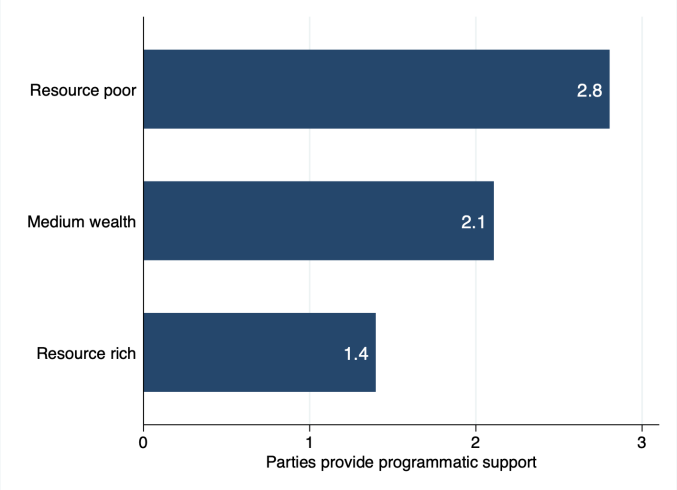
4.4.2.6 RRC political parties are less programmatic, conditional on being a young democracy

There is, to my knowledge, no empirical literature studying the relationship between programmatic political parties and resource rents. Nonetheless, in line with the higher predominance of clientelism, one can observe that **higher resource riches are also associated with non-programmatic political parties** (that is, that do not clearly follow ideologies/policies). As per the V-Dem project expert assessments, between years 2000 and 2018 the main political parties in resource rich countries offered supporters a combination of clientelistic rewards and local collective goods (Figure 4.9). While political parties in resource poor countries also relied on

[132] Pierskalla and Sacks (2020)
[133] McGuirk (2013); Thorton et al. (2008)

local collective goods, programmatic appeals[134] are more frequently offered vis-a-vis resource wealthier countries.

Figure 4.9: Programmatic political parties by natural resource rents level. Years 2000-2018.



Source: own elaboration, based on the V-Dem 2021 dataset (Coppedge et al., 2021a) and WDI (2020).
Note: Resource richness measures the average share of natural resource rents over GDP. This is below 1.9% for resource poor countries, and below 9.5% for medium wealth ones (quartiles 2 and 3, respectively). Parties level of programmatic support range from 0, with fully clientelistic support (constituents rewarded with goods, cash, and/or jobs) to 4, where it is fully programmatic (constituents follow parties' positions on national policies, programs, and visions for society). For further details, see appendix 4.B.

4.4.2.7 RRC are more likely to be engaged in violent conflict

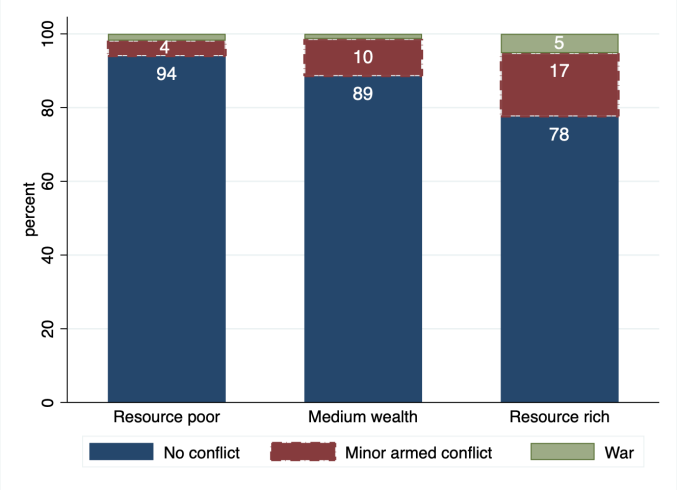
Armed conflict[135] is not a frequent occurrence: between years 2000-2018, only 11 percent of the world's countries took part in it. However, the **likelihood of having an armed conflict in any given year increases with resource riches** (Figure 4.10). While resource poor coun-

[134] Meaning, that parties mobilize support, design policies, and govern, following coherent and consistent ideological positions (Cheeseman et al., 2014).

[135] The Uppsala Conflict Data Program (UCDP) defines conflict as "a contested incompatibility that concerns a government and/or territory where the use of armed force between two parties, of which at least one is the government of a state, results in at least 25 battle-related deaths in a calendar year." (Themner, 2018)

tries engaged in conflicts (be them minor armed conflicts or wars) in only 6 percent of instances, medium and resource rich ones did so in 11 and 22 percent of cases, respectively. More dramatically, since the 1990's, oil-producing countries have been 50 percent more likely to have civil wars than non-producers. (Ross, 2012a, p.145)

Figure 4.10: Armed conflict prevalence by natural resource rents level. Years 2000-2018.



Source: own elaboration, based on the UCDP/PRIO Armed Conflict Dataset (Gleditsch et al., 2002; Pettersson and Eck, 2018) and WDI (2020).

Note: Resource richness measures the average share of natural resource rents over GDP. This is below 1.9% for resource poor countries, and below 9.5% for medium wealth ones (quartiles 2 and 3, respectively). In minor armed conflicts there are between 25 and 999 battle-related deaths in a given year, while in wars there are at least 1,000 (Themner, 2018).

Recent evidence overwhelmingly supports a **causal link from natural resource riches to armed conflict**[136]; however, there is no consensus about the causal mechanisms behind it (Ross, 2015). Furthermore, this relationship is affected by a myriad of contextual factors[137], including: i) if the resource is located in regions that are poor relative to the national average (Ostby et al.,

[136] Cotet and Tsui (2013) are a recent exception. They study historical discoveries of giant oil wells and claim that simple country fixed effects remove the statistical association between oil reserves and civil war onset. They do find, nonetheless, that such oil discoveries may increase i) the likelihood of political violence, and ii) military spending in non-democracies.

[137] See Basedau and Roy (2020) and Ross (2015) for a detailed discussion on the matter.

2009) or populated by marginalized ethnic groups (Hunziker and Cederman, 2017), ii) ethnic fragmentation (Wegenast and Basedau, 2014), or iii) whether local populations were severely affected by negative externalities arising from resource extraction, among other explanations[138]. Finally, the type of natural resource seems to matter, with non-renewable (Koubi et al., 2014) and lootable resources[139] (Roy, 2016) being pointed as the main culprits behind conflict onset and recurrence[140].

4.4.2.8 RCC are not more likely to engage in symbolic reforms

The scarce empirical evidence on the subject does not suggest that RCC are either more or less likely to engage in symbolic reforms than non-resource wealthy peers. Blum (2014) finds no effect, neither positive nor negative, of resource rents size on World Bank's PSM reform projects *short term* performance (PS reform included). Consequently, this study suggests that RCC who do engage with foreign donors do not fare any different in the short term than their resource poorer peers, and hence are no more (or less) likely to engage in quick symbolic reforms.

Intuitively, one can imagine that resource riches largess on average diminishes the need of multilateral partners funding, or that this smaller reliance on income support might reduce political leaders likeliness to accept "inconvenient" conditionalities. Hence, it is reasonable to assume that resource riches decrease the need to engage in de jure reforms that do not truly materialize. These, however, are just assumptions, and more empirical evidence is needed to strongly support this.

[138] Haslam and Ary Tanimoune (2016); Rustad and Binningsbo (2012)
[139] Narcotics, diamonds, gold and other precious metals.
[140] Dube and Vargas (2013) provide an alternative explanation, whereas conflict is fueled by price spikes in non-labor intensive resources (such as oil), or price drops in labor intensive ones (like agricultural goods).

Table 4.4: Overview. How do natural resource rents (NRR) affect meritocratic public service (PS) reform?

Factor	Conditional on	NRR correlation	PS reform likelihood	Mechanism
Low productivity	Extractive institutions	Positive	Decreases	A smaller private sector offers less outside employment options to job seekers. Thus, they are more dependent on (clientelistic) PS jobs.
Democracy	-	Negative	Decreases	The less democratic a country, the more rulers' profit from providing private transfers to a small elite; this is inconsistent with a meritocratic PS.
Electoral competition	Democracy	Negative	Not affected	If clientelism is high and electoral competition increases, PS reform likeliness can decrease.
Political budget cycles	Young democracy	Positive	Decreases	PS reforms are misaligned with electoral cycles, due to their clear short-term costs and unclear long-term benefits.
Programmatic political parties	Young democracy	Negative	Decreases	Non-programmatic parties cannot credibly offer or pursue performance enhancing reforms.
Violent conflict	-	Positive	Decreases	In fragile contexts, patronage is key for peace keeping.
External pressures	-	None	Not affected	Less dependency on foreign donors lowers pressures to *initiate* reforms

Note: See tables 4.3 and 4.1 for summaries of: i) how resource riches relate to context, and ii) how context matter for PS reform (regardless of NRR), respectively.

4.5 How do natural resource rents affect public service reform, given context?

In this section I propose how natural resource rents affect meritocratic PS reform in the average resource rich country. I build these conclusions based on the previous findings on how i) resource riches are related to context (section 4.4) and ii) how each contextual factor affects PS reform probabilities (section 4.3). Table 4.4 summarizes these findings.

4.5.1 RRC have lower productivity - PS reform is less likely

Because RRC are on average less productive, this *reduces* their likeliness of PS reform (conditional on extractive institutions). The economic resource curse literature supports a negative correlation between natural resource riches and productivity in countries with poor institutions (section 4.4.1.1). Low productivity, in turn, diminishes outside employment options, and with it the likeliness of PS reform, particularly if the politicians' key constituents are already employed in the public sector (section 4.3.2.1). Hence, we can conclude that, conditional on extractive institutions, natural resource rents diminish outside employment options, and with it the likeliness of PS reform.

4.5.2 RRC are less democratic - PS reform is less likely

Because RRC are on average less democratic, this *reduces* their likeliness of PS reform. The political resource curse literature supports: i) a positive correlation between natural resource rents and authoritarian regime stability (section 4.3.1.1.B), and ii) a negative correlation between natural resource riches and democracy levels in democracies (section 4.3.1.1.A). *Less* democracy, in turn, renders PS reform *less* likely: the less democratic a country, the more rulers will profit from clientelistic dynamics, providing private transfers to a small elite; this is inconsistent with a meritocratic PS [141] (section 4.3.1.2). Hence, we can conclude that natural resource rents are associated with less democracy, and thus diminish the likeliness of PS reform.

According to Bunse and Fritz (2012), recent democratization might trigger PS reforms; however, it is still not well understood under which circumstances this materializes. Furthermore,

[141] Politicians only have incentives to build meritocratic PS when they need to massively deliver public goods; this is only politically rational for them in democracies, when accountability and broad popular support truly matter.

democratization is considerably less likely to happen in resource rich countries than in resource poorer counterparts (Ross, 2015, p.243). Consequently, recent democratization is an unlikely candidate to enable PS reform in countries with abundant resource wealth.

4.5.3 RCC have less electoral competition - No effect on PS reform

Because RRC have on average less electoral competition, this *should not affect* the likeliness of PS reform. There is an empirically supported link between natural resource shocks and *less* competition in *subnational* elections (section 4.4.2.4). On the other hand, the literature poses that *higher* electoral competition can disincentivize PS meritocratic reforms if clientelistic politics are prevalent, as they often are in RCC (section 4.3.1.3). This in turn implies that ***if electoral competition increases* in the average RCC, this can decrease PS reform likeliness**. However, one cannot conclude from this literature that less electoral competition can incentivize PS reform. Hence, if electoral competition remains "low", this channel should not play a particular role (positive or negative) in PS reform in resource rich countries.

4.5.4 RCC have deeper political budgets cycles - PS reform is less likely

In democracies with political competition, natural resource rents *reduce* PS reform likeliness by deepening the political budget cycle. Larger natural resources rents can further deepen electoral cycles and increase clientelism, conditional on political competition and on being young democracies (section 4.4.2.5). In addition, theory tells us that PS reforms are generally misaligned with electoral cycles, as they come with clear short-term costs and unclear long-term benefits (section 4.3.1.3.A). Hence, we can conclude that, conditional on being a young democ-

racy, natural resource rents are associated with deeper political budget cycles and thus diminish the likeliness of PS reform.

4.5.5 RCC political parties are less programmatic - PS reform is less likely

Because RRC are less prone to programmatic politics, this *reduces* their likeliness of PS reform. In line with the higher predominance of clientelism, higher resource riches are also associated with non-programmatic political parties, conditional on being a young democracy (section 4.4.2.6). PS reforms, in turn, are more likely to *fail* in countries with non-programmatic political parties (section 4.3.1.3.B), since the electorate does not consider promises on performance enhancing reforms credible (in consequence, these would not grant any electoral advantage). Hence, we can conclude that natural resource rents are associated with higher prevalence of non-programmatic parties, and thus diminish the likeliness of PS reform (conditional on being a young democracy).

4.5.6 RRC are more likely to engage in violent conflict - PS reform is less likely

Because RRC are more likely to engage in violent conflict, this *reduces* their likeliness of PS reform. The political resource curse literature suggests a strong negative correlation between natural resource rents and conflict (section 4.4.2.7). In post-conflict settings, public jobs and their associated wages are a key mechanism for rents redistribution and peace keeping; because of this, meritocratic PS reforms are often unsuccessful in such settings (section 4.3.1.4). Hence, we can conclude that since natural resource rents are associated with higher conflict, they diminish the likeliness of PS reform.

4.5.7 RCC are not more likely to engage in symbolic reforms

RRC are no more likely to engage in symbolic PS reforms. Developing countries often symbolically mimic PS institutions supported by development partners, to gain legitimacy or access funds. These engagements often result in the adoption of *de jure* reforms which nonetheless do not become operational (section 4.3.2.2.A). Intuitively, resource riches largess could decrease the need of multilateral partners funding, or at least diminish leaders' willingness to accept "inconvenient" conditionalities. In addition, the scarce empirical evidence on the subject does not suggest that RCC are either more or less likely to engage in PS reforms than non-resource wealthy peers (section 4.4.2.8). Hence, it is reasonable to assume that higher natural resources do not enhance the need to engage in symbolic reforms for the sake of pleasing development partners.

4.6 Conclusions

4.6.1 This paper's findings

Despite their importance to successfully implement public policies, professionalized and meritocratic public services (PS) are far from being the norm[142]. This elucidates the fact that reforming bureaucracies to eliminate clientelism is an extremely difficult endeavor; patronage systems are very resilient and often survive and readapt formal reform attempts. The literature suggests that merit-based PS reforms a rare occurrence because they are not compatible with politicians' short-term incentives, and will only happen if politicians take a long term perspective.

Resource rich countries are considerably more likely to have clientelistic PS than their

[142](Brierley, 2020; Grindle, 2012)

resource-poorer peers. The literature reviewed for this essay suggests this is the case because natural resource rents are associated with several contextual factors that *decrease* the likeliness of merit PS reform (meant to reduce clientelistic public employment), by diminishing the short-term gains of engaging in politically costly reforms.

To start with, resource rich countries tend to have lower productivity (on average). This diminishes outside employment options for those already employed in the public sector. Because of it, PS reform is less attractive to both the public and politicians, particularly if the politicians' key constituents are already employed in the bureaucracy.

Resource rich countries are also on average less democratic, and are either autocracies democracies that score worse in diverse indicators of political participation and constraints on the executive than non resource rich countries. The less democratic a country (ceteris paribus), the more its rulers benefit from clientelistic dynamics, providing private transfers to a small elite; this, naturally, is inconsistent with a meritocratic PS.

When democratic, resource rich countries are less prone to programmatic politics. This also reduces the likeliness of PS reform, because the electorate does not believe in performance enhancing reform promises (thus, these would not grant any electoral advantage). Moreover, natural resource rents also intensify the political budget cycle in such countries, which in turn further reduces PS reform probability.

Finally, resource wealthy countries are on average more likely to engage in violent conflict. In post-conflict settings, public jobs and their associated wages are a key mechanism for rents redistribution and peace keeping; hence, meritocratic PS reforms are often unsuccessful there.

During this review, I did not find that resource riches are associated to any contextual factor that could ease public service reform, in any capacity. I only observed that PS reform

incentives are not distinctively affected in resource rich countries due to i) lower (local) electoral competition, ii) together with less symbolic reform pressures from development partners.

4.6.2 What can resource rich countries do about their public services? Policy implications

The above mentioned findings paint a bleak panorama for merit public service (PS) reform in resource rich countries: on average, these will not succeed *in the short term*, since they are generally incompatible with politicians incentives. And higher resource rents, on average, further decrease the likeliness of successful PS merit reforms.

This calls for a pragmatic approach when it comes to improving PS in resource rich countries. Policy makers, or development partners, seeking to engage in it may need to adopt modest selective reforms in the short term, rather than expect immediate success of comprehensive PS reform agendas that often run against politicians' clientelistic redistribution incentives.

Countries with high resource rents, much like any other country, must have proper legislations and systems to promote bureaucratic excellence. Nonetheless, policy makers need to be curve down expectations and understand that: i) these type of reforms only rarely succeed[143] and, when they do, ii) success happens after a *long* series of deconstruction and reconstruction efforts. In fact, it is the slow accumulation of these reform avoidance "legacies" what ultimately determines PS characteristics (Grindle, 2010). Hence, despite difficulties (or perhaps because of them), we can still observe positive deviance[144] even in the most adverse conditions.

[143] It is not unique to resource rich countries to have a poor track record of PS reforms. Truly successful merit PS reforms are the exception: most reform attempts do not lead to more functional governments who better solve problems (Andrews, 2013a,b).

[144] Some public sector reform literature uses this term for successful reforms, which lead to more functional governments that solve problems. See (Andrews, 2013a; Cammett, 2022; Pascale et al., 2010), among others.

The evidence on positive deviance highlights an important lesson for *all countries* aiming to improve their PS: these reforms are long and bumpy roads, which require a **long-time horizon to succeed. Endurance is key**. According to Grindle (2012), predictable and non-clientelistic PS prospered in Europe and the US thanks to the strategic action of reformers, who consolidated institutional changes after keeping their project alive over several periods (surviving many administrations).

A second lesson for *resource rich countries* in particular is to **aim for modest, selective reforms**. In the short term, it is politically unattainable to eliminate clientelism in favor of a generally meritocratic and better functioning PS. Nonetheless, it may be possible to develop "islands of excellence" (IE), which effectively provide public goods and services despite operating in environments where this unusual (Hickey, 2019). Early EI research[145] sustains that such outlier institutions can be instrumental to impulse developing countries' developmental state.

When choosing where to concentrate efforts, policymakers should know that IE are more likely to arise in agencies with easily definable and measurable functions. Hence, these are usually economic technocracy islands[146], such as ministries of finance (MoF), central banks and revenue authorities. Some IE examples in resource rich countries are those of the KSA's Saudi Aramco and other pockets of administrative efficiency (Hertog, 2010a,b), Venezuela's oil company *pre-Chavez regime* (Matsuda, 1997), and post-independence Timor-Leste's MoF (Blum et al., 2019).

Interestingly, Grindle (2012) notices that motivated leaders have often made use of **patronage to improve state effectiveness**. Under this logic, change oriented leaders in resource rich

[145] Important early work discuss the cases of Japan (Johnson, 1982), South Korea and India (Evans, 1995), Brazil (Geddes, 1990), as well as a general overview (Leftwich, 1995). More recently, see Evans and Heller (2011) for a discussion on South Africa.

[146] You will rarely have IE, for instance, in ministries of education and social affairs.

countries can and should make use of the system's discretionality to recruit highly qualified staffs and hence create IE[147]. But this, of course, depends on the existence of motivated leaders' who seek to improve public service provision.

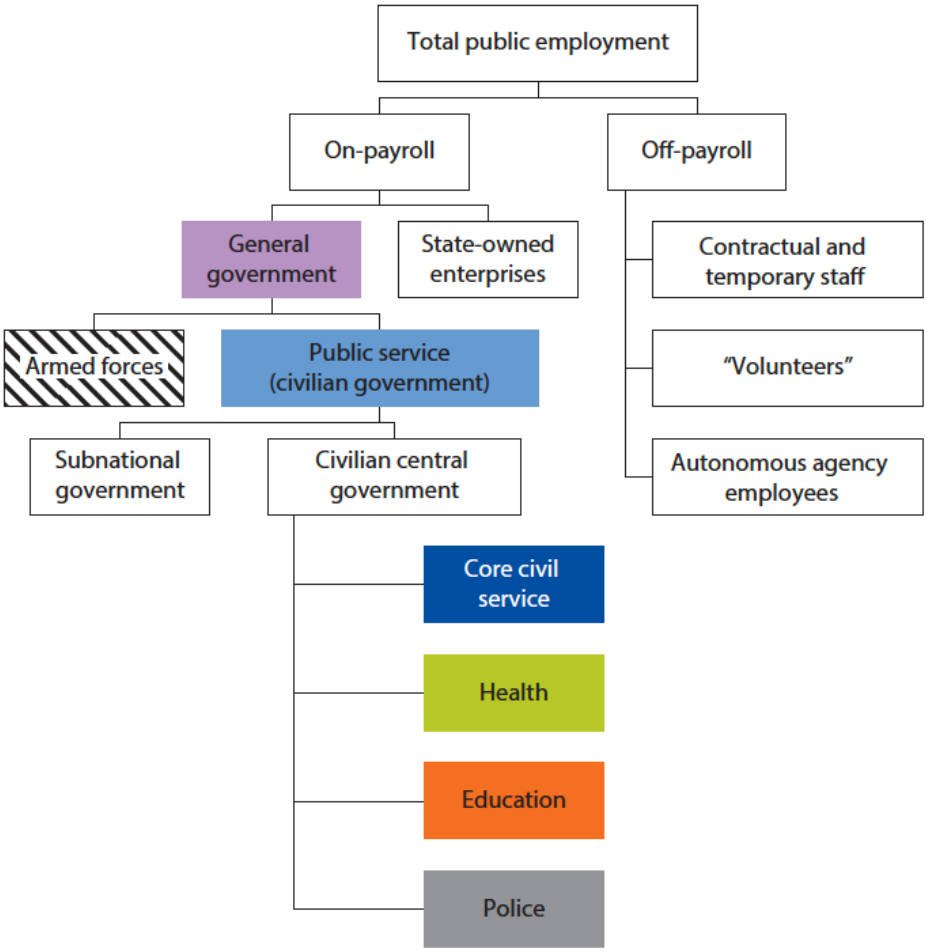
Finally, Andrews (2013a,b) tried to find common ground in terms of the paths leading to successful public sector reforms (of which PS is a subset), and found that success was generally achieved by **problem driven iterative adaptation** (PDIA). What this means that those PS reforms that (exceptionally) succeeded were: i) introduced iteratively, via experimentation and trial and error; ii) impulsed by many agents under different leadership roles; and hence iii) produced a mixed regime, suited to the particular context. Consequently, this suggests that resource rich countries seeking to successfully improve their PS should follow **PDIA's trial and error approach**, and walk away from standard "best practice" formulas.

Ultimately, we need more evidence to assess what works for the particular set of resource rich countries; at the moment, the literature on successful PS reforms in such places is simply too limited to provide detailed insights. Future research can help furthering this agenda by **identifying *relatively successful* public service reforms in countries with resource wealth**, focusing in "understanding the forms and sources of variation from context to context" (Woolcock, 2017, p.4), rather than identifying general best practices.

[147] As it certainly was done in the past in Brazil and Chile, for instance. See Grindle (2012).

4.A Additional figures

Figure 4.11: The structure of public employment



Source: Blum et al. (2019), adapted from Evans et al. (2004)

4.B Definitions

4.B.1 Concepts

Civil service is a group of government employees at the central and subnational level, and includes those working in ministries, departments and agencies - either managing daily activities, or developing and implementing government policies and programs. (Blum et al., 2019)

Clientelism is a phenomenon by which politicians distribute private goods in exchange for votes (or political support in general) (Stokes, 2009). In this dissertation, I focus exclusively on clientelistic public employment, not on other types of private goods that could also be used as rewards. My narrow usage of the term coincides with the definition of patronage (see below), so I use both terms interchangeably, as other in the literature often do (For instance, Kitschelt and Wilkinson (2007)).

Merit or meritocracy refers to the practice of recruiting bureaucrats on the base of qualifications, as well as permitting promotions according to predictable and objective criteria. (Cornell et al., 2020)

Patronage is a specific type of clientelist exchange, were public sector jobs are granted to reward for political support. (Hicken, 2011)

Political systems are programmatic when most political parties i) mobilize support, ii) design policies, and iii) govern, following coherent and consistent ideological positions. This stands in contrasts with systems were parties i) connect with their voters with charismatic or clientelistic appeals (rather than clear policies); ii) do not have internal structures to commit party leaders to their policies; and/or iii) do not implement their proposed programs when in

government. (Cheeseman et al., 2014)

Public service is a wide term that includes the (core) civil service, plus teachers, health workers and the police. (Rao, 2013)

Public service reform references activities aimed at strengthening merit in the public service.

4.B.2 Variables

Armed conflict: the UCDP defines conflict as "a contested incompatibility that concerns a government and/or territory where the use of armed force between two parties, of which at least one is the government of a state, results in at least 25 battle-related deaths in a calendar year." (Themner, 2018) *Source*: UCDP/PRIO Armed Conflict Dataset (Gleditsch et al., 2002; Pettersson and Eck, 2018).

Clean elections index: it measures how free and fair are elections, given the "absence of registration fraud, systematic irregularities, government intimidation of the opposition, vote buying, and election violence." (Coppedge et al., 2021b, 48) The index ranges from 0 to 1; higher values indicate cleaner and fairer elections. *Source*: V-Dem dataset (Coppedge et al., 2021a,b; Pemstein et al., 2021).

Clientelism index: it aims to quantify how predominant are clientelistic exchanges in any given country. The index ranges from 0 to 1, and higher values indicate a normatively worse situation (more clientelism). (Coppedge et al., 2021b) *Source*: V-Dem dataset (Coppedge et al., 2021a,b; Sigman and Lindberg, 2017).

Public sector corruption index: seeks to measure how prevalent it is for public employees

to "grant favors in exchange for bribes, kickbacks, or other material inducements, and how often do they steal, embezzle, or misappropriate public funds or other state resources for personal or family use". (Coppedge et al., 2021b, 297) The index ranges from 0 to 1, and higher values indicate a normatively worse situation (more corruption). *Source*: V-Dem dataset (Coppedge et al., 2021a,b; McMann et al., 2016).

Electoral democracies: systems with de-facto free and fair multiparty elections, and a minimum level of institutions that guarantee the participation of a plurality of actors , but failing in terms of equality before the law and individual liberty and lacking proper judicial or legislative constraints on the executive. (Coppedge et al., 2021b) *Source*: V-Dem dataset (Coppedge et al., 2021a,b; Luhrmann et al., 2018).

Liberal democracies: electoral democracies that guarantee equality before the law and individual liberty, while also having proper judicial or legislative constraints on the executive. (Coppedge et al., 2021b) *Source*: V-Dem dataset (Coppedge et al., 2021a,b; Luhrmann et al., 2018).

Party competition across regions: this variable describes electoral support for major parties (gaining over 10 % of the vote) across different regions, as per the following categories:

1. Few regions: most major parties are competitive only in one or two country regions;
2. Some regions: most major parties are competitive only in some country regions; or
3. Most regions: Most major parties are competitive in most country regions.

Source: V-Dem dataset (Coppedge et al., 2021a,b; Pemstein et al., 2021)

Programmatic parties: captures the most common form of linkage to constituents, among the countries' main political parties. As per Coppedge et al. (2021b), party-constituent linkages

are the type of "good" that the party offers in exchange for political support. This variable is ordinal, and takes the following values:

1. Clientelistic: when party constituents are granted goods, cash, andor jobs.

2. Clientelistic and local collective linkages mix

3. Local collective linkages: when constituents are rewarded with local public goods.

4. Local collective and policyprogrammatic mix.

5. Policyprogrammatic: when party constituents follow (and expect to be rewarded on the basis of) the party's positions on national policies, programs, and visions for society.

Source: V-Dem dataset (Coppedge et al., 2021a,b; Pemstein et al., 2021)

Legislative electoral competitiveness: this variable is a simplified version of the DPI 2017's original. Following Cruz et al. (2016), I classify legislative elections as follows:

1. Non-competitive: if the county either has i) no legislature, ii) an unelected legislature; iii) only 1 party can participate in elections (with either one or multiple candidates); or if iv) multiple parties are legal but only one party won seats.

2. Competitive: when multiple political parties win seats, and the largest political party holds:

   (a) More than 75% of the seats (Mildly competitive elections)

   (b) Less than 75% of the seats (Very competitive elections)

Source: DPI 2017 dataset (Cruz et al., 2016, 2018)

Executive electoral competitiveness: this variable is a simplified version of the DPI 2017's original. Following Cruz et al. (2016), I classify executive elections as follows:

1. Non-competitive: if the county either has i) no executive, ii) an unelected executive; iii) only 1 party can participate in elections (with either one or multiple candidates).

2. Competitive: when multiple political parties can participate in elections, but the winner holds:

   (a) More than 75% of the votes (Mildly competitive elections)

   (b) Less than 75% of the votes (Very competitive elections)

As per the DPI 2017's codebook, executives are those that are elected either directly by population, or by an electoral college that is elected by the people with the purpose of electing the executive.

Source: DPI 2017 dataset (Cruz et al., 2016, 2018)

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