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FROM DEMOCRATIZATION TO POLITICAL LIBERALIZATION:

FORMALIZING, OPERATIONALIZING AND TESTING POLITICAL REGIME CHOICE AND CHANGE

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to Chiara

autocrat of my heart

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Lots of people should be included here. I will only list a few.

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Foreword and Introduction

This thesis was born out of a few, simple questions. First, why is it that we know so much about political regimes today, and yet so little? After decades of comparative politics research, we have robust findings as to what pushes important political actors into choosing a particular political regime as compared to another; and yet they are constantly called into doubt. The same can be said about our knowledge on what makes political regimes more stable, or more liable to change. Why is it so?

From that initial question, others followed. Amid the rebirth of autocratic studies, over the last decade many scholars were intent at classifying autocracies according to their institutional setting (for example into civil, military, royal, and personalistic autocracies), but only a few were trying to understand whether causes varied not just along institutional types, but also along "liberalization" types. Why was it so? Was it possible to split the political regime spectrum along a less liberal / more liberal dimension? And what would this entail for correlates of democratization as compared to correlates of liberalization? Were they the same, or different?

With these and other questions in mind, I set out on a journey to grasp what I meant when I thought about "political liberalization" processes, and what specifically was it that bothered me from the existing literature on autocratic regimes. Overall, this literature is excellent, and has greatly expanded our knowledge on the causes of stability and instability in non-democracies. I perceived it as a quantum leap from interval-level measures of democracy and autocracy, which I thought were lacking validity when it came to justify the positioning of political regimes along the whole spectrum, and especially in the middle ground. I found any classification on the "degree" of democracy or autocracy of a polity to be too loose to be valid.

Also, I was much less interested in exploring the "variations within democracy" according to their degree of liberalization: I regarded such studies to be related more with questions of how institutions worked in practice than with their specific components, and I thought they were too dependent on the investigator's operationalizations, conceptualizations – even beliefs.

Witnessing the rebirth of the autocratic literature catalyzed my interest for two reasons. First, my generation grew up within the cultural milieu of the "unipolar moment" of the Nineties, which brought with it the great (though not always sincere, or consistent) push for global democratization. But we matured academically during a period of stuck democratic transitions and autocratic retrenchment.

Initially, I wanted to know what had gone wrong. Then, after some time, I decided to give up describing incomplete transitions as "gone wrong" at all: there should have been a reason why some countries moved away from repressive autocracy, did not completely democratize, and at the same time found some sort of stable middle ground that allowed them to resist in that state for decades. They were not "hybrids". They were not "mixed systems". They were not a residual category. They had to be something else.

The second reason why I was drawn towards the study of autocracies was that I perceived the renewed push to understand autocratic regimes as a huge leap forward as compared to many other studies that, at least to me, appeared to be excessively driven by normativity. As partially open autocracies proved to be more resilient than expected, scholars had to accept to know much less than they thought, and possibly that their own beliefs had been misguided.

Problematizing our knowledge about autocracies was our way forward. This thesis is my outcome in that quest.

Chapter 1 reviews the comparative politics literature on political regimes, from the end of World War II up to the most recent past. It attempts to shed some light as to how the discipline evolved, in terms of research questions, research design, and methods. It then delves deeper into the classical findings on the correlates of political regime type, placing them into five general categories, and finds that most of the mechanisms described to justify their inclusion into the democratization literature make them suitable candidates to be included into my "liberalization" research question.

Chapter 2 attempts to formalize political regime choice as a two-player game with signaling. It starts from the assumption that an autocrat and the median citizen have different preferences over the distribution of resources within a polity, but that the autocrat is uncertain as to the precise preferences of the citizen unless it receives a signal. This explains why some autocrats may choose to endow their polity with institutions (such as elections, parliaments, less biased courts, and so on): interested in maximizing the probability to remain in power, autocrats trade off regime strength for knowledge that may help them reduce their total costs.

The game maps outcomes onto a tripartite political regime space: repressive autocracy, open autocracy, and democracy. I simulate comparative statics and draw some hypotheses from them, while I draw others from the literature. In particular, the model suggests that one should find more liberal political regimes at average levels of inequality, while resource rents accruing to the state may push in both directions – meaning that they may either push towards more liberal or less liberal political regime types. Other key hypotheses are drawn from the modernization theory, the regime diffusion literature, and studies on democratic (time-clustering) waves.

In Chapter 3, I survey interval-level indexes of democracy and autocracy and then pick up the two that appear to be the most robust. The first is a version of the Polity index, as corrected for a huge weakness that may skew results, especially when the index is employed in panel-data settings. The second is the Unified Democracy Score index, which is a meta-index that collects information from a host of other indexes of democracy, trying to gauge “expert consensus”. The chapter serves more as a review of the literature on research questions and research designs, and sets the stage for a deep dive in the most recent studies on autocratic types.

In Chapter 4, I survey the literature on autocratic types, highlighting its great strengths and some of its weaknesses. I then search for measures of degrees of liberalization, or “regime openness”. Being unsatisfied with existing measures, I build my own. To construct my index, I look at both institutional and de facto conditions within each polity. Institutional features comprise the mode of effective executive and legislative selection, the status of the legislature and the degree of legalization of political parties. De facto features include the actual existence of parties, the actual existence of an opposition, and the way in which parties are actually represented within the legislature.

I then use my interval-level measure to divide the political space into a tripartite typology that tries to map onto the typology in Chapter 2, by identifying closed autocracies, open autocracies, and democracies. For robustness, I build more than one typology employing different thresholds, and show how descriptives vary as thresholds change.

Chapter 5 focuses on measuring the political leverage that governing actors derive from fuel rents. I survey the political resource curse literature, and show how most of the operationalizations that have been used to gauge the political leverage of fuel rents are misguided or unsatisfactory. I therefore develop my preferred measures, building them up from a host of different sources and employing multiple imputation techniques (see below). At the end of this process, I regard my measure as not just valid but much more reliable than alternatives. I conclude describing how fuel rents, and the political leverage actors may derive from them, have evolved in space and time.

Finally, in Chapter 6 I develop empirical models to test the hypotheses derived in Chapter 2, together with other related questions on the stability of political regimes. The first part is dedicated to multiple imputations, which allow me to do away with listwise deletion or simple imputation techniques in cases of missing data, which are an even worse problem in a panel data setting.

Empirical findings confirm most of my hypotheses, allowing me to adjudicate between competing theories on the correlates of political liberalization. My findings also generate novel and controversial insights over the modernization, regime diffusion, and "regime waves" literatures. Most importantly, I find that some correlates of liberalization can interact in crucial ways with regards the choice of political regime and the timing and likelihood of transitioning to that specific regime.

To make just one key example, I find that higher economic well-being makes it much more likely for a polity to choose more liberal political regimes – in accord with the modernization theory –, but at the same time it greatly decreases the probability of regime transitions. This raises the possibility that some polities remain "stuck" with unwanted political regimes, as forces can push both ways. This, in turn, underlines the crucial role of agency. Under specific circumstances, single personalities or groups of people may act to catalyze change and move the polity in a more liberal direction, or to stifle any liberalization move in the first place.

Chapter 1. Political Regimes

Democracy, Autocracy, and Their Correlates

1.1. The study of democracy and autocracy

The study of political regime types, their features, their assets and liabilities dates back to antiquity and classical philosophy. Political philosophy thinkers frequently pondered about which political institutions and overall political regimes worked best for polities as a whole, for citizens, or for rulers.

Classical thinkers inextricably linked regime types to morality. Plato contrasted Socrates' own thinking to that of the sophist Thrasymachus, who appeared to defend an idea of justice as "nothing else than the interest of the stronger" (Ferrari 2000) – a position clearly resonating with modern and contemporary thinkers, and with sceptics of all ages. It takes most part of *The Republic* for Plato (and Socrates) to defend a universal idea of justice and, from there, propose and defend the best political order that would fit such high moral standards (the republic of the philosopher-kings).

Aristotle, with his two-fold tripartite classification of political regimes (good: monarchy, aristocracy, constitutional republic; perverted: tyranny, oligarchy, democracy) and his practical reasoning that distinguished between his ideal preferences towards monarchy and his pragmatic preferences for a constitutional republic, also betrays an underlying normative reasoning.

Even less than three centuries before our time, David Hume, sceptical philosopher *par excellence* who went as far as disputing the most fundamental politico-philosophical concept of his times, i.e. the social contract theory of political obligation, seemed to be very little troubled to assert in his *Essays*, in an act of absolute normativity, that “free governments” should be preferred to “absolute governments”, and that the best government is a federal “well-tempered” (representative) democracy (Haakonsen 1994). This stands as evidence that the moral push towards a universal ideal of the “best” political institution for any polity in the world remained a consistent feature of centuries of political thinking.

It is only normal, then, that normative thinking continued to pervade the comparative politics discipline, and still does to a certain extent. The fall of the USSR and nascent democratic transitions both in Eastern Europe and Sub-Saharan Africa unleashed a wave of optimistic thinking (for Sub-Saharan Africa, see e.g. Lindberg 2006 for one of the final optimistic stories, soon-to-be overwhelmed by the course of current events, Lynch and Crawford 2011), that peaked into a prominent Western liberal thinker foretelling the imminent “end of history” and the “universalization of Western liberal democracy as the final form of human government” (Fukuyama 1992).

Historically, it has taken the emergence of multiple “anomalies” at the positive level, and multiple failed attempts at bringing to life and propagating a social ideal of a “best of all possible worlds” at the practical level, for most scholars in a scientific discipline to start doubting engrained ideas before finally embracing change. Kuhn, frequently regarded as the scholar advancing the theory of “scientific paradigms” and abrupt paradigm shifts, emphasized this point more than once in his most renowned endeavour (Kuhn 1962), generalizing it to within-traditions paradigm shifts: “[anomalies] can also be retrieved from the study of many other episodes that were not so obviously revolutionary”, and concern “the far smaller group of professionals affected by them”).

In what could be described as the most recent case of a “paradigm shift” in the study of the nature and evolution of political regimes, it took the realization that the post-Cold War democratization push had produced at best only a gentle “fourth wave” (after the so-called “third wave” of the Seventies-Eighties, see par. 1.2.5), at constant risk of falling back towards less liberal political regimes, for the literature on autocratic regimes to be brought back to the fore.

Scholars and policymakers alike had to come to terms with the fact that it may be much more difficult, complex, or even ultimately impossible to pinpoint the features of the best political regime, for all ages and all peoples, and that even in the face of high economic growth, secularization and the expansion of public discourse thanks to the internet in some countries, this could make democratization much less likely than was expected at first.

Most of all, it took the failure at “exporting” idealized versions of political regimes to allied and hostile governments alike, by advertising their merits while playing down transitional and implementation risks, for scholars to widely recognize that autocracies (or “failed” democracies) might deserve another go and should not be regarded just as simple precursors to better, complete, or perfected political regimes.

These short-term undercurrents also brought to the fore the realization that, as centuries have gone by, what the highest thinkers of their age regarded as almost ideal examples of “democracy” (e.g., Athens for Greek philosophers) would fall far short of any contemporary definition of democracy – even to a minimal (or procedural) definition that only required free and fair elections. Slavery, the exclusion of women from politics, and the frequent limiting of political decisions to the wealthy restricted political participation (and civil rights) in ancient “democracies” to levels that we only encounter in autocracies today. As we come to realize that political regimes’ normative appetite can change wildly over time, we are also progressively brought to recognize that democracies do not tend to significantly outperform autocracies under many respects – including economic growth.

On the face of these complexities, this section aims at looking briefly at the evolution of scholarly thinking on political regimes and institutions since the end of the Second World War. I will then shift to a reflection over social sciences in general, and the interactions between structural and agency explanations of political outcomes. Finally, I will trace in broad strokes the state of the literature on political regimes today, identifying avenues for further research.

1.1.1. Comparative politics and the study of political regimes after WWII

Normative thinking, the thought of what "ought to be", is always present in human activity: not even science can be regarded as devoid of normativity, even when scholars do their best to leave preconceptions and biases aside and try to observe things "as they are" and not "as they should be". Moral judgment and ethical preferences run below every choice we make – even below the choice of embarking upon the study of social sciences in the first place. Theory and empirical analysis can never be completely insulated from pre-judicial choices during study design, concept definitions and operationalisations, sample and variables selection, data collection, et cetera.

Given that it is impossible to free oneself from any moral judgment, it could be argued that trying to do so when referring to "social facts" would either be naïve, morally wrong, or both. However, to the very least, the aim to set up "social sciences" requires that one strives for a certain detachment from her/his object of study. But while the attempt to approach the study of political regimes in a sufficiently accomplished and scientific way could be traced back to at least John Locke (Tuckness 2016), and others may trace it even to Hobbes or Machiavelli, the thought of these scholars is actually imbued in normativity (Burelli 2015). Indeed, it would not be before the rationalization (and data collection) attempts by Max Weber and a number of other sociologists and *ante litteram* ethnographers that comparative politics would start to settle down into something more similar to a science, and to be institutionalized within academia.

When the first wave of post-World War II political scientists embraced behaviourism, a deluge of data took the discipline by storm. However, these studies mostly focused on personality types and social groups. The natural outflow of this tsunami had social sciences progressively diversifying. In the first decades after WWII, the discipline was still commanded by "political sociologists", who prioritized the development of theory and higher-order concepts above the study of men or, at most, social groups (Mitchell 1967). At the same time, the advent of bipolarity and the onset of the Cold War spurred the development of game theory and its first applications to international politics (Schelling 1960). Comparative politics, still far from being termed this way, began to consolidate around a “political sociology”, which Lipset and Bendix (1959) came to define as the analysis of “the stability of a specific institutional structure or political regime – the *social conditions of democracy*”. Also, social scientists started to focus more and more over contemporary politics in order to identify their discipline as a separated field from historical studies (Munck 2007 reminds us of the motto of political scientists of the time: “History is past Politics and Politics present History”), and to focus upon state institutions in order to differentiate their research subject from sociology. Correspondingly, the latter veered towards the study of social groups and distanced further from state actors.

Narrowing the subject matter was important in order to establish the field of political science. This is also one of the reasons why domestic politics and international politics were separated by such a sharp divide at first. On the one hand, it is true that the question of whether specific, measurable ideological, cultural or socio-economic forces could influence the choice of social groups between radically different institutional regimes, such as democracies or dictatorships, continuously resurfaced over time (see the seminal Moore 1966). On the other hand, most often the comparative political field focused its “comparisons” on sufficiently similar alternatives: usually, different institutional features of democratic systems. This ultimately remains the focus of the discipline to this day (see Lijphart 1999/2012, William Roberts Clarke 2013).

As comparative politics slowly evolved, the study of political regimes continued to be confined to the study of a subset of regime types and polities. For some scholars, authoritarian countries were uninteresting simply because they were too different to be comparable to domestic politics in democratic regimes. For others, authoritarian countries were just a *deviation* from the optimal path (see below). Other times still, the problem consisted in an utter lack of data, not just in correlates but even in the bare knowledge of the specific “patterns of authority” experienced by non-democratic countries. Even when data existed, it was sparse at best, and anyway most countries had gained independence just five or ten years earlier, having been colonies beforehand.

As time passed, however, important attempts at capturing differences along the autocracy-democracy spectrum started to emerge. One of the most systematic, the Polity study, was launched by Ted Robert Gurr in the late Sixties and resulted in a book exploring “patterns of authority" in most of the independent countries of the world at the time. The aim was to provide social scientists interested in the study of political regime types with a structural, systematic analysis of the institutional settings and the practical conditions characterizing authority within each single country (or "polity"; Eckstein and Gurr 1975). The first edition of the Polity dataset was released in 1978.

Five years earlier, Freedom House started publishing its *Freedom in the World* index, which also attempted at gauging different levels of political rights and civil liberties in the world – relying upon a systematic expert survey.

As for the release of data updated with sufficient frequency, in 1968 Arthur S. Banks started publishing and circulating widely the Cross National Time-Series Data Archive (Banks and Textor 1963), while Charles L. Taylor released new versions of the *World Handbook of Political and Social Indicators* (Taylor and Hudson 1972; Munck 2007).

As comparative politics grew as a separate subfield of political science, normativity still remained profoundly engrained within the scholarly community. The degree of normativity within the discipline could be gauged just by looking at the titles of some the most important books produced in the last decades of the Cold War on the subject of political regime types and their correlates. Take, for example, Juan Linz's (1978) *The Breakdown of Democratic Regimes* and, on the other hand, Guillermo O'Donnell and Philippe Schmitter's (1983) *Transitions from Authoritarian Rule*. Democratic regimes break down, while authoritarian countries "transition" towards democracies – overall, a better world for everyone. If such inclination could be justifiable in terms of rights gained or even just total lives lost due to repression, it still was not a conclusion reached via science, but through normative judgment. Meanwhile, however, even comparative politics was getting more scientific. Empirical studies started to be produced ever more frequently. But it was not until the end of the Cold War that an entirely new discipline emerged, and consolidated to be almost consecrated as the landmark of the discipline: democratization studies.

1.1.2. The end of the Cold War: rational choice and democratization studies

The last years of the Cold War and the abrupt collapse of the Soviet Union brought about a profound shift in the comparative politics field. First, an increasing number of countries was transitioning towards democracy. In the earlier part of the Eighties, the share of autocratic countries over total independent countries continued to remain constant, or even slightly increased as new countries emerged from former colonies and, often, adopted autocratic regimes more frequently than democratic ones. But by the end of that decade and the early Nineties, the collapse of the strongest Communist regime and the dissolution of the Soviet Union, first, and of Yugoslavia in a few years, disclosed a unique opportunity for countries whose societies appeared to be ready for liberal democracy and a free market economy after decades of oppression, censorship, and material deprivation.

Before democratization studies could properly take off, another advance had to be embraced by the field. Indeed, the huge developments brought about by the spread of rational choice theory to political science (Tsebelis 1995), and the steady development of large-N quantitative methods (Achen 1983, King 1991) had been slow to penetrate a field which was still largely monopolized by former sociologists and historians, scholars generally interested in case studies or small-N studies at best who looked with mistrust at how economics was taking over the whole field (Przeworski 1991 could be thought of as an "early adopter"). Moreover, it was still difficult to study autocratic countries with a fair degree of precision, due to a profound lack or unreliability of data, caused by the level of closure of autocratic regimes at the time. A negative feedback loop had made social scientists even less interested in collecting data for those countries. Autocratic countries were the object of politics, of diplomacy, progressively more of historical studies; less so of political science scholarship.

It would take around a decade for data to gradually start dripping in (for example, the first edition of the World Bank's World Development Indicators was launched in 1997) and for formal game-theoretic, rational choice models to appear more frequently and be employed in the study of political regime choice. Consequently, large-N studies increased both in quantity and quality.

Even in the face of science "pervading" the field, the logics and ideology of democratization studies were still the probable outcome of a normative push. As the US appeared to emerge as the undisputed ("lone") superpower in the post-Cold War world, Washington's penchant for its own version of liberal democracy proved a compelling attractive force for other countries. The US appeared to be no more constrained in the support of democracy and human rights by the logics of the balance of power and the need to preserve the bipolar order. Such neoliberal push, which throughout the Eighties appeared to be increasingly contrasted by proposals for a New International Economic Order, was immediately propelled to mantra by Bretton Woods international economic institutions. The IMF's conditionality norms are most famous for their push in favour of free market reforms, but they also called for political liberalization, at least to a certain extent (Stone 2008).

Also thanks to this turn in the international milieu, in the early Nineties scholars and observers alike watched as one by one many countries transitioned towards democracy. Setbacks were still pervasive, but an indisputably higher share of countries was rallying around the liberal banner – or at least trying to do so.

As new data on autocratic countries increasingly became available, democratization studies were born. Researchers tended to focus on how democracy occurs and on whether, and how, it had been or could be supported from abroad (i.e., by specific third countries or by the international community, usually identified with the United Nations).

Przeworski (2000) aimed at systematising the knowledge within the field by settling debates on the main correlates of democracy and democratization. At the same time, the first seminal works on the theory of political regime choice and dynamics were springing up, laying game-theoretic foundations to comparative politics questions (Persson and Tabellini 2000), and reaching political regime studies a few years later (Boix 2003, Acemoglu and Robinson 2006).

Broadly, democratization studies trace back the causes of regime change to three different factors: structure, process, and culture. Structural theories highlight distributional shifts due to socio-economic changes that can empower actors that were previously at a disadvantage, or the role of external actors that may influence the regime change process. Here, broad socio-economic conditions are key, and the place for agency is muted in favour of theory generalizability.

Process theories, instead, study paths and actor's interactions that brought about regime change, emphasizing the steps that were conducive to (more) stable democracy and those that resulted in failed attempts at democratization. Cultural studies, finally, highlight the role of individual or collective agents, especially elites, that became pro-democratic and tried to shift the domestic balance of power in their favour. In section 1.2 I will show how these general frameworks of analysis can differ in their conclusion over the correlates of democracy and autocracy, but also how they frequently overlap.

Apart from these broad differences, what is important from the point of view of the whole discipline is that democratization studies slowly but consistently brought again to the fore the acknowledgment that political institutions are endogenous to the political process. While domestic political scientists tend to take the most fundamental institutions of a polity as given, these studies question whether some determinants can bolster or undermine them, affecting their overall durability.

It could be stated that, on the one hand, domestic political scientists are interested in equilibria when institutions are stable (broadly static, or changing slowly over time), and exogenous shocks are generally limited to changes in the number or preferences of veto players within a given institutional setting, or to even more specific policy changes. On the other hand, studies of political regime choice are interested in times of "constitutional crisis", as their shocks concern the whole "political space" – the very framework within which political life takes place and is organized within a polity.

1.1.3. A renewed interest for autocratic types and their correlates

Finally, over the last decade, a renewed push in the understanding of autocratic regimes has started to blossom, and is now in full bloom. Historical causes of this shift can be traced back to the increasing number of "failures" of transitions towards democracy, as autocratic regime "failures" (even those that were facilitated or directly brought about by international military interventions, like the US-led invasion of Afghanistan in 2001 and Iraq in 2003) did not lead to stable democratic regimes.

In fact, most transitions stopped at the "open autocratic" stage (according to my later categorization – see Chapters 4 and 6), or simply to autocracy by most of the existing dichotomous measures of political regime type. These failures made both political actors and scholars much more aware that democratization processes are frail, especially during the transition phase. Democratization attempts can upset consolidated cultural traditions, be met with resistance by previous elites (whether they were at the government or at the opposition in the previous regime), and generally take time and considerable political, military, and financial resources.

International events showed that previously democratizing countries could remain stuck in the middle of the process (at least as early as Zakaria 2003), sparking a doubtfully helpful literature proposing to add to the democracy-autocracy partition a third category of poorly-defined "hybrid" regimes (Levitsky and Way 2002, who then went on to postulate the definition of "competitive authoritarianism"; a refining of the concept would ultimately result in the much more useful idea of "electoral authoritarianism", see below).

Some countries failed to consolidate their democratic transition, while others relapsed into autocracy, often following coups d'état (see, e.g., the very recent example of Thailand in 2014). Today, Freedom House findings appear to confirm that at the same level the democratization process appears to have been blocked for years now, or even being in retreat in some regions (Freedom House 2014). Confronted with autocratic retrenchment in countries as diverse as Kazakhstan, the Philippines, Russia, and Peru, studies started to focus on the obstacles that these countries encountered in the democratization path in this and previous decades. The very focus of democratization studies started to shift. Ultimately, scholars grudgingly had to come to terms with the fact that, in spite of all the gains democracy could in theory grant to the general welfare of a polity, democratization had not turned into a one-way road, and setbacks deserved to be studied in their own right. To the very least, studying democratization setbacks was needed to deepen knowledge on social, economic, and political processes that hindered the consolidation of democratic transitions.

Thus, a renewed blossoming of autocratic studies was born out of the resilience of autocratic regimes in the face of democratization "waves" (Merkel 2010) and the rediscovery of the troubles that specific democracies experienced in transitioning from previous autocratic regimes (Linz and Stepan 1996). The new studies were built around the quest to uncover factors that made autocracies more durable (Brownlee 2007). These were accompanied by a normative change which was subtle at first, but appears to have strengthened over time: today, democracy and democratization scholars appear to have put their preferences aside, acknowledging with pragmatism that some countries are much harder than others to be "converted" to the "benefits" of democracy, or even questioning whether these benefits are present in the first place (Clark et al. 2012). Autocracies deserved to be studied also in order to understand what factors increased the likelihood that their citizens, as well as power groups that generally have a crucial role in democratization processes such as the military and the business elite, continued to prefer the current autocratic regime or were not able to overcome collective action problems in the same way than other countries did.

At the same time, studies of regimes that were neither fully democratic nor fully autocratic took off from the acknowledgment that international efforts to promote democracy produced, at best, countries that were stuck in this limbo, or *terra incognita*. These moved, again, from normative questions: how was it that countries would not democratize in the face of (at the time, at least) clear and measurable benefits for the population and, possibly, even their leaders? Second, being a weird mix of "pure" ideal types, how long would it take for these hybrid countries to "decide" which way to lean, i.e., to fall back towards autocracy or to properly democratize once and for all (Ottawa 2003)?

After all, studies on the stability of hybrid regimes (or "anocracies", as they came to be called early on by scholars relying upon combined scores from the Polity index) appeared to show that these regimes were more prone to civil war and political instability (Muller and Weede 1990, Krain and Myers 1997, Hegre et al. 2001). Therefore, they were expected to transition sooner or later towards more stable regimes, either outright autocratic or more fully democratic.

The unexpected resilience of such middle-ground regimes instead encouraged further debate (Gandhi and Vreeland 2004). After more than a decade has passed from early attempts at new conceptualizations and categorizations, it seems high time to trace a brief outline of the literature produced by this new strand of comparative politics studies.

First of all, scholars needed a way to typify the political space into new categories, partitioning the autocratic field into new dimensions which did not necessarily mirror the unidimensional "level of autocracy" proposed by quantitative indexes such as Polity. The increasing dissatisfaction with univariate measures that tended to clump all autocracies towards very similar autocracy/democracy scores was problematic to scholars who, having thoroughly studied autocracies and having focused on specific case studies, found considerable variation in terms of institutional configurations, leadership types, the specific role of elites and other veto players, or the way in which governing figures/groups legitimized or ring-fenced their power from outsiders (and from democratization pushes).

Back to the drawing board, researchers focusing on autocracies started to re-elaborate and refine previous typologies, peculiarly discarding the presence of strong ideologies as a defining trait of autocratic political regimes. Classic post-WWII typologies of political legitimacy in authoritarian countries, such as Friedrich and Brzezinski (1965), in fact relied upon the presence or absence of strong ideologies in order to tell apart totalitarian regimes (those displaying ideologies that are imposed upon a mobilized population) from "traditional" autocracies (in which rulers only aim at maximizing the benefit they can extract from office, but do not display a characteristically identifiable ideology).On the one hand, these studies drew inspiration from the rise and fall of totalitarian regimes in the first half of the 20th Century, which had spurred a flurry of literature – mostly on political philosophy, such as Hannah Arendt's – on the means through which these regimes legitimized their stay in power. On the other hand, such typological subdivision had clear political, normative motives: for Brzezinski in particular, who at the time was supporting Lyndon Johnson's presidential campaign, they were also intended to justify the US's condescending behaviour towards some autocratic (personalistic) regimes, while condemning others – which also happened to be US' enemies. The fact that both Brzezinski's and Jeane Kirkpatrick (1982) books, the latter embracing which embraced the same typology and underlying political framework, were written by actual or future high-rank US government officials only serves to restate the obvious.

Following a similar line of reasoning, however, other scholars were able to make important scientific contributions. Wintrobe (1990) attached a political economy model to the distinction between ideological and non-ideological dictatorships, tying the difference between one and the other type of autocrats to the level of repression they employed: traditional (what he called "tin-pot") dictatorships were characterised by rulers that only wished to minimise the costs of remaining in power in order to collect the benefits of office (thus employing repression at a minimum, "rational" amount), while in totalitarian dictatorships the leader maximises power over the population.

While totalitarian dictatorships appear to have faded as a viable possibility today (Acemoglu et al. 2010) the model's ability to explain the much lower longevity of military rule compared to other types of autocratic regimes (Nordlinger 1977) can be regarded as an early success in favour of political economy models of political regimes.

Over the last decade, however, the "new institutionalism" school did away with ideology entirely, arguing that the stability and durability of an autocratic regime strongly varies with autocratic regime types subdivided according to the degree of institutionalization or other non-ideological features.

First, these scholars argued, it is possible to distinguish between personalistic dictatorship, military juntas, single party/corporatist systems, and bureaucratic/technocratic political regimes. Some are more institutionalized than others, and their stability and durability partly depends precisely on such institutionalization level.

As regards political institutions, part of the literature on autocratic regimes maintained for a long time that they are just places that facilitate the distribution of rents to allied or potential opponents (Collier 1982), or window-dressing tools for the autocratic leadership to maintain power (Linz 1973, O'Donnell 1979). While this might be one of the purposes for the very establishment of some political institutions (such as legislatures) in the first place, recent literature has tried to "take institutions seriously". The aim is to rationalize the reasons why such institutions exist and resist in authoritarian regimes, while at first glance they may appear to risk undermining the legitimacy or the cohesiveness of the ruling elite. This literature treats institutions, and especially formal political institutions (legislatures, parties, and elections), as places or events where compromise and co-optation can occur between the ruling elites and the potential opposition (Gandhi and Przeworski 2006).

Formally, there are mainly two mechanisms through which institutions might prop up the chances for autocratic regimes to "survive":

a) more institutionalised regimes depend less on leaders. Therefore, when these leaders die or are ousted, succession mechanisms are already in place and can be relied upon in order to legitimize succession (Geddes et al. 2014). This decreases the likelihood of intra-elite infighting resulting in attempts at overturning the current political regime, and/or a lower likelihood of protests by the general population;
b) institutionalized autocratic regimes may include some acceptable "venting mechanisms" for the opposition (elites and citizens alike) to express their discontent in a manageable way for those in power. In particular, the literature emphasized the peculiar and important role played by: limited participation to some governing decisions (Magaloni 2008), the presence of legislatures that allow a limited presence and organization to the opposition (Gandhi and Przeworski 2006), free but not fair elections (Levitsky and Way 2002, Gandhi and Lust-Okar 2009), or a limited tolerance for political protests or expressions of political dissent (Magaloni and Wallace 2008, Kendall Taylor and Frantz 2014, Rød and Weidmann 2015).

In a word, countries that might at first appear to be more prone to transition towards democracy, or at least to experience more instability being not entirely autocratic and presenting a certain degree of partially liberalized institutions, under certain conditions might instead be best suited to withstand political, social, economic or other shocks and, therefore, to survive through critical periods.

Namely, each autocracy (and, one might add, any political regime) has three ways to survive changing conditions: being consistently considered as legitimate; repressing or managing dissent; and co-opting political opponents. Legitimacy, repression, and co-optation are the three tools that governing elites must juggle in order to remain in power and, more generally, political regimes must rely upon to survive. Clearly, under democracy the "legitimacy channel" is the most resorted to, being enforced through the periodic holding of elections, a higher degree of checks and balances constraining governing actors, and legal rules to terminate governments that may fail to respect the meta-political norms of that particular polity. However, co-optation mechanisms are certainly at work even in durable and stable democracies, while repression must clearly be left as a tool of last resort when all else fails. Under autocracy, instead, co-optation and repression are far more frequent, while legitimacy tends to be far more difficult to garner and preserve (Gerschewski 2013, Gandhi and Przeworski 2007).

Now that more than a decade has passed from this latest blossoming of autocratic survival and its correlates, one may regard this literature as already almost "classic", with datasets on autocracies like Geddes et al. 2014 being cited over 150 times in less than two years from publication. This field of research however still has to grapple with the fact that, while scholars have focussed on democratization trends and on autocratic survival, very few studies appear to have been interested in studying whether common causes may induce a country to "liberalize" in a general sense.

Sticking to a bipartition of the political regime space, at least ideally, between autocracies and democracies, some comparatists have been blind to liberalization trends, instead referring to countries that did not fully democratize as being "stuck" in a nobody's land. Other scholars decided to categorize countries that did not fit neatly within the bipolar policy space as just "hybrid": this, in turn, appeared to create a lack of interest in studying similarities and differences within this residual category. Finally, autocratic studies have "appropriated" these hybrid non-democracies and started to place them in different categories in order to study their resilience in the face of different institutions, behaviour, origins, or comprehensive political "setting" of a country.

This points to a road that still has to be followed to its very end. To my knowledge, scholars have yet to attempt to place "pure" autocracies and "hybrid regimes" over a spectrum of different levels/degrees of political liberalization, and then study whether: (a) the correlates of democratization are the same as, or similar to, the correlates of liberalization; (b) the mechanisms that may push autocrats may decide to liberalize are similar, and to what extent, to mechanisms that bring about democratization.

This is precisely what I will try to do from Chapter 2 onwards. Before that, however, my aim is to delve deeper in the correlates of political regime type as identified in past and current literature on the correlates of political regime type. This is important in order to preliminarily check whether some mechanisms and correlates identified as fit to influence the decision of the actors of a particular polity to democratize or not, may be already fit (or only require minor adjustments) in order to study the mechanisms and correlates of liberalization trends in general.

Although I try to stray from normative judgment, as stated before no scholar can be regarded as being only a "positive scholar" – certainly not so in a social sciences setting. Therefore, I prefer to interpret my work also under an ethical/moral light, and to set my preferences straight instead of being subtly guided (and possibly biased) by them. Normatively, my opinion is clear: liberalization is always to be preferred, whether this brings to proper democratization, or whether it simply brings an authoritarian country to employ a more tolerant strategy in order to guarantee its survival. Pragmatically, not all countries or all elites may prefer democratization, or be able to enforce and preserve democratic institutions in practice.

I must also acknowledge that it is true that, by partially liberalizing, a country may be anticipating some liberalization today but postponing democracy in the future. This is the reason why the total inter-generational "liberalization level" is difficult to calculate: what is a country giving up tomorrow by ensuring some degree of liberalization today, and how should we value liberalization within an autocracy as compared to democratization? However, it is precisely this uncertainty that makes me be more prone to favour (any kind of) liberalization today than proper democratization in an uncertain and possibly very distant future.

I believe that, even normatively, scholars should re-evaluate liberalization conditions and make do with what they find in the world today. Our preferences for a world free of tyranny and oppressions should be acknowledged, but not get in the way of our scholarly research. Our studies should be as theoretically sound and as empirically robust as possible.

Our quest is to describe how the world *is* and *why* it is so; not how we would *want it to be*.

1.2. Correlates of political regimes and political liberalization

Over the course of the last century, comparative politics scholars have advanced a huge number of tentative explanations for what causes a country to democratize, what enhances the likelihood that it stays democratic in the medium-to-long run instead of "falling back" into autocracy, and more recently what tends to stabilize political regimes in general.

The movement towards a progressively more scientific comparative politics literature, and a growing focus towards medium- and large-N studies (see section 1.1.2), uncovered a number of correlates of democracies and autocracies, ranging from domestic political, institutional, social, economic, and ethnic variables, to regional and international factors, to more structural effects such as the structure of the international system or wave-like regime change chains.

At the same time, very few variables have been found to correlate with specific regime types in a consistent and unequivocal manner. This is certainly something not unique to the comparative politics literature: it is typical of studies in fields as diverse as psychology, medicine, or even the hardest sciences of all, such as theoretical physics, to uncover contradictory results, as debates rage on for decades at times. However, social sciences are most prone to controversy given that human behaviour is hard to predict and even harder to study. This is all the more the case whenever controlled experiments are impossible or unethical to undertake, so that causation is much harder to tell apart from spurious correlation (Desposato 2015).

Debates notwithstanding, in the following section I will outline some of the most robust correlates of democratization, the mechanisms proposed by authors to explain their importance, and their robustness to statistical testing.

First of all, however, a preliminary caveat. Given the difficulty to ascertain causality, researchers have employed various methods with different degrees of success in order to dismiss spurious relationships and, hopefully, leave just causal links. The first and most consistent method adopted is to employ panel data instead of cross-sectional data, and justify the jump from correlation to causation by lagging independent variables so that correlates occur before the outcome variable. This is called Granger causality, from the Nobel laureate in economics Clive Granger. Granger (1969) proposed that the belief of causality and not just correlation could be reinforced by the ability of any variable to predict future values of another. To be sure, this method has huge liabilities: the direction of causality could be the opposite, and one would still find a significant correlation in the series in case of slowly moving variables. However, this is the first and the most common test in comparative politics – again due to the difficulty, outright impossibility or unethicalness of controlled experiments. History occurs just once and we cannot rerun it, changing one or more correlates while keeping others fixed – in other words, we never have a counterfactual at hand.

Although some movement towards "natural experiments" and the employment of instrumental variables in order to control for endogeneity has seen some progress over the last two decades, finding an instrument or a natural experiment still requires considerable time, effort and most of the times still results in controversy, requiring it to be thoroughly defended through a number of assumptions.

This is why it is important to stress that correlates and causal mechanisms are intended here as *facilitating conditions*. Statistically, those variables we believe to bear upon the choice and stability of political regimes should be found to increase likelihoods, but leave an important – and generally very wide – space for agency. It will always be possible to find "deviant" cases of democracy and autocracy, i.e. countries that are democratic in spite of a lack of facilitating conditions or an abundance of hindering conditions, or vice versa countries that are autocratic in spite of the presence of many factors that would facilitate democratization (Doorenspleet and Mudde 2008). Unobservable structural factors, together with agents' preferences not captured in their utility function (see Chapter 2), play a role in establishing whether facilitating conditions will ultimately act as a "trigger" for regime change, or whether things will remain as they stand despite them: as I will find in Chapter 2, and show extensively in practice in Chapter 6, political regimes prove highly resilient to changing conditions.

All this underscores the puzzling place for case studies in the today's comparative politics. Indeed, case studies, or even comparisons among a relatively limited number of countries in the world, tend to become more useful in the earlier stages of analysis and theory development, as heuristic tools that help scholars to identify hitherto missing *explanans*.

They may also serve as a way to complement large-N quantitative studies, when variables are difficult to measure, or when the underlying mechanisms are conceivably too complex to be modelled – although in the latter case a much bigger burden of proof falls onto the case studies' authors, considering the increasing availability of indicators for the most diverse research fields, and recent advances in formal modelling.

Given that case studies move from national or regional histories and try to simplify and contrast them in order to highlight possibly overlooked factors or general dynamics, they tend to lose relevance in the later stages of a proper comparative politics study, mainly due to the observational biases that they induce in a field that is already plagued by the paucity, or absence of viable experimental conditions.

In particular, the mere selection of cases to be contrasted is an intellectual operation that is based more on previous knowledge, precedent, and intuition than on systematic analysis. The frequency with which the United Kingdom and France's paths towards modern democracy have been called upon since the earliest stages of comparative political studies mirrors the eagerness with which, today, scholars call upon the two countries' different (average) relationships with their colonies to explain some current features of post-colonial nation states.

While this does not imply that fundamental insights cannot emerge from new case studies, it does appear to stack the cards against them as useful to infer some "general laws" on the structural determinants of political regimes and their stability. In short, case studies *per se*, and the generalizations that may be derived from them, appear no more than relics of a pre-scientific past of the discipline.

As human inquiry and ingenuity are constantly limited by the ability of the human mind to consider more than a handful of factors together – and typological models in political science clearly display the same limitations through the ubiquitousness of two-, or at best three-dimensional scatterplots (Dahl 1971, Huntington 1991) –, only statistical models, with all their issues arising from linearity assumptions, multicollinearity problems, and omitted variables biases, can be deemed useful in testing hypotheses in a joint and systematic way.

1.2.1. Path dependence and long-run conditions

The first set of conditions liable to have a bearing upon political regimes is rooted in history. In particular, this strand of research focuses on studying the development and consolidation of persistent institutions that tend to reinforce a particular set of norms and conditions thought to be preconditions to the development of properly democratic institutions.

Particularly fertile proposals appear to come from the literature of path dependence and "critical junctures". According to this literature, there are peculiar moments within a polity's history that tend to determine its future in terms of the likelihood that it will develop democratic institutions, together with a set of other more specific institutions, regimes, and norms. In these peculiar, defining moments, often occurring in a period of high instability or quasi-revolutionary conditions, political institutions are particularly flexible and pliable, and can be moulded much more easily by actors that can model them after foreign examples, or according to different principles than the ones that dominated under the previous regime. Once some forces are set in motion during these peculiar critical-juncture periods, they tend to be self-reinforcing, setting a country upon a particular path that it will be difficult to make it stray from further down the road.

The literature on sociological path dependence, which is sometimes referred to as historical institutionalism, draws heavily from economics (Arthur 1994, Mahoney 2000, Beyer 2010), and sometimes the authors in comparative politics even assume that the same mechanisms may be at play in determining the conditions within a polity than there may be in other fields that are clearly much more path-dependent, such as technology. Think, for example, to the Qwerty keyboard: after a period during which different kinds of keyboards competed for market share, the Qwerty keyboard ultimately emerged victorious. Although this keyboard may not be more efficient than others with different letter dispositions, by the time other optimisation attempts were done, operators had already become so proficient with the Qwerty setting that no other method could prove to be better and, even if it could, Qwerty keyboard were so ubiquitous that the cost to replace typewriters (and, later, computer keyboards) and training people outweighed any potential short-term gain. Another example may be car driving technology.

For path dependence to occur, initial economies of scale and positive feedback loops must reinforce a condition and tend to make it so hard to change that it sticks there for a very long time. In other words, they help a custom, condition, or institution to become “locked in”.

However, sociologists and historians have few examples of institutions self-reinforcing themselves and becoming engrained due to some sort of increasing returns, economies of scale process. Generally, institutions tend to be born out of unlikely events and get more and more engrained not through their “efficiency”, but because humans tend to get used to them. One might explain Christianity, or in fact any religion, through processes of “inertia and resilience” whereby small sects have had to fight for survival for decades or centuries, but then suddenly become predominant and even taken for granted (Schwartz 2004, Boas 2007). These institutions become “locked in” only after a certain set of particular conditions has realized that had little to do with increasing returns, but is in fact rooted in contingent decisions by powerful actors: in the case of Christianity, think of how Christians were emarginated for over two centuries, and then officially persecuted starting in 250 CE by emperor Decius. After around half a century, these policies were reversed by Galerius in 311 and Constantine I in 313, and Christianity finally became the state church of the Roman Empire in 380. Similar processes have characterized most heresies: it is very hard to tell which will be persecuted until being almost totally wiped out, and which instead will catch on and become a widespread alternative, possibly to the point of being officially recognized by whole societies or states (Koenig 2012).

Ultimately, this literature explains the locking in of institutions with the passing of time and historical junctures.

By their mere existence, some institutions tend to raise the cost of their own demise. Some studies within the democratic consolidation literature argued that proportional (versus majoritarian) representation and parliamentarism (versus presidentialism) may contribute to stabilizing democracy by providing losers of an election with the best chances to influence politics and, after some years, to wield more power directly by winning elections (O’Donnell and Schmitter 1986; Linz and Valenzuela 1994). At a higher level, Przeworski (1991) argued that proportional representation and parliamentarism would have a stabilizing effect by discouraging elites from trying to overthrow the elected government and install an autocratic regime instead: overall, according to him, parliamentarism "reduced the stake of political battles" and, this way, contributed to stabilizing democracy.

Some articles have argued against this form of stability-inducing consolidation, either because they found that institutions deemed to be stable were in fact subject to change much more frequently than thought at first (Alexander 2001), or because consolidated democracies were found to employ a wide variety of voting systems or government-parliament relationships.

Another strand of the literature emphasizes less path-dependence and more long-run conditions that build up and prepare a polity to democracy. Studies underline the role of culture, pre-state and/or colonial history, and sometimes religion.

Superficially linked to path dependence, these studies in fact approach the problem of the historical causes of autocracy and democracy by a wholly different angle. Path dependence postulates that an abrupt change, whose effects may be small in the short run, puts a country on a different path that is then self-reinforcing and becomes difficult to reverse.

When they highlight long-run conditions, instead, scholars point at features of a polity that may have been centuries in the making. Sometimes, such conditions pertain to single polities or ethno-social groups, but most of the times scholars highlight that common causes may affect entire regions or areas of the words, shaping their institutional preferences and ultimately their attitudes towards democracy.

Inglehart and Welzer (2003, 2005) found that orientations to trust, tolerance, and participation shaped the likelihood of each polity to adopt democratic institutions. At the same time, "patterns from a deep past" appear to correlate with contemporary political cleavages, discourses, partisan affiliation, and ultimately institutional choice in Central and Eastern Europe (Ekiert and Ziblatt 2013) and Western Africa (Owusu 1997)

More generally, some scholars tend to argue that culture may shape conceptions of political legitimacy. For example, Hinnebusch (2006) states that "it is plausible to argue that Islamic traditions accept authoritarian leadership as long as it is seen to serve the collective interest, that is, defends the community from outside threats and delivers welfare to which people feel entitled, and as long as it is seen to consult with the community (*shura*)".

Colonial dependence is also seen to possibly shape the future of a polity. Early studies, such as Crenshaw (1995), highlighted the importance of "proto-modernity" and the pre-industrial past that would lay the foundations to pluralism (something similar to Inglehart, see above), finding that colonial dependence and the imposition of democracy by the past colonial leader were in fact conducive to democratization. More recently, however, other studies (Wejnert 2014) appear to throw this finding into doubt. A similar question on the long-run role of colonial rule upon the likelihood of democratization may also be retraced in the debate on the effects of institutions on economic performance, whereby some argue that different colonization policies adopted by the colonial ruler have a direct effect on current income per capita (and, this way, on democracy through the modernization hypothesis, see below; Acemoglu et al. 2001), while others found even longer-run effects of geographic location over levels of income per capita (Sachs 2003; Haber and Menaldo 2011b).

Finally, Moore (1966) offers an interpretation that has to do with less long-run effects, but still spans decades if not centuries. According to him, social structure explains the political paths that states embark upon. Social structural analysis posits that democracy can be viable only when dominant classes and civil society achieve a balance, so that the state is influenced by dominant classes but not totally captured by them. For democracy to come to life, the dynamics of socio-political life need to bring to the emergence of a democratic coalition. Historically, this has been a mix of the middle class and the working class, where the former is concerned with expanding political liberalization, while the latter aims at broadening participation and substantive equal opportunities. Where dominant classes continue to subordinate lower classes, the outcome is a right-wing autocracy; while where workers (and agricultural farmers) revolt in a revolution, succeeding in overthrowing the dominant class and seizing power, left-wing authoritarianism results.

All these long-run causes can be easily generalizable to degrees of "liberalization", even short to democracy. In fact, just by looking at the Polity IV time series, one can see that the current stable European democracies took a long time to democratize and fell far short of democracy by contemporary standards for long periods of time. The liberalization process took decades, if not centuries, amid setbacks and democratic retrenchment. The UK and France offer two models of progressive or "revolutionary" democratization, and appear to show that only where democracy has time to develop and then consolidate, will it be less at risk of experiencing a "failure" towards a new autocratic experience (Tilly 2004).

Though fascinating, it is very hard to test theories of long-run determinants of political regimes. For one, these long-run causes constantly interact with contingent conditions, and it is often hard to separate potential equilibria that are "shocked" in the short term when, as it is the case here, political regimes prove to be quite durable and resistant to change (so that once they are shocked it may take years before they go back to their stable-state equilibrium). It is therefore hard to test whether some long-run equilibria are indeed present, or whether any institution is contingent upon present conditions that share nothing with the past but similarities due to the inertia and slow-movement of the correlates of political regime type and regime stability.

1.2.2. Socioeconomic factors

In 1959, Seymour Martin Lipset found a strong, positive correlation between democracy and the level of income per capita in a cross-section of countries in the world (Lipset 1959). Thus, the modernization theory was born. Just a few years later, Lipset managed to sum up his theory in a few, simple words: "All the various aspects of economic development – industrialization, urbanization, wealth and education – are so closely interrelated as to form one major factor which has the political correlate of democracy" (Lipset 1963).

The correlation between democracy and income per capita has proven to be so durable that it has become the mainstay of comparative politics handbooks, and of multiple attempts of testing it – sometimes with the specific purpose to refute it. Indeed, almost two decades ago Przeworski and Limongi (1997) found that income per capita could not predict transitions towards democracy: while the correlation resisted in explaining levels of democratization, and also appeared to enhance the likelihood that an already-democratic country would not fall back into authoritarianism (they called this "exogenous democratization"), it did not correlate in any meaningful way with the likelihood *to democratize* given that a country was not democratic yet (or "endogenous democratization").

Their finding was contradicted by Boix and Stokes (2003), who replicated the earlier study but corrected it in different ways, finding support for the modernization theory in both its exogenous and endogenous form. A second study form Epstein et al. (2006) also found support in favor of the modernization theory, but its reliance upon the uncorrected Polity IV index, and its focus upon partial democracies as opposed to full democracies, may have slightly biased their results – a replication with the corrected version of Polity that I present in section 3.3 might be in order.

Meanwhile, Acemoglu et al. (2008) found that the relationship held steadily at the between-country level, but failed to materialize at the within-country level. To them, this proved that the relationship was either spurious, or could not in any case explain the trajectory towards democratization undertaken by countries given changes in their income per capita. Eventually, Fayad et al. (2012) appeared to disprove their findings by employing a method that did not include country fixed effects, therefore preserving a sufficient level of variability and not discarding information from the dataset, while at the same time offering a method to control for time-invariant omitted variables.

While results conflicted, one great conceptual advancement today is that we are able to look at the modernization hypothesis from at least two different angles: does income level per capita make countries more likely to democratize, or does it simply support democracies into not falling back towards autocracy? Or is the relationship more complex still?

A second interesting relationship between democracy and socio-economic factors is that between democracy and inequality. The literature is highly diverse, and tends to come up with contradictory results. Acemoglu et al. (2013) find that, contrary to the tenets of theoretical models with a representative agent of the median voter, democracy does not tend to decrease inequality at all times, but does so only at certain conditions. At the same time, higher inequality does not tend to lead to democracy more often – for example due to popular revolt, or the emergence of a more pro-median voter elite that musters support from her.

Theoretically, the link between democracy and inequality has been covered by two competing theories. Boix (2003) argues in favor of the fact that democratization should decrease inequality, so that higher inequality should be found in more autocratic regimes. On the other hand, Acemoglu and Robinson (2006) argue that the relationship between democracy and inequality should follow an inverted U, predicting that only countries with average levels of inequality should tend to democratize.

A plethora of other demographic, economic, or social conditions have been proposed or found to influence the likelihood of a country to democratize: among them, it is possible to include education (Glaeser et al. 2006), ethno-linguistic fragmentation (Merkel and Weiffen 2012; but see Fish and Kroenig 2006), poverty (Sabatini and Arias 2007; but Krishna 2008 disproves it, showing that the relationship is mediated by changes in education levels), and urbanization (Lechler 2014). While for all these cases some scholars have found results contradicting the relationship, these correlations appear to be among the most robust to be found in the literature.

An important question that may be asked is: does democracy precede or follow these socio-economic correlates? In his essay, Kaplan (1997) argues that “democracy emerges successfully only as a capstone to other social and economic achievements”, while Ikenberry (1999) thinks that democracy is instrumental at making countries grow more peaceful and economically interdependent, thus improving the very socio-economic conditions that correlate with it.

Clearly, issues of timing and reverse causality constantly plague all questions of correlation between democracy and any other factor. But it is even more urgent to disentangle cause from effect when correlates can vary rapidly over the short term, as in the case of economic growth. The question here is: assuming that democracies tend to grow faster than non-democracies, do they do so because they are democracies, or is it that given that they grow faster, they had a higher likelihood to democratize in the first place?

Looming in the background is a further question still: do democracies really tend to grow faster than non-democracies? Some authors argue as much (Acemoglu and Robinson 2012), but the existence of important counterexamples since the last quarter of the 20th century works against their theory: how to explain, for example, Singapore, South Korea (starting to grow rapidly before democratizing), South-East Asian countries and, most of all, China? While growth in autocracies that depend on the export of primary commodities could be explained away once accounting for fluctuations in their price and decomposing resource and non-resource growth (Fayad et al. 2012), Acemoglu and Robinson's argument stumbles in explaining catch-up economic growth in relatively resource-poor autocratic countries.

The debate in the literature notwithstanding, all these factors, from slow-moving demographic conditions such as urbanization levels, to those changing much more rapidly, such as economic growth, appear to be liable to being extended to a test for liberalization conditions, and not just democratization per se. It would be especially interesting to understand whether correlates of democracy and autocracy are robust to liberalization movements even within autocracy – and whether theory may help us in tracing the relationship between socio-economic factors and liberalization levels, or between them and the likelihood of further liberalizing.

1.2.3. Domestic actors' characteristics and their structural relationships

Correlates of political regime type and regime stability may not just depend upon long-term or more contingent conditions of a particular country, but also on the relationship between relevant political actors within it. According to Vanhanen (2003), for example, democracy does not depend on a high level of socioeconomic development. Instead, it "presuppose[s] the distribution of economic and intellectual power among various social groups and their elites".

Possibly the most famous theory relating the structural relationships between domestic actors to effects upon the choice of political regime is the selectorate theory.

The selectorate theory gives up the distinction between democracies and autocracies, resorting instead to a classification over a two-dimensional space: the size of the selectorate and the size of the winning coalition. The former comprises the subset of citizens that can have a say in selecting the leader; the latter, instead, is the subset of the selectorate whose support the leader needs to remain in office (Bueno De Mesquita 2003 et al.). While the theory does not tell democracies and autocracies apart, it is actually fairly straightforward to map typologies of the bipartite democracy-autocracy space onto the bipartite space of selectorate and winning coalition sizes. This mapping shows that there is a notable difference between democracies and autocracies, especially in terms of the size of the winning coalition: in democracies, winning coalitions are much larger because to remain in office leaders need the support of the median voter. The theory goes on to predict that countries with relatively larger winning coalitions will tend to provide more public goods while autocrats will provide a mix of public/private goods depending on the ratio between the selectorate and the winning coalition.

One huge benefit of such a theory is that it suggests the need to cut across the political space, going beyond typologies of regime types based upon certain institutional or leadership conditions, such as the by-now classic tripartition of autocratic regimes between military, civilian, and monarchic (see Gandhi and Przeworski 2007 and Cheibub et al. 2008, to which Geddes et al. 2014 add the personalistic category). The selectorate theory suggests a compelling way forward, and highlights a narrow gap in the literature that still needs to be filled by competing theories.

Although very convincing, the selectorate theory is hard to test, because it is very difficult to gauge intangibles such as the size of the selectorate or the winning coalitions in autocracies, and it is hard as well to measure how the two vary year after year within a country (Gallagher and Hanson 2015). Also, earlier empirical findings derived from this theory have proven to be very sensitive to different operationalizations, and highly model dependent (Clarke and Stone 2006).

Therefore, while I am persuaded that domestic actors' structural interactions may shape the present and future path of a polity, included the choice of political regime, I am also convinced that the discipline is in need of further theoretical modelling that is liable to bear useful results or to uncover previously overlooked (or poorly modelled) relationships. The last decade has not been stingy with modelling attempts, as mentioned in section 2.2. Though important findings and insights can already be found in the current literature, I will argue that there is still space to shed novel insights upon the theoretical mechanisms of political regime choice, and to do so by modelling the specific relationships between a polity's actors, as other parameters vary. To me, this appears to be the best way to account for the structural role of agency, while clearly leaving some or even most space to the role of specific actors within a polity, or to personal traits of relevant political figures, that are impossible to generalize and even less to incorporate within a model.

The role of “relevant figures” should constantly and thoroughly be explored: but this is more the duty of historical studies than it is comparative politics’. Meanwhile, in the following paragraphs I simply focus on the identity of potentially relevant actors within both a democratic and an autocratic polity.

First of all, the elites. Italian scholars Gaetano Mosca and Vilfredo Pareto posited that power in a society is always fought over by a political oligopoly, composed of a number of elites. According to John Dewey, indeed, power will always be contended by elites, and democracy’s purpose is to manage the unresolvable equation between a power that is too disperse, and one that is too concentrated. Striking the proper balance is both hard and essential, and meanwhile a polity evolves around the current concentration of power – that one may formalize as the preferences of the ruling elites and the citizens’.

Among elites, in comparative politics one in particular stands out for its role: the military. The latter is often found to be either guiding the process towards democratization, protecting it from other forces; or (as a whole or as a subset) is found to be the author or guarantor of a coup d’etat that ushers in a new period of autocratic rule. Even in a democracy, the peculiar equilibrium in civil-military relationships has been extensively investigated in the literature (Finer 1962, Nordlinger 1977, Sundhaussen 2002).

Finally, and aside from specific groups that may merit scrupulous study, leaders are important *per se* for the survival and features of a specific political regime – especially for autocracies. While studies trying to account for differences in leaders’ psychology (Post 2004) do not properly belong to the comparative politics literature, recent studies on autocracies have focused on a leader’s way of demise from office, trying to account for systematic differences that may explain why they are ousted, how, and what makes them more or less resilient (Gandhi and Przeworski 2007; Kendall-Taylor and Frantz 2014).

To conclude, the literature on the characteristics, preferences, and role of various domestic actors appears to be important especially in modelling relationships in a more complex, refined, and precise way than it would be possible to do in plain words. While models will never be able to account for the wide variety of citizen-elite, or of civil-military relations, they are important to scholars as "mediators" of other correlates. Otherwise, correlates could only remain theoretically un-modelled, which entails that a researcher could never be confident that a linear relationship between democracy, or liberalization levels, and specific correlates is the best test for her/his theory, or that any other different functional form should be preferred to it.

1.2.4. International and regional conditions: diffusion and contagion

Scholars of comparative politics have often found themselves grappling with the fact that their discipline shares some elements with domestic political studies (classical political science) and others with international relations studies. Moreover, while initially comparatists were mostly composed of scholars interested in comparing the domestic political systems of two or more countries (or some specific institutions within them), it appears that nowadays more and more persons are drawn to study comparative politics after studies with a more "internationalist" profile (Golder and Golder 2015). I include myself among the latter category.

This is probably why, while scholars periodically acknowledged the need to account for international factors shaping or influencing the choices of domestic political actors (Rustow 1970, Ross and Homer 1976, O'Donnell et al. 1986), this relationship was seldom taken into serious account until about three decades ago. Starting from the Nineties a growing interest on the interaction between domestic and regional or international conditions gave rise to a small but important niche investigating political regime diffusion. This renewed interest possibly originated from the observation of current international events, showing that for most countries affected by conflict in many regions of the world, such as Sub-Saharan Africa, the Middle East and North Africa, or South Asia, political borders did not act as a watershed, and violence frequently spilled over from a country to its neighbours (Lake and Rothchild 1999, Gleditsch 2002). At the same time, diffusion arguments were bolstered by what appeared to be an acceleration of globalization processes (which would increase the frequency and intensity of interactions between countries) and a lowering of barriers between nation-states (increase of visa-free travel zones, international migrants and international tourists alike).

The hypothesis that political regime types, like conflict, tend to cluster in space has sprung out from the empirical observation that some areas of the world are more prone to democratize or remain democratic, while others are much more prone to remain autocratic or for democracy to remain fragile and constantly at risk of falling backwards towards autocracy. From here, a plethora of studies originated and were produced over the last two decades in order to assess whether regional diffusion processes were ongoing, both in terms of conflicts, and in the likelihood of choosing a certain political regime type – and the chances that it survives in a "hostile" or "benevolent" regional environment (see the appendix to Chapter 6 for a broader literature review and for an extensive discussion of the results and the problems involved in the study of regime diffusion).

The studies of regional processes that produce political regime clusters has however come up against a series of hurdles that need to be overcome in order to tell whether the observed clustering is the result of actual regional and international factors, or whether it is simply the result of the spatial clustering of other correlates of political regimes, such as GDP per capita, ethnolinguistic fragmentation, inequality, and so on. This issue, known as Galton's problem, has frequently been addressed (Buhaug and Gleditsch 2008), but it is hard – if not near to impossible – to come up with satisfying solutions, especially in the face of the actual clustering of correlates of democracy and autocracy, which complicates the disentanglement process. To this day, it remains very hard to test whether regional diffusion processes are actually at work or whether it is the clustering of domestic correlates that causes "second-stage clustering". Despite all this, even if it were caused for the most part by the clustering of domestic conditions, it would nonetheless be interesting to study why the correlates of democracy tend to cluster in space themselves.

Moving to a higher level of abstraction, that of the international system, international relations scholars have frequently posited that some configuration of international power (between unipolarity, bipolarity, or multipolarity), and the peculiar position and role of each country within each system, could shape domestic actors' preferences towards particular political regime types, or constrain their choices by removing some alternatives from their decision set. Boix (2011), for example, argues that “the structure of the international system affects the resources and strategies of pro-authoritarian and pro-democratic factions in client states”, and that “[t]he proportion of liberal democracies peaks under international orders governed by democratic hegemons (…) and bottoms out when authoritarian great powers (…) control the system”.

Indeed, it has been frequently recognized that the perceived strategic value of some countries within a definite international system made them an important target for outside attempts at influencing or outright imposing political regime types upon them. At the same time, great powers tend to promote their own regime type, imposing it upon less powerful countries (Owen 2002), while under other conditions they tend to ignore a country’s regime type in order to preserve the international balance of power (Levitsky and Way 2006).

In Chapter 2, I will try to model these conditions by positing that an abrupt change in the international system (in my sample, from bipolarity to unipolarity) changes the utility function of domestic actors. In particular, I will assume that autocratic leaders will tend to perceive increased benefits in leading the liberalization process themselves – irrespective of whether the final result is actual democracy, or just the opening up of a previously closed autocracy. This should occur in particular during the “unipolar moment”, the decade between 1991 and 2001 during which the United States could pretty much act as the undisputed hegemon of the international system, benefiting from the fall of the Soviet Union which allowed Washington to come out as the winner of the Cold War period. Sure, even during that decade the US experienced several setbacks, such as in Somalia, Rwanda, or the post-conflict reconstruction of Former Yugoslavia. But its role in promoting democratization, as deep-rooted interventionist and pro-democratic ideas took seat within the US foreign policy establishment, was pervasive, also aided by the expansion of liberal economic international organizations (Pevehouse 2002) and the use of foreign aid conditionality in order to encourage progressive liberalization of the target country (Dunning 2004).

After at least 2001, however, the US’ focus shifted from democracy promotion to direct regime change attempts in some hostile countries (Afghanistan and Iraq) and appeasement with other regional autocratic allies such as the Gulf countries. At the same time, rapid economic growth in some authoritarian countries (China and Singapore, not to mention resource-rich hydrocarbon exporters) appeared to show that democratization was not inevitable in order to prop up growth. China, in particular, acted as a strong counterbalance to pro-democratization movements, while its emergence also marked the end of the unipolar moment as its economic and military might has grown over the last decade and a half.

Both diffusion processes and the domestic effect of international structure and agency are liable to generalizations from democratization to levels of liberalization. In fact, it is striking how easily such arguments can be extended to comprise general movements towards a progressive opening of closed autocracies. On the one hand, autocrats wishing to preserve their power might pretend to initiate a democratization process, while stopping well short of properly democratizing their countries but still managing to "appease" both neighbours (in cases of democratic diffusion and regional pressures) and liberal superpowers. On the other hand, liberalization processes are often the only gradual, coordinated way that governing elites have in order to lead a sufficiently orderly democratization process, as sudden democratization might threaten to undermine their power or outright unseat them.

Therefore, liberalization processes appear to be natural extensions of democratization processes even with respect to regional diffusion and international determinants of domestic pressures towards liberalization or autocratic retrenchment.

1.2.5. Time windows, waves, and sequencing

A final source of "contagion" found in the comparative politics literature on democratization has less to do with regional diffusion and more to do with time. This strand of literature postulates that there exists a precise timing, not specific of a single polity but of the international environment as a whole, that can determine the fate of attempts to democratization, by increasing or decreasing the likelihood of their success.

In his famous 1991 book, Huntington described what he called a "third wave" of democratization that he assumed had started during the Seventies and was still ongoing. In order to explain this renewed push towards democratization, he pointed at changes in socioeconomic and ideological conditions, highlighting the role of the Catholic Church in

Communist countries such as Poland, coupled with international efforts to promote democracy by Europe and the US (Huntington 1991). This way, Huntington was explaining a trend as driven by a change of different factors, so that the wave was mainly exogenously driven by a wave-like change in the correlates of democratization.

However, it may be interesting to investigate whether waves, either towards democratization or autocratic retrenchment, are robust to controlling for the simultaneous change in other correlates.

This is equivalent to asking whether observed global waves towards democratization and back are completely determined by the change in other correlates, or whether a "time shock" domino effect is present, so that the trend cannot be entirely explained away by other correlates. In other words: are time effects significant, and do they tend to cluster in waves? Is the "third wave" evident along the whole 1970s-early 1990s time window suggested by Huntington, or is it significantly smaller? Are any other waves, either towards democratization or autocratic retrenchment, evident in recent decades, or was that a rare and peculiar event?

This is similar, but significantly different from "democratic diffusion" approaches that rely upon geography or network linkages (Starr 1991), and suggests the significance of time windows rather than changes in the configuration of neighbours' political regimes.

Teorell (2010) finds some support for waves occurring over time, but he also associates wave effects to regional effects, while I am interested in disentangling them, possibly controlling for both in the same multivariate setting.

Here, a "wave" can be explained as contagion through time, meaning that something happening in some parts of the world (arguably nearer than farther, but not necessarily so) increases the likelihood that it also happens elsewhere. In particular, once a certain threshold in the number of events is reached, emulation processes and a continuation of initial favourable conditions (conducive either to democracy or autocracy) may self-sustain, generating a ripple-like effect before dying out due to the exhaustion in the number of political regimes sufficiently near the "transition threshold". The distance from this transition threshold provides the required negative feedback that makes the wave come to an end, instead of reproducing to infinity. Apart from distance from the threshold due to the combined effect of correlates of democratization, emulation/learning processes may work in both ways: for example, in case of democratization waves, they may stimulate increased repression in countries that still have to democratize, or heighten fears in citizens, opposition elites and the military, in seeing that liberalization processes are stumbling elsewhere or do not coincide with initial expectations of rapid and complete democratization.

In the end, it appears natural to extend the “waves” hypothesis to a liberalization context, instead of relegating it to full democratization processes or complete authoritarian breakdown. This way, the nuances in a complex process might emerge more easily, and they may allow to uncover time trends that would be much harder to come about in a stark, bipartite democracy-autocracy setting.

1.3. Conclusion

Over the last three decades, comparative politics has made huge strides. At the same time, within the discipline the political regime choice-and-change literature has progressively caught up with more domestic-oriented research areas in its level of refinement and formalization. However, many gaps still need to be filled before the literature on political regimes can be deemed to have matured, finally and fully.

More specifically, as this chapter has shown, most of the processes and mechanisms that were proposed to explain the different likelihood in the success of democratization processes can be extended to more nuanced liberalization or anti-liberalization trends within each country, and this is a gap that strangely appears not to have been filled yet.

To study processes of liberalization does not imply giving up a normative preference towards full democratization. However, it does entail an acknowledgment that, in many countries of the world, conditions may not be ripe for outright democratization, as regime changes towards democracy have often failed to take root and consolidate, resulting in authoritarian breakdowns that were sometimes even less liberal than the existing regime preceding the democratizaiton attempt.

To say that conditions for a country may not be "ripe" appears to suggest that there are specific stages of maturity for every polity, and that democracy is only fit for countries that are "mature enough". In fact, my work aims at shedding some light upon those factors that correlate strongly with liberalization trends, and is especially focused on uncovering common correlates of liberalization and democratization. Whenever these two processes are related, one can be confident that progressive liberalization of autocratic regimes may lead to higher chances of democratizing later on – and, possibly, to a higher likelihood of stabilizing and consolidating fledgling democracies. On the opposite, when factors that facilitate liberalization are instead found to hamper full democratization, this finding will help paint a much clearer picture as to the set of conditions that may increase the likelihood of autocratic liberalization but hinder full, outright liberalization.

At the end of the day, studying the correlates of liberalization processes opens up a crucial and untrodden path. Throughout the following chapters, I will attempt to formalize the political economy game that describes political regime choices, draw a set of hypotheses liable to empirical testing, operationalize types of regimes over degrees of liberalization, and empirically test hypotheses related with the correlates of democratization and liberalization.

Chapter 2. Modelling the Survival of Autocratic Regimes

Signaling, Inequality, and Political Liberalization

2.1. Introduction

In this chapter, I identify an important gap in the extant comparative politics literature and suggest a way forward. The gap consists in the lack of formalization of some of the mechanisms through which a polity's main actors interact in order to determine a country's future in terms of its choice of political regime. This gap is particularly evident in the "political resource curse" literature.

In fact, theories on regime choice, stability and change, together with their determinants, have been the subject of a wealth of research in recent years (see section 2.2). This huge body of literature starts off from the premise that, in order to have a clearer picture of the mechanisms at play when different actors contend for power, and in order to explain their preference for a specific political regime (and its economic consequences, especially concerning the redistribution of the resources produced by its labor and capital endowments), we need to take a step back and have a look at the bigger context in which the political regime change "game" is played. Only through a formalization of the preferences, utilities, and strategic choices of the main actors in this regime change game can we hope to catch a glimpse at how a host of political and socio-economic factors affect the socio-political processes involved in polity creation – including the choice of political regimes in which thousands of other sub-games are played daily.

I first review some of the main attempts at formalizing regime change in the contemporary political economy literature, discussing their features and gaps. Most of the recent attempts go a great length at uncovering some of the relationships between regime type and specific variables, such as inequality or economic development. However, what tends to be overlooked has to do with the way in which informational asymmetry may affect the very way in which the "regime choice" game is played. Most importantly, we often lack a formalization of the ways in which powerful elites may want to measure the level of political dissent by citizens, and of the fact that autocratic leaders tend to be most uncertain about the level of support they may enjoy at any specific moment – absent some kind of signal.

Therefore, I propose an original way to recover political regime equilibria under a formal setting, given the peculiar features of a particular polity. My formal model sheds insights on some of the important mechanisms that we may expect to bear some significance throughout history, and which I will put to the test in Chapter 5. In section 2.6 I derive a set of formal hypothesis from the formal model, compare them with some pre-existing literature, and develop more hypothesis to be tested in the empirical models in Chapter 5.

2.2. The lack of formalization in the 'resource curse' literature

In the past four decades, the 'resource curse' literature has progressively differentiated into three theoretical strands. These strands are only loosely independent, as it is often the case that scholars conflate one another when discursively explaining the full effects of natural resources over politico-economic outcomes, or assume that one effect follows another in a stepwise manner (Ross 2012).

The first of these strands is the classic 'economic curse' literature (alternatively referred to as the 'Dutch disease' or 'oil curse' literature), in which scholars study the effects over a country's economic growth of a high dependence on exports of commodities in general. Given the strong empirical relationship found between some particular exported commodities and the tendency to follow boom-and-bust cycles and to depress domestic economic development of the non-export sector (crowding out investment there; see Arezki and Ismail

2010, Van der Ploeg and Poelhekke 2009), this field has specialized towards hydrocarbons and extracted minerals dependence. Generally, natural resource dependence is supposed to stifle domestic economic growth by decreasing the likelihood of its sustainability in the long run, as countries tend to focus on the export of a single primary commodity (or a small set thereof), leaving diversification and import-substitution aside. Moreover, dependent countries often rely on domestic subsidization of that very commodity, thus losing external competitiveness, supporting higher levels of domestic unemployment, and depressing long-term growth (Arezki and Ismail 2010, Corden 2012, Davis 1995, Frankel 2010).

A second strand, the ‘conflict studies’ literature, focuses on factors linking natural resources to higher levels of domestic (sometimes even regional or international) conflict. Highly-dependent countries are thought as more prone to experience domestic instability or conflict in general, because the more natural resources are present and readily available, the more this worsens both ‘greed’ and ‘grievance’ processes (Fearon and Laitin 2003, Fearon 2005, Collier and Hoeffler 2004).

Finally, it is possible to identify a ‘political regime strand’. Theorists here ponder on the possibility that natural resource-dependent countries may use their non-tax wealth (rents) in order to coopt their citizens, or pay the security apparatus to repress them more effectively without increasing taxes. This way, they may avoid fueling further discontent before repression can be enacted, or may more easily commit to promises to pay rises once repression has proven successful. The ultimate consequence is that, given that leaders enjoy more latitude thanks to natural resource rents, regimes in these countries tend to be more durable autocracies and/or less durable non-consolidated democracies, and therefore expect highly-dependent countries to face worse prospects at democratization or higher likelihood of authoritarian backslides (Ulfelder 2007, Wright et al. 2013). At the same time, other scholars suggest that the presence of non-lootable natural resources may increase the stakes of capturing the government through violent (or at least non-constitutional) means, thus undermining regime stability and potentially counterbalancing the benefits of power through the higher likelihood to face leadership challenges (Ross 2012, Tsui 2011). Scholars find even more reason for contention when ‘regime durability’ is replaced by terms such as ‘regime stability’, as this tends to conflate the mere expectation of a decrease in the likelihood of success of protests or coup attempts in undermining regimes in resource-dependent countries with the mere frequency of protests and coup attempts alike (irrespective of their likelihood to succeed).

Here, I focus on the regime strand of the literature. In this field, theoretical mechanisms have remained starkly underdeveloped for decades compared to the "economic curse" strand, which relies on carefully detailed and fairly complex economic models (for a review, see Auty 2001, and Caselli and Cunningham 2009). In general, and irrespective of a growing literature over the last 15 years, mechanisms explaining the "political regime" game remain underdeveloped also compared to the domestic politics literature, with its focus on electoral competition, veto players, coalitions, and political agency (including delegation).

It is not hard to retrace the reason why regime change has had to wait for a while before formalization attempts barged into the field. For one, the movement towards formalization took off from domestic politics in a democratic context, and chiefly from electoral politics, exactly because of its highly institutionalized, rule-based setting. The latter enhanced the plausibility of any further simplification: think of how realistic Downs' (1957) classical model can appear at first when compared to the predominantly two-party nature of the contemporary US political landscape. It was therefore somewhat straightforward to reduce this setting to a specific set of players, preferences, allowed moves, information distribution and (potentially) timing of move disclosure.

Second, as the setting was already pretty well-defined, with a limited number of actors present (President/Congress/electorate; a certain number of relevant parties; a "transmission belt" between the government, a specific ministry, and its bureaucracy), this enhanced the feasibility of any model. This made possible to preserve some of the complexity of a real-world context and still reach some meaningful equilibria given a set of free parameters and not too many (hopefully plausible) assumptions.

Under this light, it is also not difficult to understand why, in the aftermath of the Second World War, the perceived exceptionality of the international strategic and political context – which had been abruptly reduced to a contest between two superpowers and two opposing political and economic blocs after the long multipolar era – pushed seminal economists and sociologists to finally attempt to crack the code of international interactions (von Neumann and Morgenstern 1954; Schelling 1960).

Between the huge development of the domestic politics and the international relations fields (with a starkly lower degree of sophistication for the latter, save for some notable attempts; see Signorino 1999, 2002), and constant incursions by political economists formalizing the resource curse, the comparative politics field has remained relatively untouched by formal theory until the last decade.

When studying the determinants of political institutions of a particular polity, and what makes that outcome different from another in terms of political rights, civil liberties, electoral competition, etc., historians and sociologists have swept the comparative field. Few political scientists or political economists regarded the field as ready for formalization, possibly believing that political regime processes could not easily avoid narrative-driven generalizations that fit more with historical accounts and leave little to systematic comparison. All this happened while studies of civil wars, domestic conflict, and regime change were increasingly turning towards data-driven analyses, creating a mismatch between the growing number of empirical models and the lack of formal theories able to formally justify their choice of parameters and the formulation of hypotheses around them.

At the same time, some of the most appealing theories developed to explain political regime outcomes in a comparative context remained too disconnect from empirical testing. Take the "selectorate theory" (Bueno de Mesquita et al. 2003). It is safe to assume that every political leaders' main aim is to remain in power, and that he can also do this while maintaining support from his "winning coalition" (the subset of a country's population who directly support him while in power) and from the "selectorate" (the subset of people whose express or implicit consent the leader needs in order to avoid being deposed).

At the same time, the operationalization of such a vague and dynamic set of the population as the selectorate might prove too much for the theory, so that attempts at testing it always appear to suffer from huge measurement biases. On the other hand, some of the most important implications of the theory, such as the fact that autocratic leaders are expected to focus more on private goods, while democratic leaders are expected to focus more on public goods, while having been tested (Bausch 2014), appear trivial at best and can suffer from second-order measurement biases. In this case, it is very hard to distinguish between private and public goods – think of military expenses, which might be classified as public goods if they contribute to national defense, but would be very private goods when they are aimed at increasing repression capacity or at redistributing income towards a loyal military.

Despite this disconnect, during the last decade a growing number of seminal papers by political economists has contributed to the formalization of models of political regime change (Wintrobe 1998, Bueno de Mesquita et al. 2003, Acemoglu and Robinson 2006, Gandhi and Przeworski 2006, Cox 2009, Dal Bò and Powell 2009, Acemoglu 2010, Besley and Robinson 2010, Magaloni 2010, Boix and Svolik 2013, Edmond 2013). These days, scholars interested in studying the effects of natural resource dependence over regime stability and durability can turn towards a constantly expanding set of analytical and mathematical tools in order to close the gap between data and theory, and to support the development of comparative politics into a full-fledged, formalized, data-driven field where hypotheses can be derived from theory and are liable to empirical testing.

In our case, informal (i.e., narrative) theoretical mechanisms seem to suggest that natural resource dependence might enhance as well as stifle the likelihood of autocratic regime survival. One might hope that the progressive formalization of the theory upstream might lead to better model specification of econometric models downstream and, possibly, to better data collection.

2.3. Models of political regime choice

During the last decade, as the last 'wave' of democratization fizzled out (it is now regarded as at risk of receding, Freedom House 2014), the focus of comparative politics scholars appears to have shifted from explaining democratization and democratic consolidation processes towards a more neutral and less normative stance. The renaissance of autocratic studies has made progressive inroads, and with it has come a theoretical refinement of half-a-century-old concepts, coupled with a more precise and systematic unpacking of what is hidden in the grey area that the Polity IV index classifies as “anocracy” and “autocracy” (see sections 3.1 and 3.2).

As for autocracies, an important recent advancement in the literature concerns the decision to move away from the study of transitions *from* autocratic *towards* democratic regimes, and vice versa, in order to focus on the durability of autocratic regimes and the intra-regime durability of autocratic leaders. Studies have focused on transitions between different types of autocratic regime (e.g. from civilian to military authoritarian governments; Geddes et al. 2014), on the length of tenure of different autocratic leaders (Kendall-Taylor and Frantz 2014), and on the way in which power is passed along the line (in an institutionalized manner as opposed to a violent/unconventional removal of the previous leadership; Frantz and Stein 2012).

This recent impetus overlaps with a renewed interest in redefining the full spectrum of political regimes and the political institutions that characterize each regime. For example, after a lull that lasted almost two decades, the study of electoral (as opposed to ‘liberal’ and ‘substantive’) democracies has finally regained center stage, and is being complemented by the study of multiparty electoral systems and other political institutions under autocracy (Gandhi 2008, Hadenius and Teorell 2007, Levitsky and Way 2010). In short, scholars today are really trying to “unpack” autocracy and see what kind of generalizable concepts may have been disregarded by the literature on the ‘degree’ of democracy that rely upon univariate, or at most bivariate, indices.

Meanwhile, formal models of regime change and survival are slowly emerging and being refined. Reaching high levels of mathematical complexity, formal models aim at offering a simplified (but hopefully plausible) version of reality, and therefore require whoever builds them to make a precise choice about what to include in the formal structure of the model and what to leave out. As a general caveat, it should be noted that the consequences of adding or removing just a single model parameter can be hard to track, and even models with high levels of generality and few moving parts can degenerate quickly towards unstable equilibria, or offer no simple way to reach non-degenerate solutions without increasing the number of assumptions that underlie them.

One of the main basic assumption of political economic models is that conflicts between citizens and (military or civilian) elites arise out of a divergence about distributional preferences over incomes. Elites prefer to increase the share of administered incomes that will end up in their own pockets, for a number of reasons that also entail increasing the chances to remain in power. Citizens, on the other hand, would prefer an equitable distribution of incomes and the use of the remaining portion to generate public goods. In a sense, therefore, elites and citizens are seen as possessing diverging interests over one or more policies.

Many variables can have an effect on incomes. Incomes are generated through a production function, which is also a function of the tax rate. The tax rate is used by the elite(s) to extract a share of the income to be used by the government administration. It may entail producing public goods or financing private gains. Also, some resources may accrue to the government without taxing the citizen (too much): models refer to these as rents. Again, the leader will have to decide how to distribute these rents.

Formal models always rely on representative agents, i.e. collective actors representing a group of people that are considered to share similar, usually in terms of access to political power (e.g., citizens, oligarchic elites, the military, etc.). A representative agent has to embody the preferences of the socio-economic and political category it represents. In order to justify this logical jump from collective actors to a single representative agent, political economy models cannot enjoy the luxury of "micro founding" their models in the way current state-of-the-art economic models do (Smets and Wouters 2003, Woodford 2008).

They therefore need to assume that preferences at the micro-level interact in such a way as to make it possible to aggregate them at the macro-level most of the times. For the representative agent for the citizens, we must therefore suppose that there is a "median agent", or a "median voter", who embodies the policy preferences of citizens at a median income level and at a median level of many other characteristics. For elites, we must suppose that intra-elite conflict is absent, and that there is a (collective or single) representative leader taking all the decisions.

While both assumptions might appear highly implausible, we unfortunately would otherwise be unable to capture complex dynamics – without the model spiraling out of control –, without making many more assumptions along the way, thus limiting the degrees of freedom of many moving parts that we would like to keep free. As with any model, there is a natural tradeoff between accuracy and efficiency – between complexity and solvability.

If we are willing to commit to a representative agent model, we can see where it may bring us and what kind of testable hypotheses we may derive from it. Given a distribution of preferences over policies, a natural consequence of political economy models is that the representative citizen is always better off in a democracy than in any kind of autocracy, because in a democratic regime – by voting into office his preferred candidate – he could set a policy nearer to his preferences than under any other regime (although principal-agent problems might get in the way and skew the results somewhat). In turn, the autocratic leader does not want to be ousted, and will do whatever it takes (including repressing and/or coopting the citizen(s)) to survive, until costs exceed available resources. Therefore, representative agents clash over policies and, ultimately, over the power to decide upon and implement such policies. As a consequence of all this, if and when democratization occurs, it is the product of strategic interaction between political actors, and the elite always has a say in it. This is something we can justify from history, given that it is very rare to find historical cases in which democratization occurred through a bottom-up revolution (even the great sociologist of revolutions, Charles Tilly, recognizes exactly that, see Tilly 1978; also see Skocpol 2015).

Generally, formal models of regime change try to formalize a full economy, complete with taxes and decisions over the redistribution of resources (Acemoglu and Robinson 2006, Alesina and Tabellini 2007, Acemoglu et al. 2010). On the one hand, in my proposed model I will not do away with redistribution; on the other, I acknowledge that conflicts can depend on a much more complex function of actors' needs and perceptions. Therefore, one may want to allow for a model that refers to 'political ideal points' in general, and not just to economic ones, even at the cost of sacrificing some economic complexity.

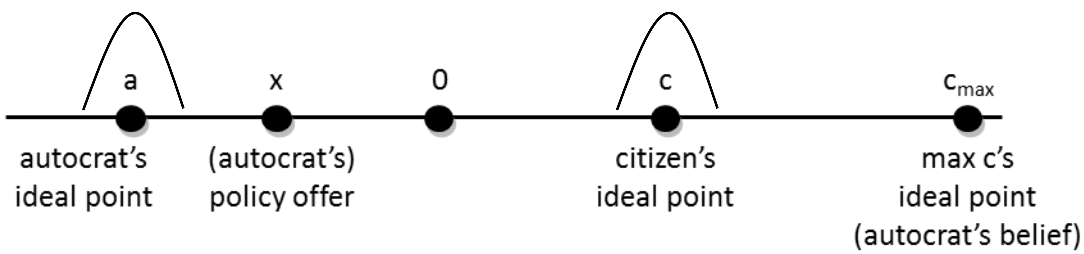
Also, in most previous models, actors usually possess perfect information about present policy preferences of other actors in the game (an exception is Svolik 2009). Uncertainty is introduced through indefinitely repeated plays of the same game into the future, and equilibrium is induced by modelling the agents' time-discounting features. Often, at the start of each subgame one or more actors are allowed to change their "type" based on a stochastic process, so that future payoffs and the future balance of power between actors remain uncertain and cannot completely be determined in advance.

In my model, instead, I introduce uncertainty by modelling an informational asymmetry between the representative agent of the citizens and the representative agent of the elites (the autocrat). Citizens continuously see the autocrat taking political decisions, so that despite propaganda they can be assumed to have a much better idea of the position of the autocrat's ideal point. In the model, this is simplified to perfect knowledge. On the other hand, the autocrat cannot "see" the citizens play by default, unless he allows them to express (signal) their preferences in some way – e.g. through protests or some form or another of media freedom. Therefore, depending on the features of the political regime, the representative agent can only have a certain idea of where the citizen's ideal point may lie on the policy space, and only occasionally and in very limited circumstances will he become aware of the true (median) preference of the citizens (Rozenas 2012). Signaling games of this kind have appeared very seldom and only very recently in the comparative politics literature: in my literature review, I was only able to uncover two (Dal Bò and Powell 2009, and Edmond 2013).

2.4. The workhorse model: signaling and the survival of autocratic regimes

I model the political economy game as a two-player game: a representative agent for the elite, the autocrat A (which can be thought of as whoever effectively holds power, be it a sole dictator, or an oligarchic regime or a military junta), and a representative agent for the citizens, i.e. the "median citizen" C. Both agents are assumed to move within a unidimensional policy space, and to have quadratic utilities centered around their ideal point.

Figure 2.1 – Visualizing the unidimensional policy preference space



Their utilities therefore directly depend on the distance between their ideal preferences and the actual policy x that is offered and implemented. The general form of their (unparameterised) utility function is thus:

$$u_a(x) = -(x-a)^2$$
$$u_c(x) = -(c-x)^2$$

The game assumes that between the autocrat and the citizen there is a distributional conflict, i.e. that the autocrat would rather concentrate as much rents and wealth in his hands and those of his winning coalitions, than distribute them to the general population in the form of public goods. I assume that there is no previous status quo, but that players are playing in order to determine the policy outcome: each political regime starts out as a *tabula rasa*, and players are playing (negotiating) the constitutional setting for the polity in that period.

As stated above, I also aim at incorporating informational asymmetry within the game: namely, I assume that the autocrat is uncertain over the exact preferences of his citizens. The autocrat must choose whether to trade off a portion of his power (more precisely, a portion of the probability of him remaining in power in case of a violent removal attempt) in order to receive a sufficiently precise signal from the citizens and narrow down uncertainty, or to retain all his power but remain uncertain as to the citizens' policy ideal. There is a definite benefit to receiving more precise information on citizen's preferences, because this way the autocrat can have a clearer picture of the risks it faces and decide upon whether to use repressive or cooptation methods in order to keep consensus and remain in power, or to lead a democratic transition instead so as to avoid to be violently ousted.

I assume that whoever is in power, whether the autocrat or the median citizen, receives a direct benefit from him staying in power. The citizen (here representing the whole democratic opposition) may attempt to violently remove the autocrat through a revolt, but must take into account that he will incur a cost, as well as the autocrat, and that this cost will be higher for the side who loses. This is a one-shot game, therefore it is played only once (an extension towards an infinite-horizon game might be an avenue for future research).

The game starts with the autocrat, A, choosing a political regime type between three possibilities: democracy (D), repressive autocracy (RA) or open autocracy (OA). If the autocrat chooses to stay in power in one of the two types of autocracy, RA or OA, he receives a direct benefit, $R > 0$, which incorporates part of the state's economic activity, including rents. If the game ends in D, the citizens get a direct benefit, D_c, irrespective of how the game ended there (through the autocrat's choice or a successful citizens' revolt). If the game ends in D because the autocrat chose to democratize, however, the autocrat himself earns a benefit, D_a. This is because we are assuming that, if the autocrat chooses a democratic regime, he will lead the transition instead of being ousted by rivals (either angry citizens amid a climate of social revolt, or militaries leading a coup). Therefore, the autocrat should see his chances to preserve some influence increased, either by maintaining government posts, or through an increase in relevance of the institution it leads in the transitional phase and, possibly, in the new democratic period. This may be the case in democratic transitions driven by the military – see e.g. Turkey during the Eighties, or many other occurrences in Latin America.

In each subgame, one of the actors (the autocrat in RA or in OA; the citizen in D) chooses a policy offer $x \in \mathbb{R}$, which is a function of economic, social, and political states, describing an offer of "general politico-economic conditions". We assume that, in case of violent removal of the autocratic leader and transition towards democracy, the policy offer chosen by whoever is in power is sufficiently close to the median citizen's preferences that it can still be represented by the citizen's ideal point, so that for our purpose of diversifying between democratic and autocratic conditions we can safely assume that the policy choice and the citizen's ideal policy point coincide. However, in case of democratic transitions led (or at least initiated) by the previous autocratic leader, the representative of the citizens will still have to make a compromise policy offer to the autocrat, so that in that case the policy is free to move away from the median citizen's ideal policy point. The policy offer is implemented straightforwardly in RA and D, so that there is no negotiation occurring between actors, while in OA the dynamics are slightly more complicated (see below).

As anticipated above, both players have an ideal point, which represents the best policy that could be implemented in their view (see Figure 2.1). Moreover, their utility functions decline proportionally with the squared distance from their own ideal point. The autocrat's ideal point, a, is perfectly known to both players: as long as the representative citizen's uncertainty around the autocrat's ideal point is much smaller than the autocrat's uncertainty around the citizen's ideal point, this simplification appears reasonable. On the opposite, I assume that the citizen's ideal point, c, is not known to the autocrat, who instead has only a vague idea of where it may lie in the policy space: he is sure that it lies on the opposite side from 0, as the model assumes that the political relationship between the autocrat and the citizen is always, at least latently, conflictual (no "benevolent dictator" or "enlightened absolutism" can really exist here). The autocrat has a precise idea of what the citizen's maximum preference, c_{max}, might be, and we assume that he generally guesses right, so that $c_{max} > c$. This does not mean that the autocrat can guess as low as he wishes: c_{max} is, in fact, a representation of how the information asymmetry may affect the game, and it cannot be controlled by the autocrat any more than he could force citizens to not rebel just by being confident that they will not. The autocrat's best guess of the position of the citizen's ideal point is assumed to be a random draw from a uniform probability distribution between 0 and c_{max}.

Once the autocrat (or the citizen) has chosen his own policy offer, the other actor can choose to revolt (if it is the citizen) or to try to stage a coup (if the autocrat). The choice of resorting to violent means is always costly, so that the actor employing violence faces a certain cost $k_i > 0$, and an additional cost $j_i > 0$ if the protest or coup is not successful in deposing the opponent from power. Autocratic strength can be measured by the probability that the autocrat remains in power in RA (α) or in OA (β), or the likelihood that a coup against democracy D succeeds (γ). We assume that regime strength is strictly higher in a repressive autocracy than in an open one: this is so because, while the autocrat gains insights on the citizen's preferences by allowing them to signal opposition, in OA citizens can organize and coordinate more freely. This is a liability to the autocratic regime in terms of effectiveness of the citizens' coordination efforts to solve collective action problems (for theoretical and empirical elaborations on this issue, see e.g. Cox 2009).

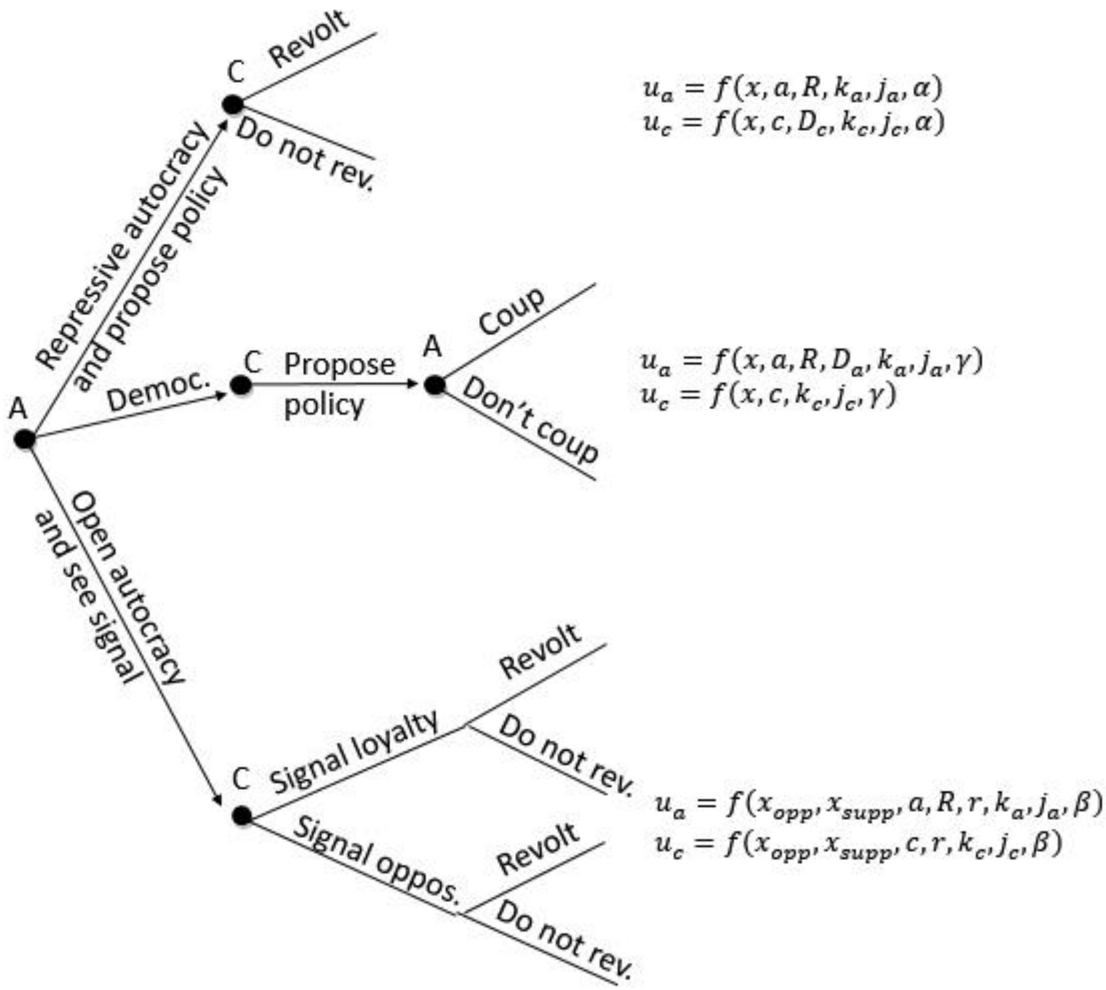
Therefore, if the autocrat chooses a repressive regime (RA) or to lead a transition towards democracy (D), there is a single policy offer and a choice between accepting the offered policy or resorting to violence (see Figure 2.2). Namely, in RA the autocrat will choose x, and the citizen/opposition will choose whether to try to remove the dictator and impose their own $x = c$; in D, the citizen will choose x, and the autocrat will choose whether to stage a coup.

If the autocrat chooses an open autocracy (OA), the structure of the subgame is a little more complicated. I suppose that in this case the citizens have a way to signal their support or opposition to the autocrat, and at the same time the autocrat will know with precision the citizens' policy ideal point. Thus, the subgame begins with the autocrat proposing a set of two 'policy choices' to citizens. The two offers are conditional on what the citizens will choose to signal when they will have the opportunity to do so. If the citizens signal opposition to the autocrat, they receive a policy concession, x_{opp}. If the citizens choose to signal support, they will receive x_{supp} plus a direct transfer r. The latter is a direct economic "concession" whose level is set by the autocrat. The autocrat uses r to "reward" the citizens of their support. If the median citizen signals support, however, the policy offer will be more distant from its ideal point, because we assume that the autocrat will take advantage of the knowledge that the citizen is willing to compromise some of its "policy" outcome (and the expected outcome in case he revolted and won) to gain a sure reward/rent, r. Therefore, we assume that $x_{opp} > x_{supp}$.

To sum up, in OA the citizen chooses whether to signal opposition or support, and he knows that, depending on his choice, the autocrat will automatic implement one of the two offered policies. As soon as the citizen chooses between these two actions, the autocrat will be able to know the position of the citizen's ideal point and will have more information on the cost-incentive structure of the citizen, thereby adjusting its strategic behavior to the new knowledge about the citizen.

Before proceeding further, I explicitly discuss three important assumptions of this model. The first assumption, which was already stated before, is that I expect that while there is a gain to the autocrat for choosing an open autocracy in which he gets a signal from his citizens as to the precise position of their ideal point on the policy space, there is also an important cost in that the characteristics of an open autocracy make the autocrat more prone to be ousted in case of revolt. This may occur, for example, because political coordination is easier: while the autocrat gets to know the citizens' ideal point, many citizens get to know the ideal point of many other citizens as well (for example as an outcome of not-too-rigged elections, or with the occurrence of popular protests that the autocrat chose not to stifle).

Figure 2.2 – Extended form representation of the one-shot game



Note. Payoffs are expressed as functions of key parameters. For the full specification, please see below the discussion in the relevant subgame section.

Therefore, I assume that the probability of a revolt being unsuccessful in OA (or alternatively, of an autocratic regime being strong under OA) is strictly lower than under RA:

Assumption 1. $\beta < \alpha < 1$.

Secondly, I need to remove trivial cases from my analysis. For one, if the autocrat has a policy preference that is too far away from the citizen's ideal policy preference, then the autocrat will always choose a repressive autocracy RA and set the policy offer to its own ideal point, irrespective of the likelihood that the median citizen's removal attempt is successful (and anyway minimizing that likelihood by choosing RA instead of OA). I therefore need to assume that:

Assumption 2. $a > -\sqrt{\frac{k_c + \alpha j_c - (1-\alpha)D_c}{1-\alpha}} - \frac{k_a + (1-\alpha)(R + j_a)}{4c_{max}}$

Finally, if the expected benefit from democracy to the citizen under OA is higher than the costs of revolting (including the expected costs of losing) under OA, the citizen will always revolt under any autocratic regime, so that the autocrat will have no incentive to compromise, and will therefore either choose a repressive autocracy and set the policy offer to his own ideal point ($x = a$) or democracy and put up with $x = c$.
I therefore need to assume the following:

Assumption 3. $D_c(1-\beta) < k_c + \beta j_c$

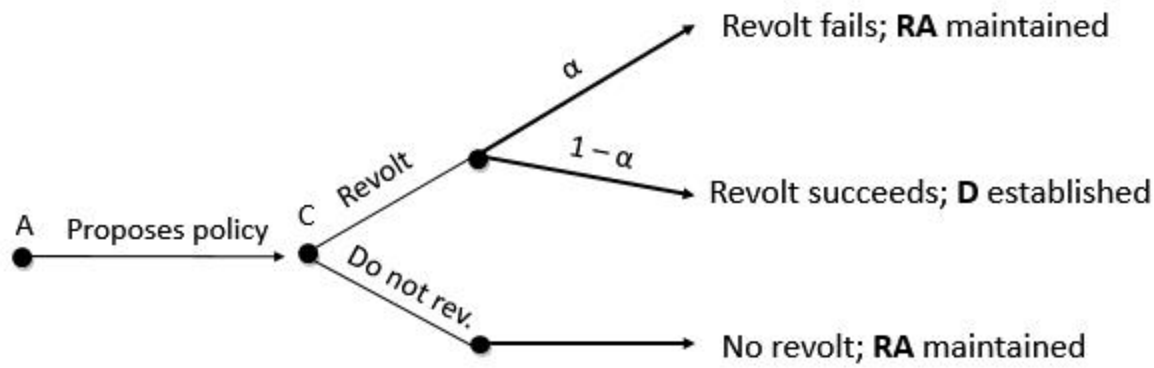
In order to solve the game, I first need to find each subgame perfect equilibrium, employing comparative statics. Instead of then finding the full-game solution employing perfect Bayesian equilibrium, which would both be cumbersome and prone to errors as I solve it step by step, in section 3 I will resort to simulations and graphical representations, in order to show most potential solutions that could materialize assuming some fixed values for most parameters and letting two of them vary freely.

2.4.1. Repressive authoritarian subgame

Once the autocrat chooses a repressive authoritarian regime, he will implement the policy x, the citizen chooses whether to revolt, and if he does he will succeed at regime change with probability $1 - \alpha$. When policy x is implemented, the autocrat's utility will be a function of the distance between its ideal point and the policy, and the direct transfer R: $R-(x-a)^2$. The citizen's utility will be a function of the distance between c and x: $-(c-x)^2$.

We also know that, in case of violence (i.e., in RA, citizens trying to violently remove the autocrat), each actor incurs a cost k_i, and that there will be a variable cost j_i incurred by whoever loses. In case the citizen tries to oust the autocrat but is unsuccessful, the autocrat keeps R and keeps on implementing x. Otherwise, if the citizen manages to oust the autocrat and install democracy, the autocrat loses R and the citizen gains D_c (see Figure 2.3).

Figure 2.3 – Extended form representation of the repressive autocracy subgame



Note: the figure only represents outcomes. Payoffs are described in the main text.

Therefore, utility functions are the following. For the autocrat:

$$u_a^{RA}(x) = \begin{cases} R-(x-a)^2, & if\ no\ removal\ attempt \\ -k_a + \alpha R - (x-a)^2 + (1-\alpha)(-j_a), & if\ removal\ attempt \end{cases}$$

For the citizen:

$$u_c^{RA}(c,x) = \begin{cases} -(c-x)^2, & if\ no\ removal\ attempt \\ -k_c + \alpha[-j_c - (c-x)^2] + (1-\alpha)D_c, & if\ removal\ attempt \end{cases}$$

Statement 1. In the RA subgame there is a single, unique equilibrium.

Let $l = \sqrt{\frac{k_c + \alpha j_c - (1-\alpha)D_c}{1-\alpha}}$.

1. The autocrat chooses the policy $x = x^*$*, where:*

$$x^* = a + \frac{k_a + (1-\alpha)(R + j_a)}{2c_{max}}$$

2. There is a removal attempt of the autocrat by the citizen if and only if:

$$|c - x| > l$$

Proof of Statement 1:

By backward induction, the citizen will try to remove the oligarch if (and only if), given *x*:

$$-k_c + \alpha[-j_c - (c-x)^2] + (1-\alpha)D_c > -(c-x)^2 \;\; \rightarrow$$
$$-k_c - \alpha j_c - \alpha(c-x)^2 + (c-x)^2 + (1-\alpha)D_c > 0 \;\; \rightarrow$$
$$-k_c - \alpha j_c + (1-\alpha)(c-x)^2 + (1-\alpha)D_c > 0 \;\; \rightarrow$$
$$(1-\alpha)(c-x)^2 > k_c + \alpha j_c - (1-\alpha)D_c \rightarrow$$
$$(c-x)^2 > \frac{k_c + \alpha j_c - (1-\alpha)D_c}{1-\alpha} \;\; \rightarrow$$
$$|c - x| > \sqrt{\frac{k_c + \alpha j_c - (1-\alpha)D_c}{1-\alpha}} = l$$

The autocrat chooses x so that it maximizes his own utility, which is:

$$u_a^{RA}(x) = P(c \leq x + l)[R - (x - a)^2]$$
$$+ P(c > x + l)[-k_a + \alpha R - (x - a)^2 + (1 - \alpha)(-j_a)]$$

By construction, the autocrat will never choose a policy which is further to the left from his ideal point (i.e. $x < a$), and he will choose something different from its own ideal preference point a if and only if this sufficiently reduces the likelihood that citizens try to depose him. Also, whenever the distance between c_{max} and l is too high (i.e. if a lies within this distance), the citizens will not try to depose the autocrat and the autocratic leader will again be able to set $x = a$.

When this is not the case and solutions are not trivial, the policy x will be chosen in order to maximize the autocrat's expected gain: this is a first-order condition. The maximum utility of choosing $x \neq a$ should then be compared with the utility that the autocrat would derive from choosing a and risk a removal attempt from the citizens.

Given that the autocrat's belief depends on a uniform probability distribution, we know that:

$$\frac{\partial P(c < f(\varepsilon))}{\partial \varepsilon} = \frac{f'(\varepsilon)}{c_{max}}$$

The denominator of the function is the length of the uniform distribution, which is $c_{max} - 0$ by construction, and identifies the autocrat's uncertainty about the citizen's ideal point position.

So the first-order condition (solving for the maximum of the function – therefore taking the partial derivative on the policy x and solving for it) is:

$$\frac{\partial u_a^{RA}}{\partial x} = 0\;, when$$
$$-2(x - a) + \frac{R}{c_{max}} - \frac{-k_a + \alpha R + (1 - \alpha)(-j_a)}{c_{max}} = 0$$

$$x = a + \frac{k_a + (1 - \alpha)(R + j_a)}{2c_{max}} = x^*$$

Given that the second derivative of this function is negative, this unique solution also represents a maximum of the function.

It is now simple to ascertain that the choice of x^* grants a higher utility than setting $x = a$. Indeed, by substituting the solution inside the autocrat's utility function, it is easy to verify that this holds true whenever Assumption 2 also holds (i.e., the autocrat has a policy preference that is not too far away from the citizens', given all other utility parameters).

Interpretation.

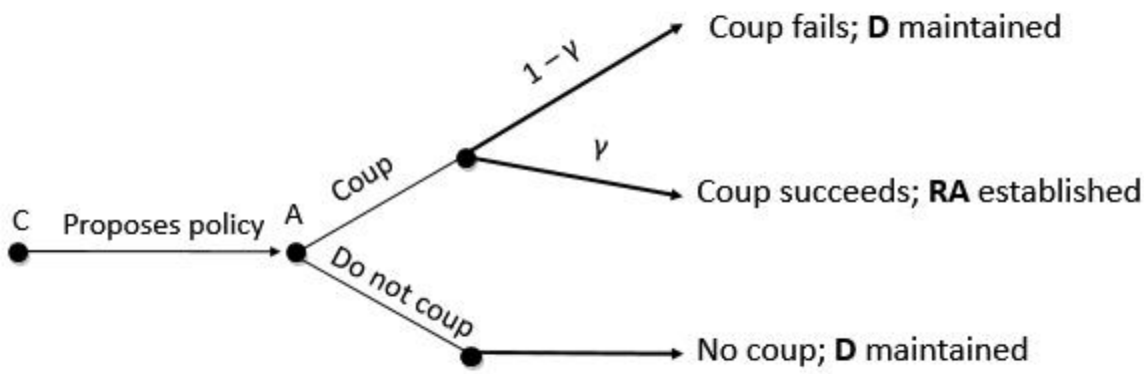
Once the subgame equilibrium has been found, note that the policy x implemented by the autocrat tends to be a non-monotonic function of c_{max}. This is because, as long as the autocrat's uncertainty remains quite low, the autocrat can increase x enough to guarantee that the citizen will not try to remove him most of the times. But the more c_{max} grows, the more the autocrat should choose a policy further from his ideal point in order to have a chance that the citizen does not revolt, and even then he would be less and less certain that his own concessions would avoid an ousting attempt by citizens as the maximum policy compromise the autocrat would be willing to choose would still tend to make only a marginal contribution to the citizens' utility function. Therefore, once a certain threshold is crossed, the marginal benefit of increasing x to avoid an ousting attempt starts to decrease, because each marginal increase in x is incrementally less likely to help the autocrat avoid ousting attempts.

Also note that the citizens will increasingly attempt to remove the autocrat, the more the distance between the implemented policy and their collective ideal point grows, so that (by construction) the more the two players of the game's preferences diverge, the more likely it is that the citizens will attempt to oust the autocrat.

2.4.2. Democracy subgame

This subgame works in a symmetrical way as the RA subgame. This time it is the citizen that will have to make a decision upon the policy to be set and "offered" to the autocrat. However, in this case the citizen knows the autocrat's ideal point. Once the policy has been set, the autocrat will choose whether to try and stage a coup (see Figure 2.4).

Figure 2.4 – Extended form representation of the democracy subgame



Note: the figure only represents outcomes. Payoffs are described in the main text.

Following are the two players' utility functions. For the autocrat:

$$u_a^D(x) = \begin{cases} D_a - (x-a)^2, & \mathit{if\ no\ coup\ attempt} \\ -k_a + \gamma R - (1-\gamma)(-j_a - (x-a)^2), & \mathit{if\ coup\ attempt} \end{cases}$$

For the citizen:

$$u_c^D(c,x) = \begin{cases} D_c - (c-x)^2, & \mathit{if\ no\ coup\ attempt} \\ -k_c - (c-x)^2 + \gamma(-k_c) + (1-\gamma)D_c, & \mathit{if\ coup\ attempt} \end{cases}$$

<u>Statement 2</u>. *In the D subgame there is a single, unique equilibrium.*

Let $m = \sqrt{\frac{D_a + k_a + \alpha R - (1-\alpha)k_a}{1-\gamma}}$.

1. The citizen chooses the policy $x = x^*$*, so that:*

$$x^* = \begin{cases} a + m & if \;\; a + m < c < a + m + \sqrt{k_c + \gamma(D_c + j_c)} \\ c & otherwise \end{cases}$$

2. There is a coup attempt by the autocrat if and only if:

$$|x - a| > m$$

<u>Proof of Statement 2</u>.

Again, I use backward induction, and rely upon the same logic as before, but with the autocrat and the citizen switching sides. What changes, apart from both roles, is that the citizen knows the policy preference of the ex-autocrat, and can select *x* with this information at hand.

The autocrat will try to subvert democracy and return to autocracy if $|x - a| > m$. What is the citizen's choice in light of this common knowledge? Whenever $c \leq a + m$, the citizen can set $x = c$ and fear no coup, because the autocrat will prefer the citizen's choice to facing the uncertainty of a coup attempt. However, as c grows, the citizen can no longer set the policy to his ideal point without risking a subversion attempt. Also, given that *c* moves further away from a, the citizen is discouraged from risking facing a coup, which would make him incur both the costs of violence and, if the coup ends up being effective, would set the policy choice away from him. However, as c moves even further away from a, the benefits of avoiding a coup attempt start to rapidly decrease as this would cost the citizen increasingly more in terms of policy compromise. Therefore, the citizen will again tend to choose $x = c$ and risk a coup attempt. This will only happen when:

$$-k_c - \gamma j_c + (1-\gamma)D_c - 0 \geq D_c - (c - a - m)^2 \;\; \rightarrow$$
$$c \geq a + m + \sqrt{k_c + \gamma(D_c + j_c)}$$

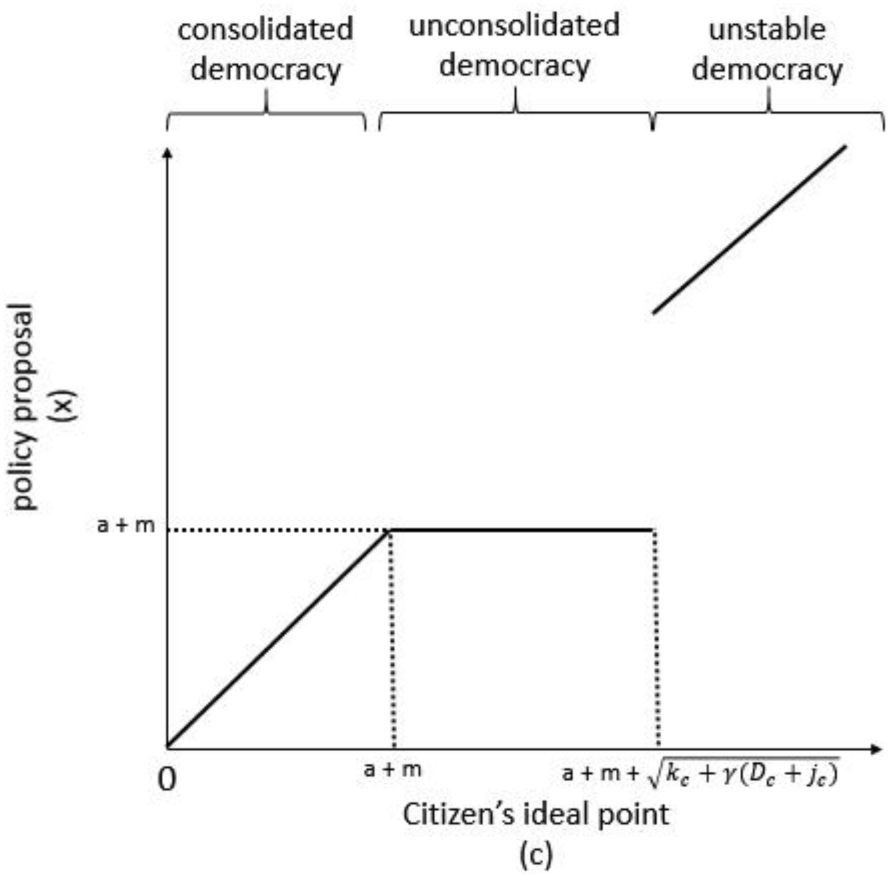
Interpretation.

Figure 2.5 is a graphic representation of the equilibrium of this subgame in terms of policy choices. *Ceteris paribus*, when *c* is low enough, the citizen can set *x* at his own ideal point and the autocrat will never attempt a coup. For middle values, when *c* is within a certain threshold, the policy offer *x* needs to be kept fixed at a value lower than *c* in order to guarantee no attempt by the autocrat to overthrow the democratic government, and this policy compromise is still attractive to the democratic government compared to the expected loss in case a coup occurs and succeeds. Finally, after *c* rises above a second threshold, the citizen will always prefer a coup to compromising its policy position, and therefore it will set *x* at *c* again, and wait to see whether the coup will be successful or fail.

The labels above the figure allow us to make a quite tempting interpretation of this game's possible outcomes, conditional on c, a, and other ancillary parameters. When the policy preferences of the citizen and the autocrat are close enough, both the citizen and the autocrat will prefer a compromise. First, in the space that we termed as "consolidated democracy", the policy preferences are near enough that the citizen can set the outcome at its own policy ideal point and fear no moves to overthrow him, so that the outcome is the "best of all worlds".

In a second case, when policy preferences are still close enough, but the distance between them widens, the citizen will choose to compromise on a policy position that is still closer to its ideal point but gets incrementally further as differences between the citizen and the autocrat widen. Here, the outcome is a policy that does not coincide with the median citizen's own ideal point, and is therefore not optimal for the whole polity, but is considered a sufficiently good compromise and is preferred to the expected costs of a return to autocracy. Finally, as positions between the citizen and the autocratic elite get too far, no possibilities of mediation between the two exist, and the polity will certainly face a coup attempt. The outcome is therefore dependent on violence, and we label this condition “unstable democracy”.

Figure 2.5 – The citizens’ policy choices in the democracy subgame



The graph may also be read from right to left: in this case, it may be regarded as describing a path of progressive democratization and democratic consolidation, as differences between the elite and the median citizen narrow. Clearly, given that this is a static game, there is no possibility that policy preferences change over time. But a dynamic extension of this “democratic game” might shed some interesting insights on factors enhancing the chances that a polity democratizes, and others lowering the chances that democratization occurs or that the political system consolidates around democracy.

While a number of studies have investigated the relationship between income inequality and political regime type, they generally assume that the political regime type determines the inequality level. This can be attributed to a number of reason. Some relate it to the size of the selectorate, which tends to be larger in democracies and smaller in autocracies, therefore bringing about a bias towards higher inequality in autocracies (Kemp-Benedict 2011). Others argue that "ideological" autocrats bring about higher levels of inequality by relying upon other sources of legitimacy or repression to prop up their regime (Galbraith 2012).

Our model, consistently with most other political economy models that take off from a distributive conflict between two actors in a polity, posits that it is the distance between the median citizen and the autocratic elite that does the heavy lifting. Inequality determines the structure of political incentives (Boix 2003, Acemoglu and Robinson 2006). In particular, Boix 2003 argues that inequality tends to prevent both democratization and consolidation, while Acemoglu and Robinson 2006 draws from the theoretical model the conclusion that inequality is expected to harm consolidation, but expects inequality to be linked to democratization through an inverted U-shaped curve.

Arguments in favor of this hypothesis date back to Aristotle, and can for example be found in de Tocqueville, who stated that "[a]lmost all of the revolutions which have changed the aspect of nations have been made to consolidate or destroy social inequality" (de Tocqueville 1951). Within the democratization literature, the implications of my democratic subgame model appear to agree with recent empirical findings that high inequality levels can harm democratic consolidation attempts, albeit some also find that this does not affect the likelihood of democratization (Houle 2009).

2.4.3. Open authoritarian subgame

In an open authoritarian regime, the citizen has to choose whether to signal support to the autocrat, getting a "reward" but accepting a policy that is further from him, or signal opposition, receiving some concessions in terms of policy (the chosen policy will be nearer to his ideal point) but no reward. I therefore assume that, by construction, $x_{opp} \geq x_{supp}$.

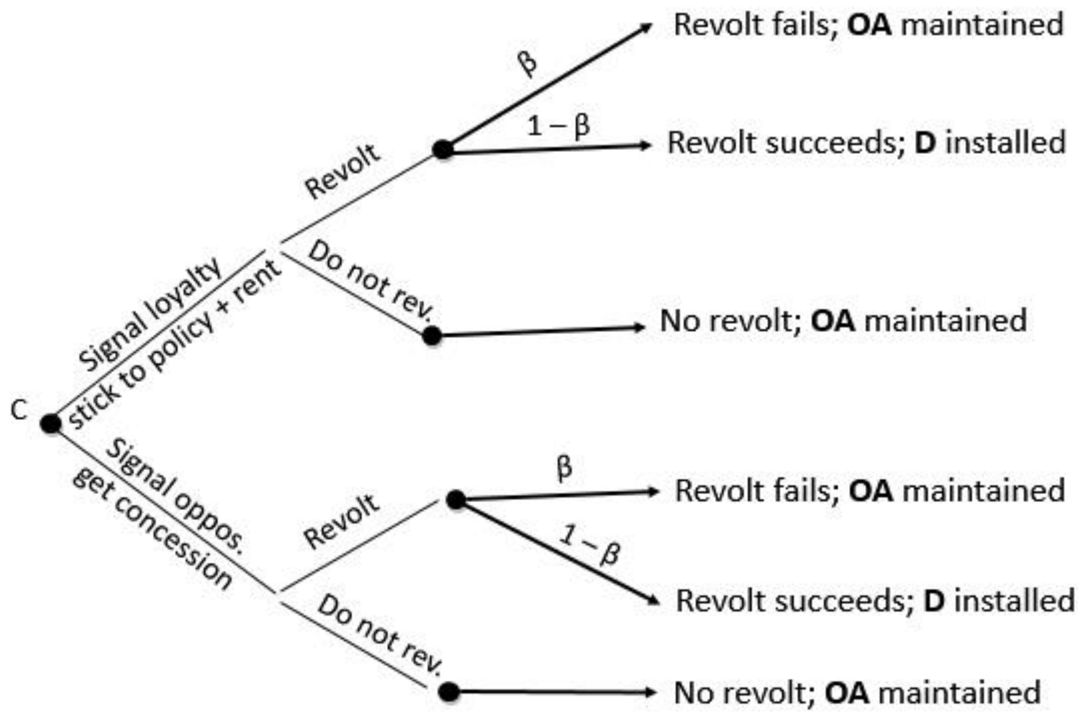
Irrespective of his previous signaling choice, the citizen can then choose whether to attempt to depose the autocrat or not, succeeding with probability β (see Figure 2.6).
Therefore, utility functions are the following. For the autocrat:

$$u_a^{OA}(x) =$$
$$= \begin{cases} R - \left(x_{opp} - a\right)^2, & if\ cit. opposes\ but\ no\ removal\ attempt \\ -k_a + \beta R - \left(x_{opp} - a\right)^2 + (1-\beta)(-j_a), & if\ cit. opposes\ and\ removal\ attempt \\ R - r - \left(x_{supp} - a\right)^2, & if\ cit. supports\ and\ no\ removal\ attempt \\ -k_a + \beta R - r - \left(x_{supp} - a\right)^2 + (1-\beta)(-j_a), & if\ cit. supports\ but\ removal\ attempt \end{cases}$$

For the citizen:

$$u_c^{OA}(x) =$$
$$= \begin{cases} -\left(c - x_{opp}\right)^2, & if\ cit. opposes\ but\ no\ removal\ attempt \\ -k_c + \beta[-j_c - \left(c - x_{opp}\right)^2] + (1-\beta)D_c, & if\ cit. opposes\ and\ removal\ attempt \\ r - \left(c - x_{supp}\right)^2, & if\ cit. supports\ and\ no\ removal\ attempt \\ -k_c + \beta\left[-j_c + r - \left(c - x_{supp}\right)^2\right] + (1-\beta)D_c, & if\ cit. supports\ but\ removal\ attempt \end{cases}$$

Figure 2.6 – Extended form representation of the open autocracy subgame



Note: the figure only represents outcomes. Payoffs are described in the main text.

<u>*Statement 3*</u>*. In the OA subgame there is a single, unique equilibrium.*

Let $n = \sqrt{\frac{k_c+\beta j_c-(1-\beta)D_c}{1-\beta}}$ *. Let* $\rho = k_a + (1-\beta)(R + j_a)$.

1. If the citizen chooses to support the regime, the autocrat will set $x = x_{supp}$ *and offer r, while if the citizen signals opposition the autocrat will set* $x = x_{opp}$ *somewhat nearer to the citizen. In order to set the amount of r, the autocrat will follow this formula:* $r = -a(x_{opp} - x_{supp})$*:*

- *If* $c_{max} \leq a + n$ *, the autocrat will set* $x_{opp} = x_{supp} = a$ *;*
- *If* $\frac{a}{2} + n < c_{max} < -\frac{\rho}{a}$ *, the autocrat will set* x^*_{opp} *and* x^*_{supp} *such that:*

$$x^*_{opp} = \min\left(c_{max} - n, \frac{a}{2} + 3n\right)$$

$$x^*_{supp} = \frac{a + x^*_{opp}}{3}$$

- *If $c_{max} > -\frac{\rho}{a}$, the autocrat will set x^*_{opp} and x^*_{supp} such that:*

$$x^*_{opp} = x^*_{supp} = \min\left(c_{max} - n, \qquad a + \frac{\rho}{2c_{max}}\right)$$

2. When $x_{opp} = x_{supp}$, the citizen will only choose to signal opposition if $r = 0$. When $x_{opp} > x_{supp}$, the citizen will only choose to signal opposition and ask for policy concessions if:

$$c > \frac{x_{opp} + x_{supp}}{2} + \frac{r}{2(x_{opp} - x_{supp})}$$

3. There is a removal attempt by the citizen if and only if:

$$|c - x| > n$$

Proof of Statement 3.

By backward induction, as in RA, the citizen will only choose to attempt to remove the autocrat when:

$$|c - x| > n$$

Clearly, here *x* depends on the choice of whether to signal opposition and get a policy concession, or to signal support and get no policy concession but a “reward”. How will the citizen choose? Trivially, whenever $x_{opp} = x_{supp}$, and $r > 0$, the citizen will always signal support and reap a better reward (overall). When this is not the case, the citizen will prefer to signal opposition when:

$$-\left(c - x_{opp}\right)^2 \geq r - (c - x_{supp})^2$$

Rearranging, the citizen will prefer to signal opposition when:

$$c > \frac{x_{opp} + x_{supp}}{2} + \frac{r}{2(x_{opp} - x_{supp})}$$

Call this c_1. For values $c < c_1$, the citizen will always choose to signal support to the autocrat and reap the reward r,while leaving the autocrat's policy offer unchanged. Also, by construction, if the citizen accepts the reward he will not attempt to remove the autocrat. For values of c above this threshold, the citizen will choose to signal opposition. Also, suppose there is a threshold for c below which the citizen will not attempt to remove the autocrat, while above it the citizen will always attempt to remove it. Call this second threshold c_2.
Therefore, three spaces open up: one in which the citizen will choose to support the regime and not revolt; another in which the citizen will signal opposition but regard the policy compromise as sufficient not to revolt; and a third one in which the citizen will signal opposition, look at the autocrat's offer and always judge it too small not to attempt to remove him.
The autocrat's decision function will thus depend on three free parameters (r, x_{opp} and x_{supp}) which he must set in order to maximize his own utility – that is:

$$u_a^{OA}(x) = P(c \leq c_1)\left\{R - r - \left(x_{supp} - a\right)^2\right\} + P(c_1 < c < c_2)\left\{R - \left(x_{opp} - a\right)^2\right\}$$
$$+ P(c > c_2)\left\{-k_a - \left(x_{opp} - a\right)^2 + \beta R + (1 - \beta)(-j_a)\right\}$$

Whenever the autocrat sets $x_{supp} = x_{opp}$ and $r = 0$, he is simply falling back to a repressive authoritarian setting in which he ignores potential signals. Again, a trivial solution in which the autocrat always sets the policy offer to his own ideal point ($x = a$) happens when $a \geq c_{max} - n$, namely when the citizens' benefit net of expected costs are always too small for him to prefer anything than an autocrat perfectly doing his own will. Thus I assume that a is strictly below that critical value. Assumption 2 also allows me to do away with the times in which the preferences of the citizen and the autocrat are too far apart to allow for any meaningful negotiation or strategic behavior, so that the autocrat would always set the policy to his own ideal point and face a removal attempt.

Having removed trivial cases, I am left to calculate first-order conditions as the autocrat attempts to maximize his utility. I start by maximizing utility as r changes, while holding x_{supp} and x_{opp} constant:

$$\frac{\partial u_a^{OA}}{\partial r} = 0 \qquad \rightarrow \qquad P(c \leq c_1)(-1) + \frac{R - r - (x_{supp} - a)^2}{2(x_{opp} - x_{supp})c_{max}} - \frac{R + (x_{opp} - a)^2}{2(x_{opp} - x_{supp})c_{max}}$$

$$\rightarrow P(c \leq c_1) = \frac{c_1}{c_{max}} = \frac{-r - \left(x_{supp} - a\right)^2 - (x_{opp} - a)^2}{2(x_{opp} - x_{supp})c_{max}}$$

$$\rightarrow \; c_1 = -a + \frac{x_{supp} + x_{opp}}{2} - \frac{r}{2(x_{supp} - x_{opp})}$$

Replacing $c_1$with this result in the equation on the second line, I get:

$$r = -a(x_{opp} - x_{supp})$$

And finally, substituting r in the third-line equation, I get:

$$c_1 = \frac{x_{supp} + x_{opp} - a}{2}$$

Now I need to maximize the autocrat's utility as x_{supp} changes, and knowing their determining equations can let r and c_1 vary with it:

$$\frac{\partial u_a^{OA}}{\partial x_{supp}} = 0$$

$$\rightarrow \quad P(c \leq c_1)\left[-a - 2\left(x_{opp} - a\right)\right] + \frac{R - r - (x_{supp} - a)^2}{2c_{max}} - \frac{R - (x_{opp} - a)^2}{2c_{max}}$$

$$= \frac{(x_{supp} + x_{opp} - a)(a - 2x_{opp})}{2c_{max}} + \frac{-\left(x_{supp} - a\right)^2 + \left(x_{opp} - a\right)^2 - a(x_{supp} - x_{opp})}{2c_{max}}$$

$$= \left(x_{supp} + x_{opp} - a\right)\left(\frac{a}{2} - 2x_{supp}\right) + \frac{x_{opp}^2 - x_{supp}^2 + a(x_{supp} - x_{opp})}{2}$$

$$= \left(x_{supp} + x_{opp} - a\right)\left[\frac{a}{2} - x_{supp} - \frac{x_{supp} - x_{opp}}{2}\right]$$

$$= \left(x_{supp} + x_{opp} - a\right)\frac{a - 3x_{supp} + x_{opp}}{2} = 0$$

In order to get a zero solution, then, either the first or the second part of the equation must go to zero. However, the first part would be equivalent to $c_1 = 0$ (see above for the c_1 equation). This is trivial, given that both policy offers and the reward would go to 0 as well, and the equilibrium would fall back to a particular corner solution of the RA subgame. So I assume that $c_1 > 0$, and therefore I will have to bring the second part of the equation to 0. The first-order condition for x_{supp}, then, brings me to a single non-trivial solution.

Also, after calculating how x_{opp} varies given the last equation, I can then substitute in the above equations for r and c_1 in order to straightforwardly determine how they vary with x_{opp}. I therefore get:

$$c_1 = \frac{2x_{opp} - a}{3}$$

$$r = -\frac{a(2x_{opp} - a)}{3}$$

Finally, I need to maximize the autocrat's utility function as x_{opp} changes. I can now allow c_1, r, and x_{supp} vary with x_{opp}.

$$\frac{\partial u_a^{OA}}{\partial x_{opp}} = 0 \quad \rightarrow \qquad P(c \le c_1)\left[\frac{2a}{3} + \frac{2}{3}\left(\frac{2a - x_{opp}}{3}\right)\right]$$
$$+ \left[R - \frac{-a\left(2x_{opp} - a\right)}{3} - \left(\frac{2a - x_{opp}}{3}\right)^2\right]\frac{2}{c_{max}} + P(c > c_1)2\left(a - x_{opp}\right)$$
$$+ \frac{2}{c_{max}}(x_{opp} - a)^2 + \frac{R}{c_{max}} - \frac{-k_a + \beta R - (1-\beta)j_a}{c_{max}}$$
$$= \frac{2x_{opp} - a}{27}\left(10a - 2x_{opp}\right) - \frac{2a}{9}\left(a - 2x_{opp}\right) - \frac{2}{27}\left(x_{opp} - 2a\right)^2$$
$$+ \frac{2}{3}\left(a - x_{opp}\right)\left(3c_{max} - 2x_{opp} + a\right) + \frac{2}{3}\left(x_{opp} - a\right)^2 + k_a + (1$$
$$- \beta)(R + j_a)$$
$$= \frac{16}{9}x_{opp}^2 + \left(-2c_{max} - \frac{16a}{9}\right)x_{opp} + \frac{4}{9}a^2 + 2ac_{max} + (1-\beta)(R + j_a) = 0$$

In order to solve this for x_{opp}, I need to apply the classic quadratic formula, getting the value(s) of x_{opp} for which the first-order condition is satisfied:

$$x_{opp} = \frac{9}{16}c_{max} + \frac{a}{2} \pm \frac{9}{16}\sqrt{c_{max}^2 - \frac{16}{9}\left[ac_{max} + k_a + (1-\beta)(R + j_a)\right]}$$

The second-order condition, needed to verify that this is a real maximum of the function, allows me to remove the higher solution, leaving me with just one possible solution. Now we have all the elements to see what the autocrat's choice would be – although some more effort is needed. When $c_{max} < a + n$, the quadratic has no real-valued solution, so that the autocrat will not be able to maximize his utility function unless it chooses $x_{opp} = x_{supp} = a$.

The functions requiring the choice between minima are reached by considering that if $x_{opp} \ge x_{supp} + 2n$, or $x_{supp} \ge n$, this constrains $x_{opp} \le \min(c_{max} - n, \frac{a}{2} + 3n)$. As c_{max} tends to $-\frac{\rho}{a}$, my solution for x_{opp} must dominate this condition (because in the interval between

x_{opp} as solved to maximize the utility and $c_{max} - n$, we have $\frac{\partial u_a^{OA}}{\partial x_{opp}} < 0$), the only solution is for the autocrat to set x_{opp} to the minimum of those two functions, and not to any value below that.

This equilibrium can only be valid until $c_{max} > \frac{a}{2} + n$, because I am requiring $c_1 = \frac{x_{opp}+x_{supp}}{2} > a$ (it is not possible to expect the autocrat to set a policy further from his ideal point but also from the citizens, as this would be logically inconsistent). This requirement also does not hold whenever $c_{max} \geq -\frac{\rho}{a}$.

Interpretation.

Substantively, this implies that larger policy concessions are awarded for "middle" values of c_{max} , i.e. of both the real divergence between the autocrat and the citizen, and the uncertainty around the real value of the citizen's ideal preference as held by the autocrat.

For very small and very large values of uncertainty, the autocrat will tend to ignore the signal: very small values push the autocrat to value the signal as useless; large values push the autocrat to incorporate a far larger potential tradeoff between policy compromise and its own ideal value, therefore convincing him to ignore the signal and risk being toppled. Therefore, in these cases the autocrat will behave as if one were observing a RA regime (and, as will be shown later, he will probably choose one given that the likelihood of being toppled in a RA as compared to an OA would be lower by construction).

At middle values of c_{max} the autocrat can be sufficiently sure that, by compromising by a fairly small amount in policies or by rewarding supporting citizens, he would be able to guarantee that no revolt occurs at a small cost. As stated above, the probability to guarantee no revolts starts to decline the more c_{max} increases.

The citizen, in turn, will choose to support the regime and not revolt for fairly small values of c_{max}. He will then move to signal opposition and gain a little superior policy concession for intermediate values, but without revolting, and he will finally signal opposition and try to topple the autocrat the further away he potentially is from the autocrat's policy preference.

2.4.4. Natural resources as a parameter

In my model, natural resources do not have a special place compared to other kinds of economic rents or incomes. They also lack an explicit place in the model's payoff parameters. However, this does not mean that natural resources – or any other rents for that matter – do not enter the game. Rents may be one of many intervening parameters affecting actors' utility functions (or, at least, that part of the utility functions that we are formalizing into our model). The lack of a special place for natural resources in the model is intentional: whether or not natural resources deserve a special place in comparative politics should not be derived directly from formal assumptions. Formal models should only serve as a guide as to the place where natural resources might come into play, while at the same time underlying assumptions should be made as explicit as possible in order to understand the complex interactions that may determine both benefits and costs of rentierism. That said, the argument begs the question: what could be the role of natural resources in my model of autocratic survival and regime choice? Following is a set of hypotheses that could not be explicitly modelled, but will serve as a guide when we will attempt at testing the influence that rentierism has on the probability that an autocratic regime lasts over time, *ceteris paribus*.

Firs, natural resources may directly affect the expected level of the benefit R the autocrat extracts from staying in power, and the benefit D_c that the citizens can expect if they succeed in deposing the autocrat. This is consistent with the "greed" hypothesis (Collier and Hoeffler 2002) as applied to political regimes instead of civil wars: given that some resources tend to be non-lootable, the parties in any conflict must aspire to take control of the legitimate government (and administrative and financial system) of a country before being able to take advantage of a significant portion of such natural resource rents. However, given that both the governing regime and power contenders may benefit from this situation, the utility functions of all actors may be augmented by a similar amount, and it could be difficult to disentangle differences in perceived utility from these natural resources. This is especially true when a governing regime draws a significant amount of public revenues from the administration or direct sale of natural resources, so that it would not be that different from a rebel group, or an opponent elite faction (be it civilian or military) aspiring to take control of the state institutions as well.

A second way in which natural resource rents enter my model is within the reward, *r*, that the autocrat dishes out to the citizens once they have signaled their support to the regime. The natural resource curse literature extensively elaborates on the hypothesis that autocratic rentier states may secure legitimacy (or at least passive benevolence) from their citizens by distributing the proceeds from the sale of natural resources. It seems natural that such proceeds are distributed only once the citizens have signaled their support to the regime – as it occurs in our model.

Although at the start of the discussion I set out that policy offers in my model include a broader set of socio-economic conditions rather than a mere economic policy, in this case a distributional conflict reemerges from the model, and is pretty much economic: the autocrat needs the citizens' support, and if the policy offer *x* is too low, it will anyway need to 'bribe' the median citizen sufficiently in order to lower the risk of facing violent opposition.

This can easily happen when a country relies primarily on natural resources, and most certainly so when they are highly-valued in the international markets, because the autocrat can intervene to increase his citizens' income, either through direct transfers or indirectly, through subsidies or investing in the provision of public goods. This behavior can for example be found in most hydrocarbon-dependent autocracies, which tend to intervene to increase their citizens' income, either through direct transfers or removing some or most of the taxes, or indirectly, for example through subsidies or by investing on the development of a national welfare system (see Vandewalle 1998 for the Libyan case). In fact, among the ten countries that had no income taxes in 2015, 70% (Saudi Arabia, Qatar, Kuwait, Bahrain, the United Arab Emirates, Oman, Brunei) were oil-dependent countries.

One important and complex interaction that could be accounted for, is that the reward distributed to the population should be directly related to the benefit *R* that the autocrat can extract by remaining in office. Indeed, we can expect that the more the expected *r* increases, the smaller *R* should become.

Finally, natural resources can also enter into the model through the cost parameter, because violence can temporarily stop their production/extraction or even stifle medium-term production prospects (together with other economic activities – Venezuela during the last decade being an especially relevant case in point; see International Crisis Group 2015). However, especially for resources that are on the ground and that need to be extracted, the ground acts as a natural safe, so that the total expected cost should be very low compared with the benefit of selling them the more reserves are thought to be in the ground and be economically recoverable.

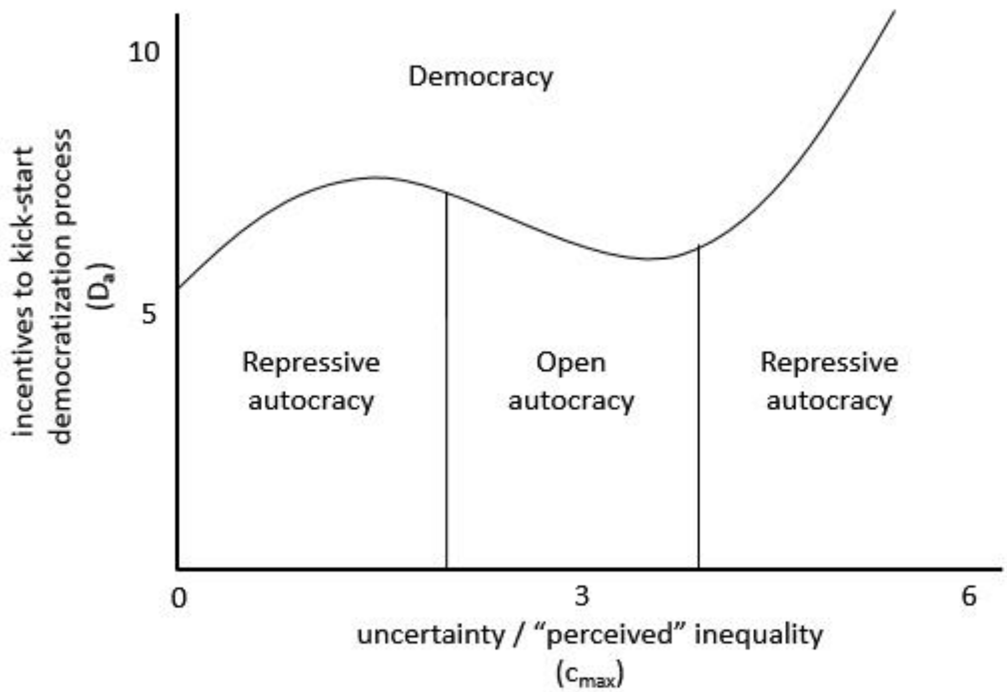
What the model cannot do is recover the dynamics of the changing value of these resources. In times of low natural resource prices, the rent that the autocrat may distribute to the population gets smaller, or it must be secured by renouncing some of the direct benefits from staying in power itself. At the same time, the expectations of the economic benefits from ousting the autocrat may get smaller for opposition groups as well – especially for elites that see the winning coalition in power struggling to keep up with austerity measures. When prices reverse, instead, and the commodity becomes highly valuable, both the distributable rents and the benefits of seizing power increase. The way in which these two main channels interact, and especially whether the autocrat's utility function increases faster or slower than the citizens', remains to be seen and is highly likely to be context-dependent.

2.5. Comparative statics: some representations of the general equilibrium

Formal models are imperfect representations of reality. Also because of this, I refrain from deriving precise closed-form conditions for general equilibria – a process that can be tedious and prone to errors –, instead choosing to rely upon simulations. Using R, I code the mathematic relationships between variables included in key utility equations, and then simulate thousands of game outcomes. In order to give a graphical representation of our model's outcomes, in the following I will fix most parameters but two, and record the outcome of the game over a bi-dimensional space.[1]

[1] In order to fix parameters, I choose the following values: $a = -2$; $k_c = k_a = 6$; $j_c = j_a = 8$; $D_c = 6$; $R = 2$; $\alpha = 0.92$; $\beta = 0.95$; $\gamma = 0.92$. Non-probabilistic parameters are set at arbitrary values, but the author has checked that results are consistent throughout a wide spectrum of possibilities. Probabilistic parameters, i.e. regime strength, are set at the historic average "stability" of closed autocracies, open autocracies and

Figure 2.7 – The autocrat's regime choice: autocratic uncertainty and incentives for democracy



In particular, in this section I focus on the level of uncertainty of the citizens' preferences (c_{max}), the strength of the different political regimes (α, β, and γ), and on how far the autocrat's ideal point is from the zero midpoint. In the lower half, Figure 2.7 shows that the game tends to end in repressive autocracy for very small and very high values of uncertainty. This is because for very small values of uncertainty, the autocrat can be confident that the citizen is near his own ideal point, and at the same time the signal that may be provided by the citizen in an open autocratic regime (whether to signal opposition or support to the regime) has a relatively low value.

As uncertainty increases but remains at relatively low levels, the autocrat only needs to make small concessions in order to increase greatly the chances to avoid revolt and the risk of being ousted. The signal from the citizens becomes so important to the autocrat that he accepts to

democracies, as coded by our variable *regime2* (see section 3.2.2), and recovering the yearly probability of regime change between 1946 and 2008.

see his chances of survival reduced (slightly) in case of revolt, in exchange for being able to discover the citizens' ideal policy preferences and set a more precise policy offer.

Finally, as uncertainty increases even more, the autocrat can expect that the citizens' ideal preference lies further from him and that therefore the median citizen will tend to revolt more, irrespective of the policy offer as it will be too far away from its ideal preference anyway. Therefore, the autocrat chooses to increase his chances of survival by going for repressive over open autocracy (remember that here the assumption is that $\alpha > \beta$)[2].

On the upper side of the figure, one can notice that democracy would be the final outcome only for sufficiently high benefits of a democratic transition driven by the autocrat. In fact, as the autocrat and the citizen grow further apart (as signaled by the increase in c_{max}), the autocrat will hardly ever choose to directly lead a transition towards democracy but always choose a repressive autocracy and maximize his chances of survival instead of giving up the implementation of his policy preference in favor of the citizens'.

This does not mean that the final outcome of the game cannot be democracy for very high values of "inequality" between the citizen and the autocratic elite: it simply means that the autocrat will prefer to risk being toppled rather than renounce its policy position.

Here, it is important to note that Figure 2.7 implies that for middle values of uncertainty, the only two outcomes are either open autocracy or democracy, depending on the level of incentives to democratize directly experienced by the autocrat. This is an important outcome that will serve to substantiate some of the hypotheses that I draw in section 2.6.

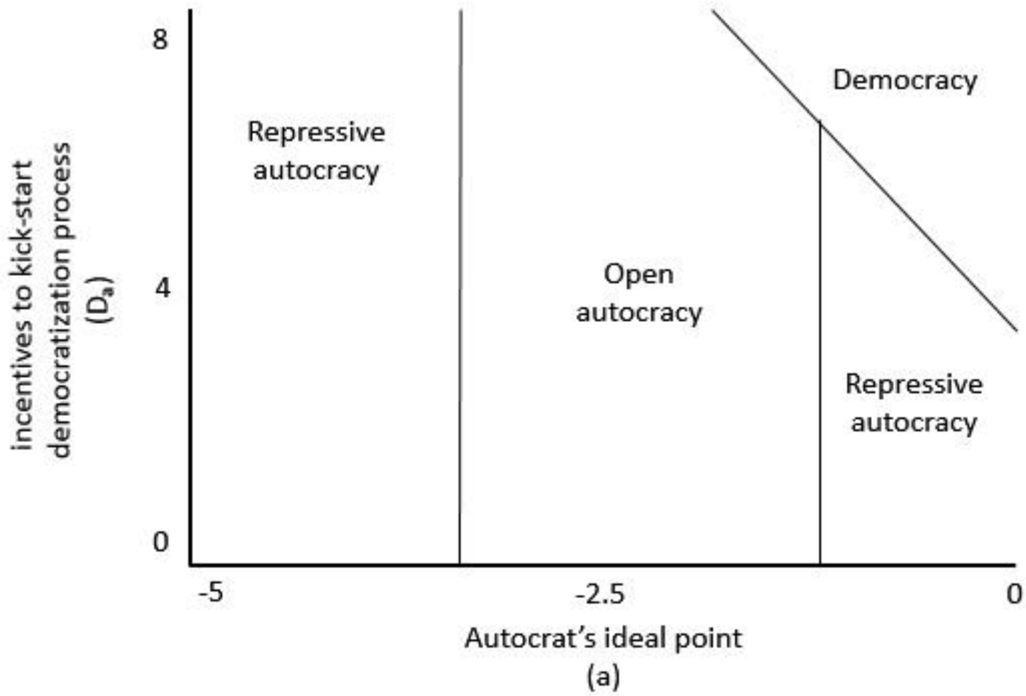
From Figure 2.8 we can also check that the autocrat's regime choice conditional on his own ideal point almost mirrors the effects of c_{max} above. The autocrat will choose OA only for intermediate values of his ideal point, but democracy here is even more difficult to achieve (although it becomes more and more likely as the autocrat's ideal point approaches the citizens', to the right).

The counterintuitive result here is that there is still a space on the lower right for repressive autocracy: this means that, even for lower values of latent conflict between the autocrat and

[2] This assumption is not liable to empirical testing because we only observe actual regime failures, and not the resilience of each regime to the occurrence and intensity of protests.

the citizen, the structure of incentives of the game is such that, at certain conditions, the autocrat might prefer a repressive autocracy to a democracy.

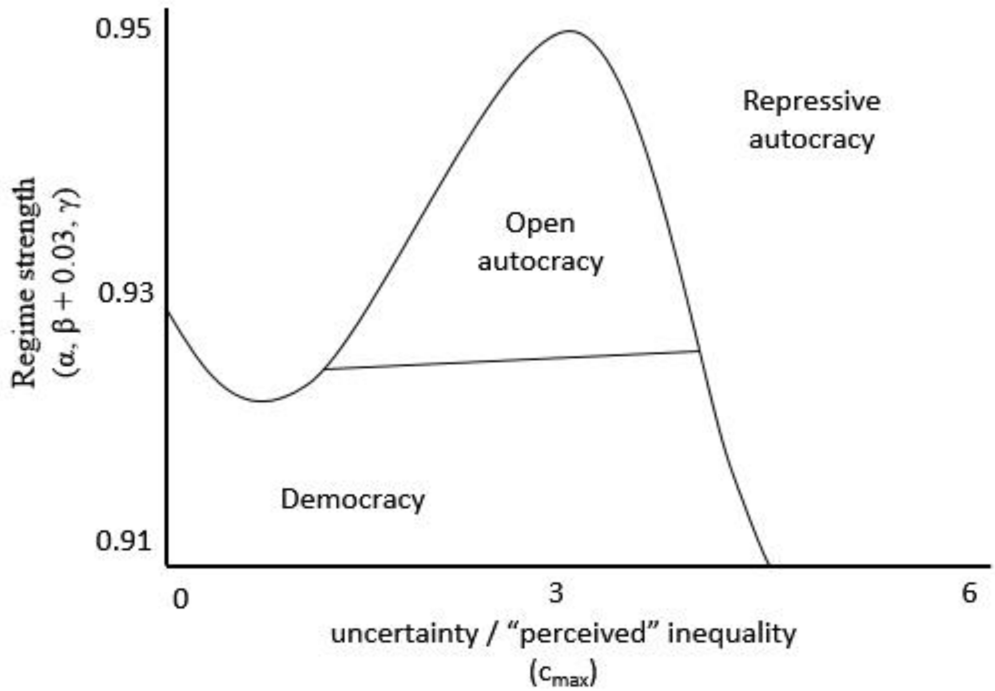
Figure 2.8 – The autocrat's regime choice: autocratic ideal point and incentives for democracy



This equilibrium however becomes less and less stable the more the benefits of a guided democratization of the country increase. The more we move to the right, the lower such benefits need to be for the repressive autocracy choice to become residual and disappear.

Finally, in Figure 2.9 we can see that the stronger the autocrat, the more it will prefer a repressive autocracy, because it can expect to survive revolt with more and more confidence. However, again for intermediate values of uncertainty and intermediate values of regime strength, OA becomes a viable solution and is the outcome of the game, as the autocrat will prefer to receive an informational signal that would allow him to set a more precise policy offer and lower, through this channel, his chances to be ousted, rather than do so via the regime type channel.

Figure 2.9 – The autocrat's regime choice: autocratic strength under different political regime conditions and uncertainty



Finally, the weaker the autocrat, the more he will prefer to directly lead a transition towards democracy. The outcome is skewed towards democracy here because I assume the probability of remaining in power to be common knowledge, so that the median citizen will be more and more tempted to try to oust the autocrat and set its own ideal policy preference, rather than letting the autocrat do so himself, as the known probability of ousting the autocrat increases.

Note that here the outcome may appear to be biased by the fact that we are letting γ (the strength of a democratic regime where the autocrat may still choose to try to stage a coup and regain power) vary with α and β. This way, it could be argued, the autocrat may choose to lead a democratic transition opportunistically, wait for the citizen to choose his own policy offer, and then stage a coup. However, keeping γ fixed does not significantly change the situation, simply reducing the space for democracy in favor of open autocracy and not much for a repressive autocracy.

Finally, again, at very high levels of c_{max} the autocrat will fall back to a repressive autocracy, set his own ideal point, and gamble for his own survival no matter how small its chances of survival are. This is because as uncertainty over the citizen's ideal preference position increases, it also becomes apparent to the autocrat that this position may be very far from his, and that he would never manage to assuage him with any policy offer.

2.6. Model implications and other hypotheses

Having described the autocrat's choice as conditional of a host of other factors, I ultimately try to draw some implications from my theoretical model, compare them with some extant literature on the theory of political regime choice and regime survival, and derive new hypotheses from this literature as well.

The first hypothesis that I draw is very general, but nonetheless interesting and liable to testing. As shown in the graphs above, in most cases political regime types tend to be the result of strong, structural and slow-moving forces. Such forces are a result of structural factors affecting a polity, and tend to vary very slowly with time. The distance between an autocrat and its citizens, for example, may remain pretty much a fixed feature of the regime throughout the whole life of an autocrat, and *ceteris paribus* will determine the elite-citizens interactions.

This is a general conclusion of politico-economic models: conditions are structural, tend to change slowly with time, and even so there is a strong inertia to them. Also, even when structural conditions are ripe for change, while political uprising or revolt might become almost certain, the probability that such protest movements succeed tends to be limited. This inertia is strongly reflected in the results of my model, and elicits the first hypothesis:

Hypothesis 1. *Because of a large inertia in political regime, it is highly likely that next year's political regime type will be the same as this year's political regime type.*

But there is also a set of very specific hypotheses that I can draw from my model. One possibility is that the distance between the citizens' and the autocrat's policy preferences, and the structural uncertainty of the autocrat towards the median citizen's ideal point, can be framed into reality by some socio-economic conditions. One straightforward consideration, already suggested in Figures 2.7 and 2.9, is that this distance may be apt to represent inequality in the overall population, i.e. how each percentile of national wealth is distributed to the population, ordered from the richest to the poorest member of the country. The more wealth is equally distributed among the nation's citizens, the more the citizens and the autocrat will be likely to be nearer in the policy space, while as inequalities get larger, the two will tend to get further apart. This suggests that "middle" values of a and c_{max} may be comparable to average values of inequality, and therefore that its implications in terms of regime type may be found even in reality.
Note that, given the constraint $c_{max} < c$, c_{max} is a function of c, so that the distance between the autocrat and the median citizen is implicitly embedded within c_{max} itself.

Hypothesis 2. *Liberalization movements (from repressive/closed autocracy to open autocracy, and from open autocracy to democracy) should be correlated with average levels of inequality, while liberalization should be discouraged by both low and high levels of inequality.*

This implication is controversial, but finds an important precedent in a by-now classic formal model of democratization. Indeed, Acemoglu and Robinson 2006 formulate a formal model of democratic transition, concluding that a choice in favor of a democratic polity should only be realized for average values of inequality. Low values of inequality would stifle democratization efforts by rendering potential social conflict a moot point, as the median citizen would have far less incentives to rebel to an autocrat setting a policy a little further from his ideal point. At the other end of the spectrum, an autocrat would never democratize and always face the chance of being ousted, because the cost of setting a policy offer that would prevent the median citizen from revolting would be too high.

A similar dynamic occurs in my model. However, a crucial difference is that I find that this conclusion is consistent for any “liberalization choice”: inequality should not just be related to the choice between dictatorship or democracy, but also determine the choice between repressive or open autocracy.

A second hypothesis can be drawn from the fact that, looking at D_a, one can see that the probability of an autocrat choosing to lead a democratization process increases as the benefits from an autocrat-led democratization process increase. Namely, these benefits may be derived from the expectations of the autocrat not to be ousted, or to even be the leader of the democratic transition and play a determinant role even in the post-transition period.

I therefore expect that, as international pressures in favor of democratization increase, both the domestic and the international environment become more conducive to enhancing the expectations that a “benevolent autocrat” will find his role preserved if he led a democratization effort, instead of being ousted by his citizens. The literature on democracy promotion bloomed during the Nineties, which were also a period of incredibly enhanced efforts in favor of democratic transitions, by national governments and NGOs alike (Chand 1997, Linz and Stepan 1996). The collapse of the Soviet Union brought about the emergence of the United States as the sole superpower: for a possibly brief interlude, this in turn elicited what some scholars have come to define as the “unipolar moment” (Krauthammer 1990). The end of the Cold war and the self-promotion of American power – also through the spread of the liberal values, which, despite many limitations especially with allied middle powers (such as Saudi Arabia), was increasingly seen as coinciding with fostering the US national interest – spurred an effort at democracy promotion which was incomparably superior to any other after the Second World War, not even to the focus on human rights by US President Jimmy Carter.

After about a decade, or even less than that according to others, the external support for democratization induced a backlash against it (Carothers 2006, Gravingholt et al. 2009, Burnell 2011, Wolff et al. 2014), both within some of the main “democracy promoters” such as the US, and in target countries. For one, regimes targeted with conditional aid tended to at best adopt cosmetic changes, while antiterrorism and regime change climbed at the top of the US foreign policy agenda after the 9/11/2001 attacks.

At the same time, the experience of failed democratic transitions, or the failure by autocratic leaders to properly lead the transition while preserving power during a period of turmoil, generated mistrust. Finally, the rise of new non-democratic powers as “prosperous, mighty, and assertive” actors served both as an example of illiberal economic development and called for an even more prudent approach by the US (Chen and Kinzellbach 2015). Finally, the growing narrative around “illiberal democracies” in Central and Eastern European countries appears to be just the last nail in the coffin of a democratization push that by the 2000s already appeared to have fizzled out, with less and less countries democratizing and even a reverse trend appearing (Freedom House 2014).

Despite all that, taking the whole post-Cold War period as a baseline against which to evaluate Cold War praxis in democratization trends and international support allows the test to be more stringent: it does not limit the possibility of international democratic support to the 1990s but extends it to the whole 18 years after the end of the Cold War in our sample (see Chapter 5 for more details).

Meanwhile, international reactions to the Arab Spring, coupled with recent efforts from the international community to push Myanmar towards a slow but steady democratization effort, might offer a counterargument that Western countries have not abandoned their attempts at supporting democratization processes whenever conditions seem ripe.

Hypothesis 3. *By increasing the incentives for autocratic leaders to lead political liberalization, the post-Cold War period should be correlated with a higher likelihood of liberalization.*

Then, as extensively discussed in section 2.4, the model envisages a complex role for the administration of private rewards or public punishments for expressions of loyalty and dissent by the median citizen in general, and for the management of natural resource rents in particular. While I may assume that an autocrat commanding control over a vast array of high-valued natural resources may be facilitated in staving off his rivals (by buying them off through cooptation, or by buying enough means to repress them), it is nonetheless apparent from my model that under particular conditions even potential rivals might be more inclined to ousting the autocrat and holding power directly themselves, than letting themselves be "bought off".

Hypothesis 4a*. The relationship between regime stability and natural resource rents, particularly hydrocarbon rents, is indeterminate.*
Hypothesis 4b*. The relationship between the choice of regime type and natural resource rents, particularly hydrocarbon rents, is indeterminate.*

Hypotheses 4a and 4b are somewhat non-hypothesis. They stand to signal that my model appears to be agnostic as to the political direction towards which natural resource rents may "tip" a polity, *ceteris paribus*, and on whether these natural resource rents may render a particular polity more or less prone to political instability.
The natural resource curse literature appears to have a well-consolidated tendency to regard rents as stabilizers for autocracies and, therefore, stifling democratization. At the same time, recent studies tend to disprove the "political resource curse" hypothesis (Haber and Menaldo 2011).
My model warns from oversimplifications: the relationship between regime type, regime stability, and natural resource rents is something that needs to be tested in practice, and the model cannot suggest the most likely political outcome as natural resource rents become increasingly available to either democratic or autocratic countries.
Moving away from my theoretical model, I also derive a set hypotheses from the general literature on comparative politics and regime stability, which are both liable to empirical testing and will serve to enrich our empirical multivariate models in Chapter 5. First of all, a classic strand of the literature finds consistent and considerable support for the modernization theory, which holds that economic development should foster social developments that are conducive to increasingly more liberal politics (Lipset 1959). Up to today, a large majority of studies has confirmed the correlation between economic development and the tendency of countries to democratize, even as "non-liberal" countries such as China, Singapore, or resource-rich Gulf Arab countries have growth considerably and remained largely autocratic in recent decades (Wucherpfennig and Deutsch 2009). Although there is considerable debate

about the mechanisms of this relationship, and the causes for this consistent finding are certainly not settled (see Przeworski et al. 2000), it is nonetheless evident that there is a broad consensus about the existence and robustness of this mutual relationship between economic development and increased likelihood of democratization.

At the same time, I want to test whether this relationship continues to hold true for liberalization within autocratic countries. Is modernization theory only pushing countries to be "more democratic" over time by transitioning from autocracy towards democracy, or are we missing a lot of variability and change (and, possibly, new mechanisms to be uncovered) by ignoring the difference between repressive and open autocracies? Therefore, I draw the following hypothesis:

Hypothesis 5. *Higher economic development should be correlated with higher levels of political liberalization.*

Another important factor that I want to include in my models is the tendency of political regime types to cluster in space. The regime diffusion (or "contagion") theory posits that nearby or similar countries tend to influence each other in terms of the choice of regime type. This means that we should expect that countries with similar regime types tend to cluster in space. This is indeed the case throughout our whole sample period, as evidenced by a cross-sectional analysis of spatial dependence that I undertook in the preliminary stages of my research (see the appendix to Chapter 6).

The issue with spatial models is that they tend to easily degenerate towards complexity and are difficult to manage in panel data settings. Therefore, the preliminary analysis stuck to a series of cross-sections over time, but could not model time dynamics or intra-country variability in any meaningful way. Also, theoretical mechanisms cannot do away with the classic Galton's problem, which can be reduced to an identification problem, whereby we can never be sure whether the spatial dependence that we find in empirical analysis is a result of a specific correlation between regime types, or is instead the result of spatial correlation in some observed or even omitted regressors (for example a result of the fact that levels of economic development tends to cluster in space as well). Despite these limitations, it is still crucial to include some measure of spatial dependence in the following multivariate models.

Hypothesis 6. *Regime types tend to cluster in space. Some regions will tend to be more or less liberal than the world baseline.*

Finally, another classic strand of the literature posits that democratization comes in "waves". Like a theory of contagion or domino effects, such theories argue that contagion effects do not just tend to cluster in space, but also happen simultaneously in time (Huntington 1991, Strand et al. 2011). Waves of democratization may be followed, or even occur together with, waves of autocratic retrenchment (McFaul 2002). Also, similar "waves" may be present in times of increased or decreased political stability overall, irrespective of the regime type outcome of such instability. Again, a peculiarity of our hypotheses is that we do not stick to democratization trends anymore, but inspect whether such "waves" occur also in tandem with liberalizing movements *within* autocracies.

Hypothesis 7a. *Some years or periods should be more or less conducive to political liberalization than others.*
Hypothesis 7b. *Some years or periods should be more conducive to regime stability than others.*

2.7. Conclusions and avenues for further research

In this section, I highlight some of the features that are missing from my theoretical model, and that may be avenues for further refinements and research.
First, by choosing to conceptualize policy offers as general political-economic conditions, I cannot study the impact over regime outcomes of the level of a country's gross domestic product or other economic determinants, such as taxes and transfers. This only allows me to

draw the modernization hypothesis from the extant literature, but not to logically tie it to my model.

In terms of levels, however, it is worth noting that even the most advanced political economy models normalize the size of an economy to 1, so that this is something that, to our knowledge, all formal models lack at the moment. As to my model, ì it would be tempting to assume that the size of the economy may enter the game through the rent parameter, R (i.e. the benefit that the autocrat receives from staying in power), and that transfers enter both in the policy offer x and the direct transfer, r. I refrain from doing so in order not to stretch too much the implications that can be drawn from my model, and postpone

Second, my model does not allow us to account for economic growth and its potential effects over political outcomes. This is again a shortcoming of any formal model in comparative politics, as they tend to treat the economy as perfectly static, for example considering the redistributive effects of taxes over the incomes of different actors but not their effects over economic growth. This is an obvious shortcoming in current comparative political models, especially considering that narrative accounts often offer mechanisms through which economic growth might affect actors' utility functions and strategic calculations, and that the occurrence of violence during political regime change might feed back into economic growth itself.

Finally, a potential deficiency of this model is the fact that it is missing recursivity. To completely model the behavior of rational actors, it can be argued that we would need an infinite-horizon game with time-discounting agents (Acemoglu et al. 2010). Comparative statics of a "static" model can only give us a general idea of how the "complete"/dynamic model would behave, and which equilibria it would converge to depending on parameter values: critics may argue that the complete long-run equilibrium still needs to be found.

This is a valid criticism, and it is one of the reasons why we should be careful not to infer too much from our model. At the same time, it may also be argued that infinite-horizon games place mathematical elegance before plausibility. Infinite-horizon games require the actors of the game to have a perfect, though fading, sense of future scenarios, and perfect forecasting skills. As behavioral economics proved to us time and again, this is already unlikely to be the case already for economic actors following sufficiently reliable economic cues depending on sufficiently strict economic laws (Samson 2014); it is even less likely in the case of actors moving in a socio-political context, in which innovations tend to be the norm.

Finally, the problem of bringing to solution a game with many parameters has been plaguing game-theoretic models for a long time, as it requires to add first- or second-order conditions that devoid those parameters of meaning by invoking them into the game but then keeping them fixed (Camerer 2003). Adding dynamic modeling to a game with uncertainty would have required a considerable leap into the unknown that we have decided to leave for another time.

Chapter 3. Measuring Democracy

3.1. Introduction

What is democracy, can we measure it, and if yes, how so? These questions have been around for decades – to some extent, even centuries (Rousseau 1762) – and have evolved with the very political systems and national cultures that have originated them. The problem of describing what is democracy was born the moment the word was invented, and is here to stay: democracy is what Gallie (1952) called a "contested concept".

The difficulty arises from the fact that "democracy" is an unobservable property of national political systems – a latent variable. We can observe, or at least hope to observe, tangible political institutions (such as legislatures and elections) and establish whether they are present or not in a political system at a given point in time, but democracy itself "is nowhere to be found" (Kaplan 1964). Moreover, even when we observe 'facts of the world', in order to assess whether a country is democratic and to what extent we can speak of democracy, it is highly likely that we will be forced to express a judgment over such facts (Wittgenstein 1921): for example, when we need to evaluate whether an election has been free and fair.

The fact that democracy is not directly observable leaves the concept in good company with many other fundamental concepts in the social sciences and, particularly, political science. For example, we cannot observe political power, both in the national and international arenas, and even consensus around the mere existence of a "power potential" outside the behavior of political actors and their strategic interactions seems to be feeble at best (Stoppino 2001). Still, political theory could not exist without relying on the concept of power.

During the last few decades, attempts at capturing the concept of democracy have proceeded in tandem with the progressive ‘quantification’ of political science in general and comparative politics in particular – i.e. progresses in the use of statistical methods and techniques in the methodology of social sciences. Gradually, political scientists have made good on their ambition to try to develop concepts that could be able to describe the political systems of all or most of the existing national polities in the world. The final aim was to make such concepts sufficiently general that they could be used to describe most national political systems, but at the same time not too vague that they become mere hollow shells of the original concept that they are trying to capture. This tension between universalization and the conservation of meaning is always apparent in contemporary social sciences.

Notwithstanding their limits (for example, substantive concepts of democracy today tend to fade out, while formal concepts step in and replace them), such efforts in categorization are of primary importance to my work. Indeed, in order to subject the formal theory I described in Chapter 2 to empirical testing, I will first need to choose the appropriate way to capture underlying political, economic, and social concepts (and forces).

This chapter will briefly summarize attempts at defining democracy, focusing on those that tried to apply the concept to the greatest number of national polities. I will focus on 7 measures of democracy and briefly describe how the authors chose to capture the concept. I do this in order to choose the best measures of democracy – those that I can regard as both valid (i.e., measuring “what it is supposed to measure”; Bollen 1989) and reliable (i.e., multiple measurements done by different persons at different times should produce similar results). Pragmatically, I will also aim for measures that are sufficiently exogenous to my covariates (i.e., do not rely, or rely only slightly, on potential independent variables for the operationalization of the concept of democracy).

In the next Chapter, I will show that even the best attempts at capturing the presence or absence of democracy, and of quantifying some “degrees” of it, fall short to a good operationalisation of political regime types – at least for my main research question. Nonetheless, they remain as important as ever to gauge the robustness of findings achieved via other methods, and deserve the specific attention I devote to them in the following pages.

3.2. Measures of democracy

Although measuring democracy should be an important aspect of comparative political studies in the first place, systematic cross-country assessments of democracy over time are not as many as one might think. In fact, three such measurements (Polity IV, Freedom House, or Przeworski-like dichotomous measures) overwhelmingly dominate the contemporary literature on democracy. This should not be regardeds as an indication of a lack of measurement attempts, nor as a gap that necessarily needs to be bridged. In fact, there are reasons why this might not be the case.

Firstly, the relative scarcity of measures of democracy could simply indicate that scholars regard existing measures as well-suited to capture the main dimensions of democracy, thus doing away with the need to come up with new operationalisations and embark in the recoding of democracy levels in every country in the world across time. After all, as Adcock and Collier claim, “in any field of enquiry, scholars commonly associate a matrix of potential meanings with the background concept[, and this] limits the range of plausible options” (Adcock and Collier 2001).

Secondly, as we will soon see, such measures seem to offer a plethora of possibilities and to cover a lot of the conceptual space, leaving little room to further innovation after the important systematic efforts accomplished in the Nineties and early 2000s: it might well be that we have entered a period of ‘consolidation’ after one of ‘innovation’. Indeed, reviewing measures of democracy more than a decade ago, Munck and Verkuilen (2002) already listed almost every large-N measurement scholars can choose from today.

However, there could also be reasons that might have left us with potential grey areas that still need to be filled. One such reason is that new codification projects tend to be big, requiring significant initial investment and, once the first huge efforts have come to an end, constant polishing and updating. Costs may sometimes be disincentive enough for scholars not to embark in such a project, even when they find current measures at fault.

Nevertheless, we can find comfort in the fact that, as soon as a ‘standard’ of democratic measures has emerged in the literature, such measures have undergone extensive criticism, and many of them have been refined over time by authors that have acknowledged fair and legitimate criticism.

What is significant about measures of democracy is that almost all of them rely on definitions of democracy that are similar to Schumpeter's procedural definition (Schumpeter 1947) and that stick to political/institutional characteristics rather than deviating towards more "substantive" (and all-encompassing) conceptualisations, which could also account for the socio-economic aspects of democratic political regimes. This is fortunate, because to be suitable for the use in empirical analysis it is of the utmost importance that the democracy measure one chooses does not include other variables, such as economic development or social pressures, which might as well be determinants of democracy rather than constitutive parts of the concept. In other words, I want my measures of democracy to be as exogenous as possible to potential correlates of democracy.

In Table 3.1, I show seven large-N measures of democracy, along with a synthesis of the concept's components declared to be important by each author in the definition and operationalization of the concept. Here, I proceed to briefly review the four most famous and authoritative measures, in order to adjudicate between them.

I start from Alvarez et al.'s (1996; also see Przeworski et al. 2000) dichotomous conceptualization of democracy, also in light of the fact that it appears to be the most conservative in categorizing countries as democracies. The measure has been updated and expanded by Cheibub et al. (2010; CGV henceforth).

Here, democracy is a political regime that satisfies two conditions:

1. the government is accountable to an elected institution, usually a legislature;
2. elections to the representative institution are *contested*, i.e. there exists an opposition which has an actual chance to be elected into office as a consequence of winning elections.

Source	*Measure*	*Components*
Freedom House 2013	Discr. / Cont.	Political rights, civil liberties
Hadenius 1992	Continuous	Elections, political freedoms
PACL (Przeworski et al. 2000; CGV 2010)	Dichotomous	Executive elections, legislative elections, party competition
Polity 4 (Marshall, Jaggers and Gurr 2006)	Continuous	Participation: competitive and regulated; Executive recruitment: competitive and open; Constraints on the executive
Polyarchy (Coppedge and Reinicke 1991)	Continuous	Free and fair elections, freedom of organization, freedom of expression, pluralism in the media
PRC (Gasiorowski 1996, Reich 2002)	Discrete	Competitiveness, inclusiveness, political liberties
Vanhanen (Vanhanen 1979, 1990, 2003)	Continuous	Competition, participation

Table 3.1. Large-N measures of democracy

For contestation to actually occur, three conditions must be met:

1. the outcome of the election must not be certain before it takes place (*ex ante* uncertainty);
2. whoever wins the election must actually take office (*ex post* irreversibility);
3. elections meeting criteria 1 and 2 should occur at regular and expected intervals (Przeworski 1991).

While this appears to be a great way to operationalise democracy, leaving out unnecessary socio-economic considerations, a problem arises when one needs a rule in order to operationalize the *contestation* criterion: how subjectively are we allowed to measure contestation, and how conservative do we want to be when we categorize a political regime as a democracy rather than as non-democratic?

Alvarez, Przeworski and all other scholars following in their lead choose a conservative rule: assuming that more than one party competes in the election, a regime can be considered democratic only if at least one alternation in power takes place under electoral rules identical (or very similar) to the ones that brought the incumbent to power.

An illustration of the "reticence" of this dichotomous measure to categorize regimes as democracies can be easily found by looking at Mexico. PACL and CGV categorize Mexico as a non-democracy until 2000, when the first alternation in power happened after more than half a century of Institutional Revolutionary Party (IRP) rule. In reality, the defeat of IRP came after more than two decades of progressive political liberalization. The Polity IV index, for example, codes the first (small) liberalization of the political space in 1977, and then two major liberalizations occurring in 1988 and 1994. PACL and CGV appear therefore to be not very sensitive to sensible regime changes before "full democracy" occurs.

Freedom House (FH) also carries out its own exercise for measuring how democratic political regimes are. The "Freedom in the World" report, released annually, has recently attracted huge media crowds, while researchers in comparative politics have been relying on the index for years.

FH index's appeal has appeared to wane in recent years, as scholars have come to consider it as less methodologically sound as it appeared to be in the second half of the Nineties and early 2000s (Brinks and Coppedge 2006). FH uses a 1-7 scoring system, where 7 means least democratic, combined with a "status" assigned to a country chosen among Free, Partly Free, and Not Free; the latter is not a different measure, but comes straight down form the overall score (a country is considered "free" if it scored between 1.0 and 2.5; "partly free" when it scores 3.0 to 5.0; and "not free" otherwise).

The overall FH score is actually the average between a "political rights" score and a "civil liberties" score, which also vary from 1 (most free) to 7 (least free). In practice, FH rates each country over 25 indicators, 10 of which are meant to describe the enjoyment of political rights (questions range from the electoral process to political pluralism and participation) and contribute towards that part of the index, while 15 are designed to capture the level protection of civil liberties (from freedom of expression and belief to associational and organizational rights).

Today, the comparative politics literature recognizes that the FH index has some drawbacks. First, it is not clear how or whether one should combine political rights and civil liberties in order to assess the level of democracy in any particular country. On the one hand, it is certainly true that the problem of aggregating many measurements into one single number is a charge that no index can be immune from, so this particular criticism is something that is not a unique liability (or, to remain neutral, a feature) of the FH index. On the other hand, aside from the aggregation method, in order to be useful to quantitative analysis an index should either stick to a formal/procedural definition of democracy, devoid of any other correlates, or clearly declare what enters the equation.

With the FH index, one can never be sure of whether it includes 'too much' information and is therefore endogenous to other social manifestations of political events (such as uprisings, riots, and the consequent use of repressive measures) which may be contingent upon the country-year instead of being a characteristic feature of the political system. This tension between procedural definitions of democracy, in which majority rule makes up the most part of the requisite for a country to be declared a democracy (together with some sort of open procedure to enforce the periodic "updating" of the country's majority through elections), and more substantive definitions that would favour the inclusion of some threshold for the level of protection of minority rights, resurfaces here in full force.

Moreover, the FH index also has some practical drawbacks. For starters, the FH undergoes several slight tweaks in its methodology every year, but the index is never revised backwards – and could not be without considerable time, effort and resources, given that it relies upon such a long expert questionnaire. It is therefore difficult to establish whether the index always captures the same underlying concept, year after year, or whether the definition itself has significantly changed, risking to bias statistical results.

At the same time, the index only ranks countries from 1972 onwards, even skipping one year (1981). Also, during the Eighties and early Nineties, it is not clear which year the report is referring to as it tends not to be synched with the calendar year, so that most of the times rankings do not refer to countries at 31 December of the previous year or 1 January for the current year. This way, consistency within the index and comparability with other indexes both risk to be undermined. Finally, FH raters unambiguously declare that the aim of the index is to measure political freedom and civil rights scores as enjoyed by individuals within a country's borders, irrespective of the fact that the curtailments on freedoms and rights come from state or non-state actors. This highly complicates the assessment of a country's level of democracy whenever there is violence within its borders, or in cases when the government is not able to assert its control over a significant portion of its territory.

Vanhanen (1979, 1990, 2003) approaches the problem of measuring a country's level of democracy from a different angle. His dataset, now maintained by the International Peace Research Institute in Oslo (PRIO), was called "Poliarchy" when it was first made public in electronic form because the index takes Dahl's (1971) famous conceptualization of democracy as its starting point. Vanhanen thus attempts to measure democracy by relying on two variables, competition and participation, spanning a period that goes from 1810 to 2000. Vanhanen's measure is interesting, and I believe that he is correct in trying to define democracy as a political regime possessing both of two dimensions: "participation", which captures inclusiveness (the right to participate in elections), and "competition", which refers to the level of political contestation (the fact that elections can be lost).

However, Poliarchy has at least one important liability. In order to assess the level of competition of a political system, one needs to evaluate in some way the level of contestation of elections. The latter, in turn, depends not only on the level of contestation allowed by the political regime itself, but also by the structure of the party system: majoritarian systems will be expected, by construction, to be less 'tolerant' of lesser parties. Therefore, when Vanhanen includes the percentage of votes won by parties other than the largest one in his measure of the level of contestation within a political system, one can see that he is conflating the political regime with the party system, and his operationalization blurs the line between the two realms of comparative politics (regime type, and the structure and characteristics of electoral systems), instead of producing a clear result.

I finally come to the Polity index approach (Marshall et al. 2006). This is by far the most widely used measure of democracy, and probably the most consistently updated (together with Freedom House's *Freedom in the World*) and revised. It is also one of the most extensive, currently covering the period 1800-2013 for all 'major independent states' (i.e., states with a total population of 500,000 or more in the most recent year – today it covers 167 countries).

The Polity index started out in a polity-case format (one observation for each political regime spell a country experienced; Gurr 1974). However, during the last two decades it has been modified to account for the growing scholarly use of time-series cross-section (TSCS) datasets, as computational power made it increasingly feasible to accommodate for the dynamic properties of TSCS data in statistical models – along with other desirable properties of TSCS data, such as the possibility to use lags and mitigate the problem of reverse causation.

The Polity index measures democracy by relying on a series of indicators (for which see the Polity codebook, Marshall et al. 2014) that are ultimately aggregated in a 0-10 measure of "autocracy" and a 0-10 measure of "democracy". By subtracting the autocracy score to the democracy score, one can therefore construct an index ranging from +10 to -10: that's exactly what the Polity project has done since 2002, incorporating a practice going on for some years before then, and producing a new variable, "polity2", that is amenable to use in TSCS analysis. However, it is important to recognize – as the Polity Codebook extensively does – that: "The simple combination of the original DEMOC and AUTOC index values in a unitary POLITY scale, in many ways, runs contrary to the original theory stated by Eckstein and Gurr in *Patterns of Authority* (1975) and, so, should be treated and interpreted with due caution. (…) *The original theory posits that autocratic and democratic authority are distinct patterns of authority* [emphasis added], elements of which may co-exist in any particular regime context" (Marshall et al. 2014).

I believe that the Polity index offers many advantages compared to its 'competitors'. First of all, it has been consistently scrutinized for errors by hundreds of scholars, and researchers maintaining the index have accepted some of these critiques by modifying the dataset (correcting errors, generating new variables, documenting discrepancies, etc.) throughout the whole period. It is also one of the most updated measures: together with the Freedom House index, every year the team maintaining the index produces new data for the full dataset of countries for the previous year. Evidence of such a continual updating effort can be seen today in the fact that Polity, which is currently at its fourth "incarnation" (Polity IV), is being reviewed in order to switch to a next generation / version (Polity 5). Development of version 5 of Polity kicked off in 2008 and, due to the tremendous amount of work that it entails (reviewing 167 countries' regime types over more than two centuries), as of early 2016 it was about two thirds to completion (Center for Systemic Peace 2016). Moreover, Polity IV relies on a political definition of democracy that is not endogenous to socio-economic variables, and also tends exogenous to many other political phenomena of interest (such as the occurrence of protests, violence, civil wars, etc.).

Therefore, for my empirical analysis, when I test for effects upon levels of democracy, I will mainly rely on the Polity IV index, and chiefly on the aggregated *polity2* variable: the unified +10 / -10 score (but "corrected": see next paragraph). However, given the tremendous amount of information that the availability of so many democracy indexes entails, it would be unwise to assume that I can select a single preferred index and do away with the full complexity of measuring a latent and multidimensional concept. This is why, together with my preferred measure, I will also rely upon the latent variable approach, for which see paragraph 3.3.

3.3. Correcting 'polity2'

Having selected the *polity2* variable from the Polity IV Index, I want to account for its limitations and, whenever possible, amend the index in such a way as to remove some possible sources of bias. As stated in the previous paragraph, many such limitations have been dealt with over the years since the launch of the current version (2002), by current and previous teams maintaining the dataset. However, to my knowledge at least one serious issue still plagues the *polity2* variable as can be found in the Polity IV dataset, and this still awaits to be tackled by the developing team. Given that this is the case, I decide to directly amend the dataset before proceeding, in order to fix the data to the best of my means.

As explained in the previous section, the *polity2* variable has been extensively used in panel (TSCS) data studies, and indeed it has been devised exactly for such a purpose. It captures the latent concept of democracy ranging from +10 (full democracy) to -10 (full autocracy) through 21 discrete steps. What is more, compared to the original *polity* variable, which also ranged from +10 to -10, the *polity2* variable provides a democracy score for periods of so-called "interruption", "interregnum" and "transition", while *polity* coded them with special numbers as -66, -77, and -88 respectively (and, therefore, as ultimately missing). This artificial substitution for a proper democracy score in periods where even the very concept of "polity" of a country is doubtfully applicable served the purpose to not lose too many observations, especially in cases of listwise deletion whenever some country-years were missing, and the more so when lags of the democracy index were thought to play a part in the true/best model (as this would at least double the missingness level).

According to the Polity IV User Manual (Marshall et al. 2014), "interruption periods" occur "if a country is occupied by foreign powers during war, terminating the old polity, then re-establishes a polity after foreign occupation ends", Polity codes the intervening years as an interruption until an independent polity is re-established. Periods of interruption "are also coded for participants involved in short-lived attempts at the creation of ethnic, religious, or regional federations": in the 1945-2007 period this applies to Singapore in 1963 and 1964, when following a heated referendum in 1962 with no option of independence it entered a short-lived union with Malaysia, before finally gaining independence in 1965. It also applies to Syria for the period 1958-1960, when the country entered into a political union with Egypt (joining the so-called United Arab Republic), until conflicts between Syria's Ba'ath party and Nasser's Egypt led to a military coup that restored Syria's independence.

Country-years are coded as an "interregnum period" (-77) when "there is a complete collapse of central political authority. This most likely occurs during periods of internal war" (Marshall et al. 2014). Notable cases of interregnum periods in the dataset include Somalia 1991-2010 (encompassing the Somali civil war and concurrent state collapse), Lebanon 1975-1989 (spanning the Lebanese civil war), Democratic Republic of the Congo 1992-2002 (the two Congolese civil wars and, before 1996, a situation of quasi-state collapse with Mobutu and Mulumba creating two parallel state authorities within DRC), and Laos 1961-1972 (the full-scale outbreak of the Laotian civil war, which had been ongoing since 1953).

Finally, a period is coded as a "transition" when a transition between two polities exceeds a one-year length, for example when the building of the new polity takes time as in Angola between 1993 and 1996, Burundi 2001-2004, Cambodia 1988-92, El Salvador 1979-83, or Iran 1979-81. Most transition periods however last just a single year.

Out of the 9,201 country-years between 1945 and 2013, such periods represent a meagre minority of the grand total: 108 cases (1.2%) are coded as interruptions, 127 (1.4%) as interregnums, and 173 (1.9%) as transitions. Altogether, however, they make up 4.4% of the dataset, meaning that out of the average 55 years that a country is present (in my post-World War II initial dataset) special cases appear on average 2.42 times per country. Obviously, special cases are also skewed to occur more frequently in unstable countries experiencing a higher level of regime transitions (rare events themselves) and political instability. Such countries can expect special cases to happen almost double the time they take place in "advanced" countries and regions of the world, while at the same time these countries' "life span" since they first gained independence can be considerably shorter.

The occurrence of such special cases raises a twofold problem for TSCS studies:

1. it causes a break in the series, so that the dynamic components of TSCS models lose out in estimation efficiency, and the more so the lengthier time lags are included in the models;
2. it precludes the study of the causes of such missing data cases: listwise deletion forbids the researcher to explore the root causes of missingness. When such occurrences are non-random events, the ensuing regression analyses are not just inefficient but outright biased.

The solution offered by Polity IV authors was to produce a new variable, called Revised Combined Polity Score, or simply *polity2*, which converts some of these special cases into conventional polity scores (between -10 and +10). In order to produce *polity2*, the values were converted according to the following rule set:

- Cases of foreign interruption (-66) were treated as system missing. Therefore, no substantial change occurred, given that a special code would anyway be treated as a missing value by any scholar employing TSCS model;

- Cases of interregnum (-77) were converted to “neutral” Polity scores of 0;
- Cases of transition (-88) were prorated across the span of the transition. So, authors employed a simple interpolation between the score for the year before the transition occurred and the score for the year in which the transition ended (Marshall et al. 2014).

Such treatment of special cases, although certainly useful to fill out most of the missingness, appears however to produce numbers that “lack face validity” (Plumper and Neumayer 2010). Although it is apparent that Polity authors made a positive attempt to take a neutral stance towards coding without inserting their own biases, such a neutral stance in the evaluation of political regime scores ends up complicating things instead of offering feasible solutions.

For example, assigning a score of 0 to interregnum periods can be conceived as “neutral” only insofar as it puts countries exactly at the middle of the spectrum between -10 and +10 scores. Given that “interregnum” codes are reserved for countries experiencing enough political turmoil that central authority has all but collapsed, the assignment of a 0 score clusters all “failed state” experiences at the middle of the spectrum, making state capacity endogenous to Polity score and inducing the “humped shaped” relationship between regime type and state capacity that many authors have observed since the end of the Nineties and early-2000s (not noticing it was oftentimes an artefact of their own making).

Moreover, assigning 0 scores is hardly “neutral”, because countries will most often have a democracy score that is non-zero (either positive or negative) before collapsing. Therefore, with a change from the non-zero score to zero it appears as if the regime type had abruptly changed, especially for countries with scores at the far ends of the spectrum, when something entirely different has actually happened (i.e., the state collapsed). For example, Afghanistan between 1992 and 1995 is considered a collapsed state, and the same holds for Cyprus between 1963 and 1967. However, given that Afghanistan’s democracy score is -8 before the interregnum period, it appears as if an important democratic transition happened in 1992, followed again by an autocratic transition between 1995 and 1996, when the country’s score collapsed again from 0 to -7. For Cyprus, at the same time, a score of 8 in 1962 becomes a 0 score in 1963, and reverts back to 7 in 1967.

The problem is further amplified when the interregnum coding rule is applied before the transition coding rule, which implies that transition years can be substituted by interpolating the score from the year before the transition with the score when the transition has ended. This adds “fake” transitions towards or away from democracy. Consider for example Chad between 1990 and 1997: it is coded as an interregnum in 1992, while 1991 and the 1993-96 period are coded as transition. The result is that, as 1992 is coded as 0, this generates a rapid transition towards democracy lasting two years (from -7 to 0), and then a slow transition towards autocracy lasting 5 years (from 0 in 1992 to -3 in 1997).

This shows that a rule-based approach grounded in theory should be preferred to an approach that declares itself neutral but ends up conflating state capacity and regime type. The approach is based on the recognition that “some a priori knowledge, or at least some logic, always exists to make selection better than an a-theoretical computer algorithm” (King 1986).

Finally, the problem gets even worse if one is interested in studying within-country variability, employing a fixed effects model that removes between-country variability, or models that try to separate within- and between-country variation.

Though a unique, feasible solution is impossible to find, I prefer to operate a correction to the *polity2* variable. My choice falls with what Plumper and Neumayer (2010) call the “minimum level” rule. I therefore set the interregnum years to the lower of the two polity scores bordering the interregnum period, then use linear interpolation to add the affected transition years. This choice has many advantages:

1. Contrary to the “classic” *polity2* coding, it makes use of country-specific information preceding or following the interregnum period. It is therefore country-specific;
2. Therefore, the rule does not treat all countries as equal by assigning a 0 score: it will assign scores depending on the country’s historical regime trajectories, which we can expect to be much more consistent with each polity’s most probable democracy score than a one-size-fits-all rule;

3. The rule constrains the democracy score to values within those before and after the interregnum. Again, this seems much more plausible than coding abrupt changes towards or away from democracy by assigning a 0 score to such cases.

Once I have corrected the *polity2* variable, I am left with a democracy index that appears much more reliable even in “fringe”, controversial cases, so that at least *a priori* my trust in regression analysis employing this measure can increase much more. The new variable correlates 99.2% with the previous *polity2* coding. Despite such a high correlation, the correction is especially important in cases of fixed-effects models, where the between-country variability is ignored and within-country movements become the only source of variation in the sample. This tends to compound the effects of measurement errors, especially since transitions are very rare compared to the persistence of political regimes (see Chapter 6), and “fake” transitions can therefore bias estimates in unforeseeable ways.

It is also important to underline that while a small number of comparative politics studies now corrects for this coding error, no analysis in the resource curse literature has ever employed such a correction. Therefore, previous analyses can be misleading, especially those studying regime transitions. The outcomes of such analyses should be considered imperfect until a replication with the improved measure of democracy confirms their findings.

3.4. Unified Democracy Scores: the latent variable approach

As anticipated in section 3.1, it seems wasteful to simply choose one among many measures of democracy. Although I came to the conclusion that the Polity index, and specifically an improved version of its *polity2* variable, is potentially the best measure out there, it is however possible that other measures are valid as well – at least partially so.

Moreover, a less stubborn, more flexible approach may account for the fact that, in comparative politics, there exist rival strategies for evaluating and validating measures of democracy (Seawright and Collier 2014). These certainly comprise levels-of-measurement approaches such as those described in section 3.1, but also include structural-equation modelling with latent variables such as those expounded in this section.

In fact, insofar as one assumes that every democracy index is an attempt to capture a unique, but complex and latent, underlying concept, one may want to assess the possibility of constructing a democracy index that accounts for this possibility.

Pemstein et al. (2010) attempt to do just that. They collect 10 different measures of democracy, including the seven measures that I reviewed in section 3.1 and three democracy indexes that maintain a regional focus. Evidence for the fact that the 10 measures are trying to capture the same underlying concept is the fact that they correlate highly with each other. This, however, does not imply that they are all equally valid (Adcock and Collier 2001).

Moreover, scholars that choose only one democracy score among many, as valid as they consider it to be, will make all the 'mistakes' that their measure of choice makes. It could be useful, then, to try to gauge just how much my index of choice is idiosyncratic compared with all other 9 measures, at least as a robustness check. For example, Polity considers Vladimir Putin's ascent to power in 1999 to increase the country's democracy from 3 to 6, and leaves it at that value until 2007, when it lowers it to 4. On the opposite, Freedom House considers democracy to be decreasing since 1998, and constantly from there on, until Putin's Russia takes on a starkly authoritarian face.

Who is right and who is wrong? Under Pemstein et al.'s (2010) methodology, I profess myself agnostic to the question and try to capture how reliable each coder tends to be compared to others, and how reliable the specific measure of democracy for that specific country could be whenever one observes discrepancies between different indexes.

Therefore, Pemstein et al. (2010) construct a Unified Democracy Score. The authors model each indicator as an approximation to a latent, continuous unidimensional variable. Specifically, they assume that each index is produced by an observer (a judge) that, in trying to observe and measure the latent variable in each country-year, makes judgmental mistakes. Another assumption is that all the attempts are equally capable, *ex ante*, to observe the same concept were it not for mistakes. Given the true level of democracy z_i, in country-year i, the rater j generates a perception t_{ij} of democracy in that country-year, such that:

$$t_{ij} = z_i + e_{ij} \sim N(0, \sigma_j^2) \tag{1}$$

Note that this model assumes that the rater makes only stochastic mistakes: systematic mistakes are forbidden. This could be problematic, and even the high correlation among measures can mask systematic errors. However, one can safely assume that differences in conceptualization, measurement and simple coder mistakes take the lion's share of the error as raters construct indexes.

We can summarize Pemstein et al. (2010)'s reasoning as follows. Suppose we can directly observe the raters' perceptions, t_{ij}, and their error variances, σ_j^2. In reality we observe raters' coded democracy scores, which, depending on the scale (be it continuous, ordinal, or bivariate), will be chosen as the closest to their perceived level of democracy. Moreover, suppose we have no a priori information about the true level of democracy, z_i. In such a case, we only assume:

$$z_i \sim N(z_0, \sigma_0^2) \tag{2}$$

If we take together (1) and (2), we can calculate the posterior distribution of z_i, i.e. its distribution conditional on t_{ij} and σ_j^2, which is:

$$z_i \sim N\left(\frac{\sum_{j=1}^{m} \frac{t_{ij}}{\sigma_j^2}}{\frac{1}{\sigma_0^2} + \sum_{j=1}^{m} \frac{1}{\sigma_j^2}}, \frac{1}{\frac{1}{\sigma_0^2} + \sum_{j=1}^{m} \frac{1}{\sigma_j^2}}\right) \tag{3}$$

So the posterior mean is simply a weighted average of the individual raters' perceptions, with weights that are proportional to each rater's precision. Moreover, by looking at the variance, we can see that uncertainty is decreasing in the number of raters, m.

While it is true that we do not observe raters' perceptions but their actual democracy scores, Pemstein et al. (2010) find a solution to this problem by employing a technique called multirater ordinal probit (Johnson and Albert 1999). To summarize, the technique allows for potential variation due to the fact that: (1) rankings in democracy scores are based on some unknown function of each rater's underlying perceptions; (2) the potential observable space is not continuous (no mind could categorize things into an infinitely continuous space), so that it can be subdivided into a number of ordinal categories (and not interval-level ones); (3) the rater can make mistakes, so that raters' assignment of each country-year to a particular value of democracy depends on an underlying probability distribution.

Estimating the model with a Markov Chain Monte Carlo (MCMC) algorithm allows the authors to recover an estimate for the latent continuous level of democracy for each country-year, as well as quantify the error committed by relying on the point estimate (its standard deviation).

For the analysis in Chapter 6, I will only preserve the point estimates recovered from the procedure that produced the Unified Democracy Scores (UDS). I do this because I believe that the value of the UDS lies more in its role as a robustness check whenever most coders find themselves at odds with my preferred democracy score (the improved version of *polity2*). While uncertainty around each point estimate could certainly be useful, a huge problem in using it is that is soaks up almost all variability in within-country democracy scores, rendering any meaningful analysis particularly problematic.

Doing so, I must be particularly aware of errors of commission: especially in fixed-effects regression analysis, the UDS point estimates might capture within-country democracy movements that are not actually happening but originate from the error around the point estimate. However, we should be confident that small-enough movements will not be significantly different from zero-movements to bias our analysis.

Ultimately, I am left with an original measure of democracy that I prefer to the others – the improved *polity2* score – and a weighted combination of 10 measures of democracy as an important variable that I can use to test the robustness of my findings.

3.5. Comparing UDS and polity2 democracy scores

Now that I have selected two measures of democracy, I can compare them and see how they tap into the same underlying concept.

First of all, the two measures are highly correlated in the sample I will use for later analysis (ρ=.923 for 5,282 country-years between 1970 and 2007). Clearly, a high correlation between the UDS and the *polity2* score is to be expected because the UDS is actually constructed using 10 measures of democracy among which *polity2* figures prominently. However, such a high correlation also proves that *polity2* is a good benchmark, meaning that either the algorithms used to calculate the UDS – including Bayesian calibration – tend to give it enough weight when other measures disagree with it, or that the various measures of democracy seldom disagree between each other.

Nevertheless, the more one scratches below the surface, the more some differences between the two indexes start to appear. In order to compare the extent to which the two scores agree or disagree, I must transform one scale in a way that it can be compared with the other. I therefore standardize *polity2* to the yearly limits (maxima and minima) of the Unified Democracy Scores. I can now plot the two, but I can also do more than that: I can take advantage of the fact that UDS scores come not only with point estimates, but also with their own standard deviations. I can therefore add to the graphs the 95% confidence intervals for the UDS, and check whether *polity2* and the UDS are significantly different from one another, or whether *polity2* falls within the UDS's C.I.

I do this for 1970 and 2007, the first and last year in my sample, for all countries. Figures 3.1 and 3.2 show, interestingly, that both measures tend to have similar shapes. In 1970, both UDS and *polity2* appear to describe a complete logistic curve, although *polity2*'s seems to be much sharper and UDS's is smoother and gentler. In 2007, instead, the curve seems to transform from a logistic seems into a logarithmic curve, driven by a large number of more democratic countries that form an upper plateau, while declining faster at the autocratic end of the spectrum.

Though their 'functional form' across countries for a given point in time might appear to be similar, if we compare the two measures with each other, we can see that they tend to agree more at the far ends of the spectrum, for full authoritarian or full democratic countries, and much less for middle values. This happens even though we corrected the *polity2* measure by modifying the values that, beforehand, tended to cluster at 0 because of interregnum periods. Moreover, compared to UDS scores, the *polity2* measure shows a remarkable "democratic bias" in 2007. This brings us back to a handful of recognized and distinctive features of Polity IV scores:

a. they tend to categorize regime types at the ends of the 21-point scale much more often than at the middle. While this in turn might be simply the way things are with regime types, it in turns begs the question of whether we should prefer a dichotomous, or ordinal, scale to an interval-level variable such as *polity2*;
b. especially for the period after the Cold War, they tend to categorize most democratic regimes as "fully democratic", while other democratic indexes, and single-country experts, appear to be more conservative (Iskahan and Slaughter 2014; Marshall et al. 2014).

Figure 3.1 – Comparing polity and UDS with 95% C.I. (country scores for 1970)

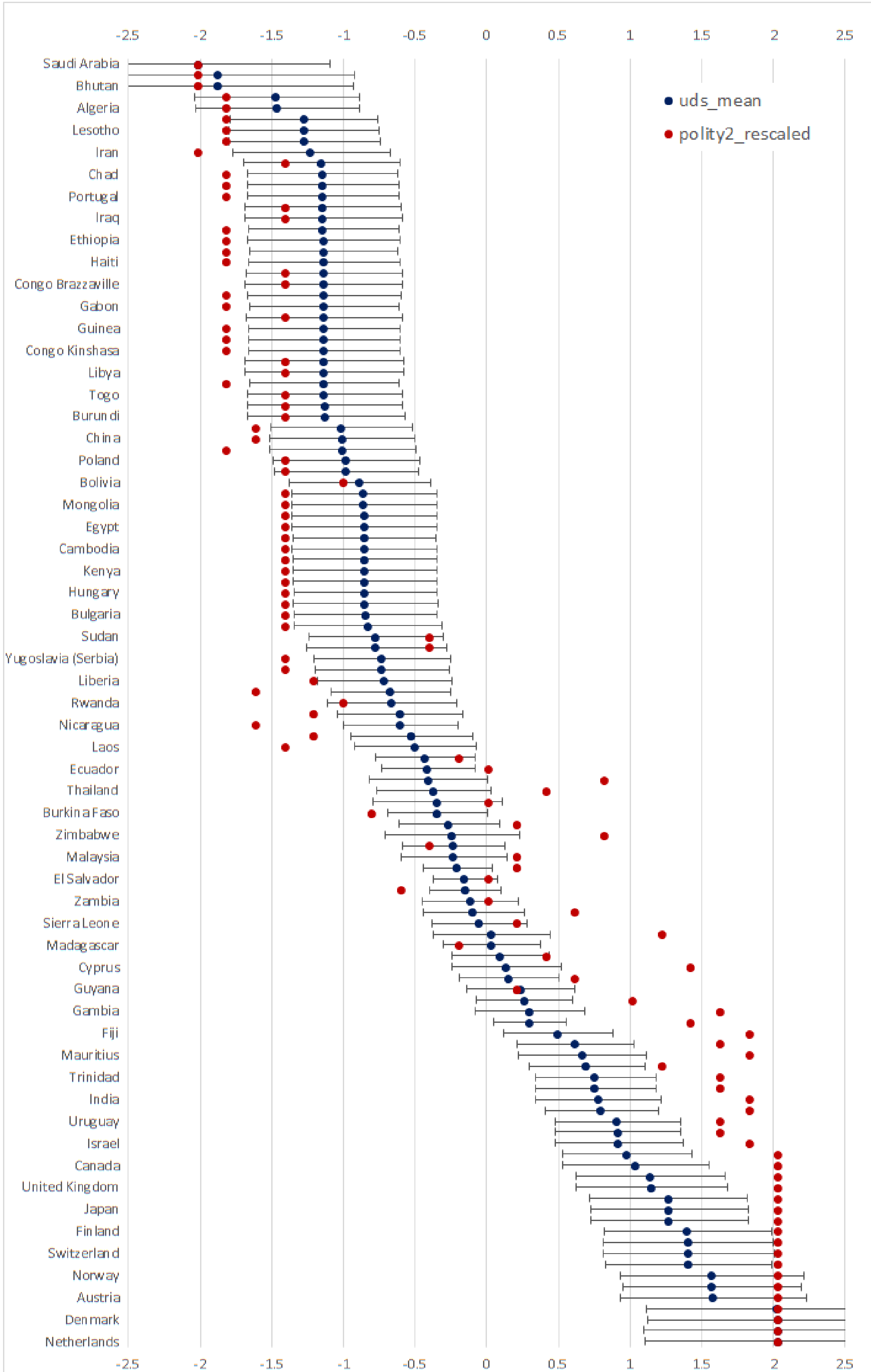
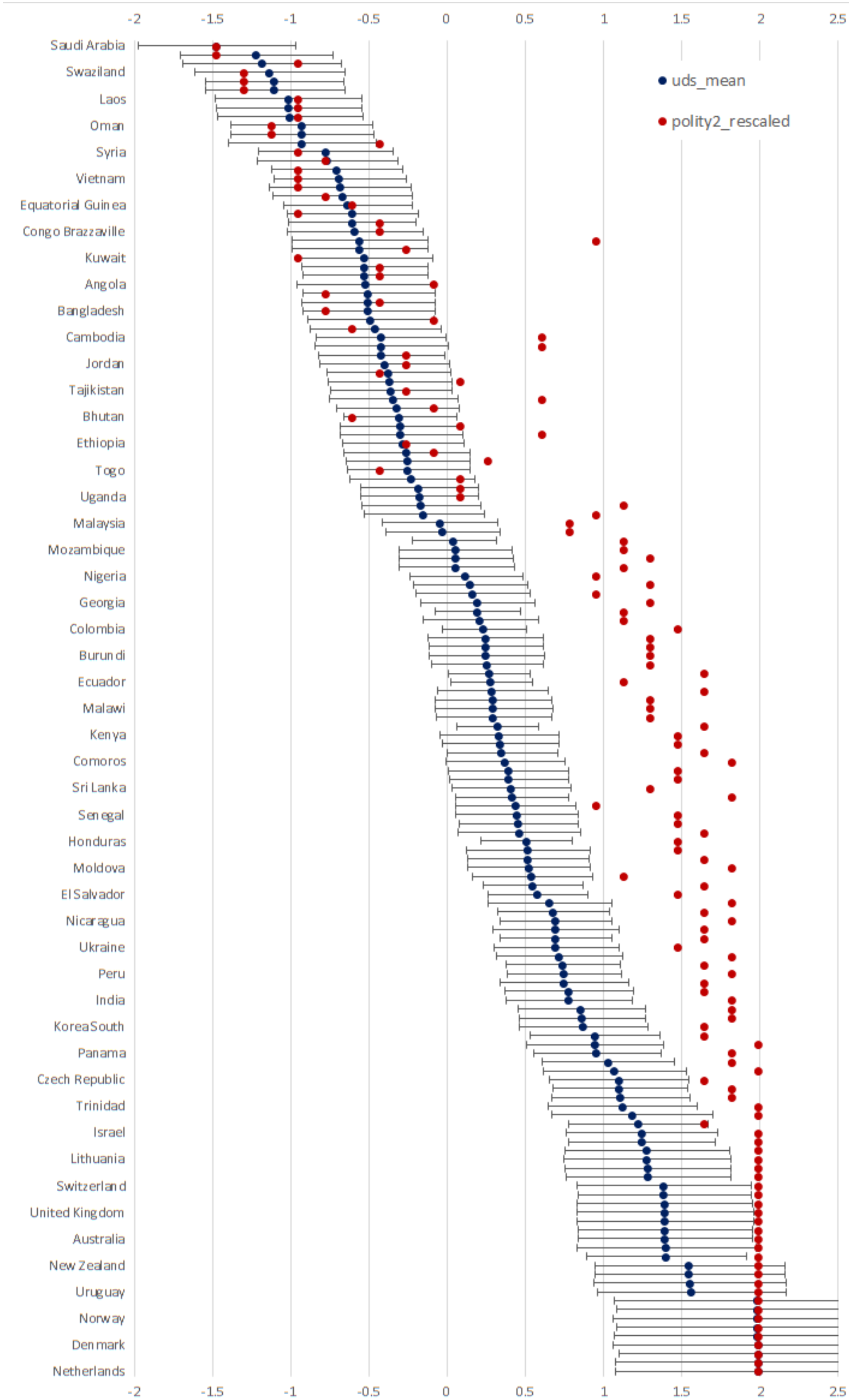


Figure 3.2 – Comparing polity and UDS with 95% C.I. (country scores for 2007)



In order to compare the two measures, while still sticking to 1970 and 2007, one can also subdivide the two continuous scores by assigning each score to one of four categories, then map the world and have a look at the differences. The tricky part consists in finding substantive, meaningful ways to assign each score to one particular category.

As regards *polity2*, I rely upon a partitioning that has been proposed directly by the authors, by creating 4 categories called democracy (*polity2* scores between 6 and 10, included), open anocracy (1 to 5), closed anocracy (0 to -5), and autocracy (-6 to -10). This partition has been used in several studies (Benson and Kugler 1998, Fearon and Laitin 2003, Davenport and Armstrong 2004, Regan and Bell 2010), although it has also been subject to important – and founded – criticism (Vreeland 2008).

As for the UDS, instead, the very fact that the score aggregates so many measures of democracy, "auto-anchoring" itself, wipes away any immediate possibility of substantive interpretation. To overcome this problem, one must be quite careful. Assuming that the UDS is tapping into the same latent concept of democracy as *polity2* and all other democracy scores do, the safest way to split the continuum of UDS scores into four categories is by first relying upon some algorithmic optimization method, and then check for the factual validity of its outcome. In my case, after having tried various optimization methods, I chose the Jenks' natural breaks classification method (Jenks 1967).

Jenks' natural breaks method determines the best arrangement of values into different classes using the following optimization rules:

a. minimize variance within each class (i.e., minimizing average deviation of each value within one class from the class average);
b. maximize variance between each class (i.e., maximize average deviation of each class from every other group mean).

Given that I aim at creating four categories, I create three natural breaks to divide each category from the next one (the fourth one will be left to the right of the last break).

Figure 3.3 – Democracy in the world in 1970, UDS

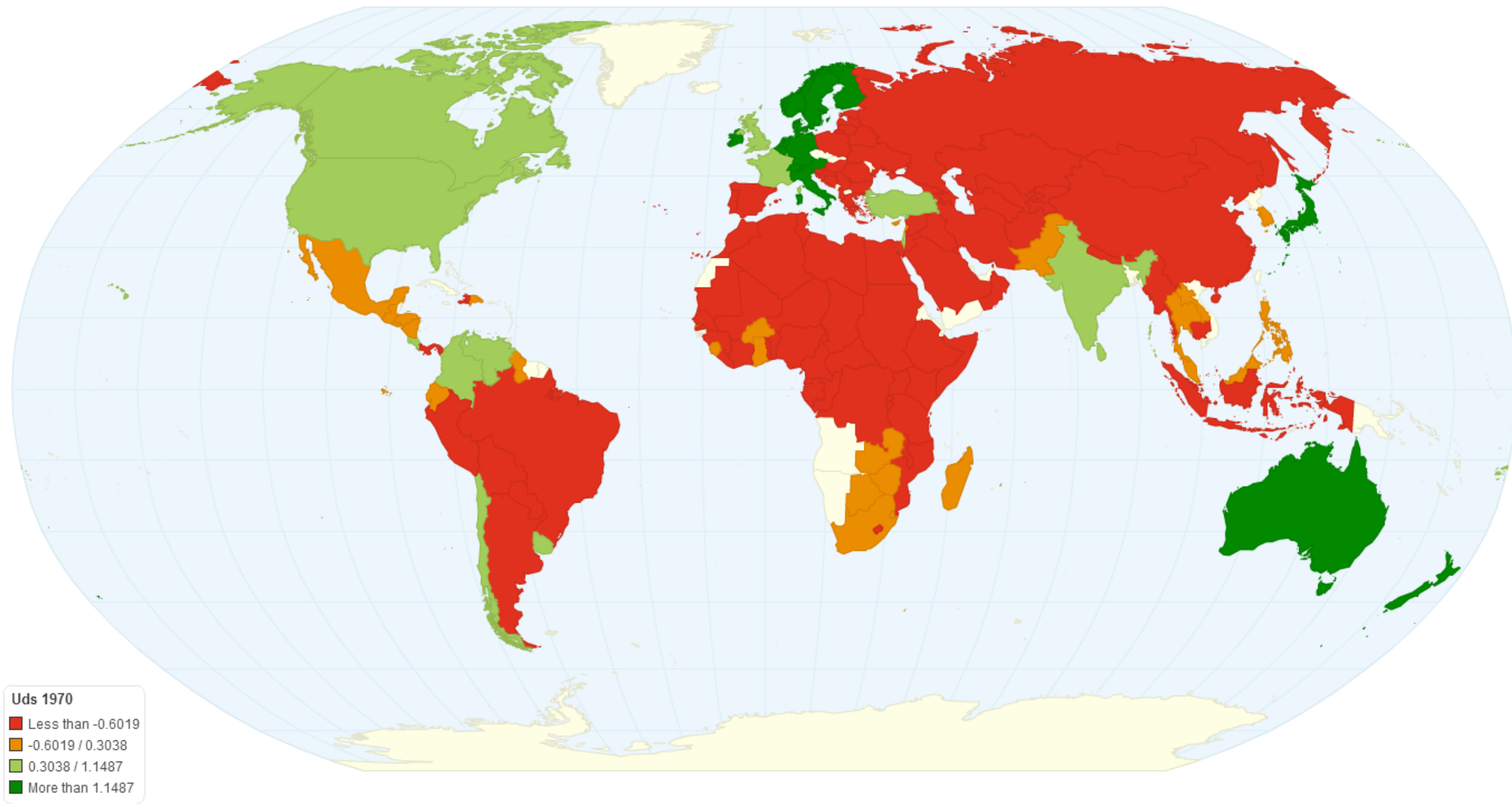
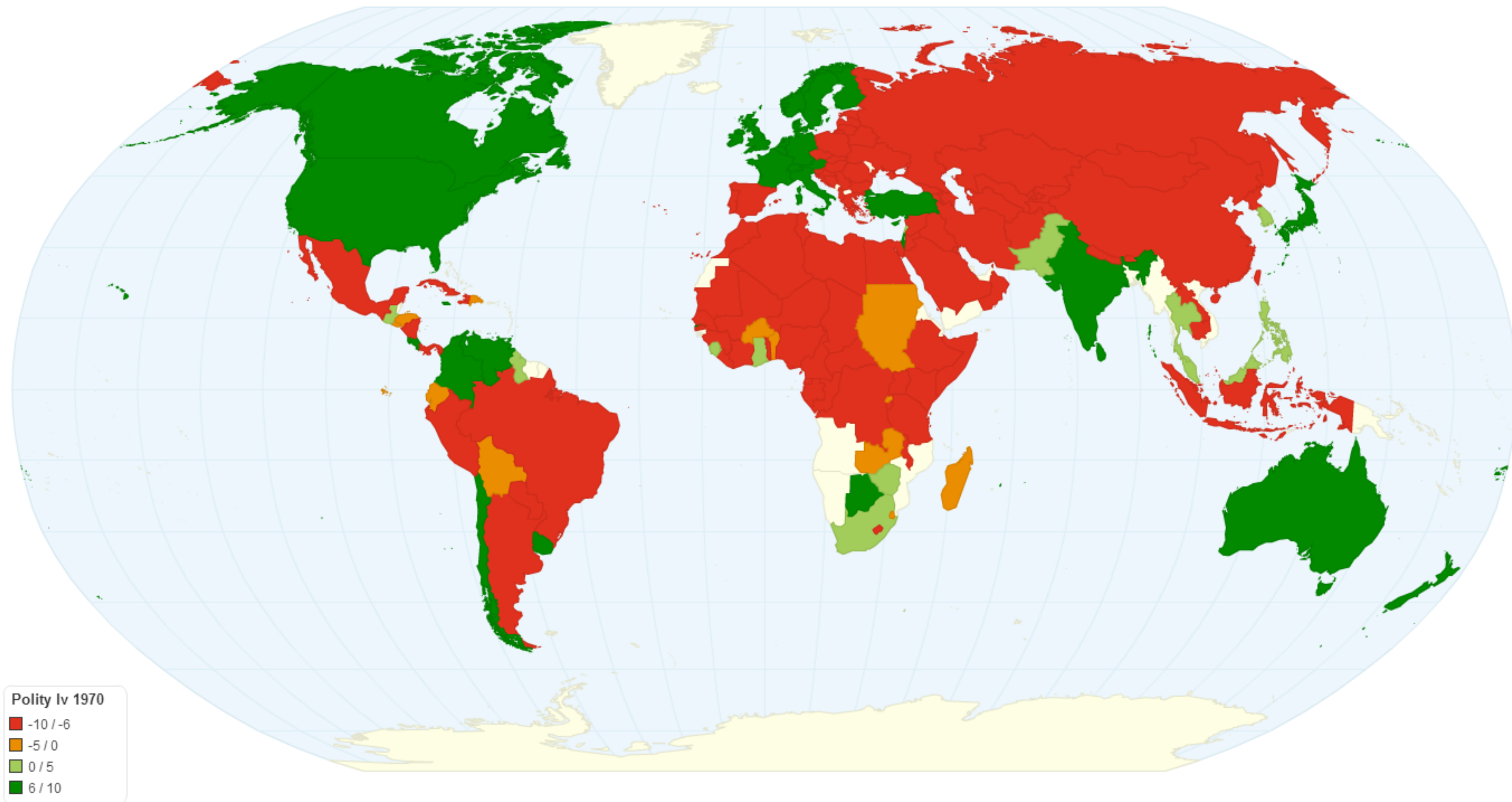


Figure 3.4 – Democracy in the world in 1970, Polity IV



At the end of the optimization, I am left with four classes that can be roughly compared with the substantive four categories in which *polity2* was subdivided. I map results over a world map for UDS and *polity2* scores, both for 1970 and 2007. The "democracy bias" of *polity2* is already evident in 1970, as the prevalence of light and (especially) dark green on the map shows. Differences are most evident for North America, Southern Africa, the Indian subcontinent, and Southeast Asia. At the same time, *polity2* considers some countries to be more autocratic than UDS does: among them one can find Mexico, Nicaragua, and Laos.

Leaping about 35 years forward, the situation is similar.

Again, *polity2* has a democracy bias with respect to UDS. However, the regions of the world in which this bias is most evident have shifted to Southern America, Western Africa, Eastern Europe, while all previous regions remain persistently more democratic than the UDS reports. At the same time, exceptions are also evident in Central Asia, Iran, Belarus and Bangladesh, where *polity2* scores are 'harsher' than UDS and end up placing these regions and countries in more autocratic categories than UDS does.

Figure 3.5 – Democracy in the world in 2007, UDS

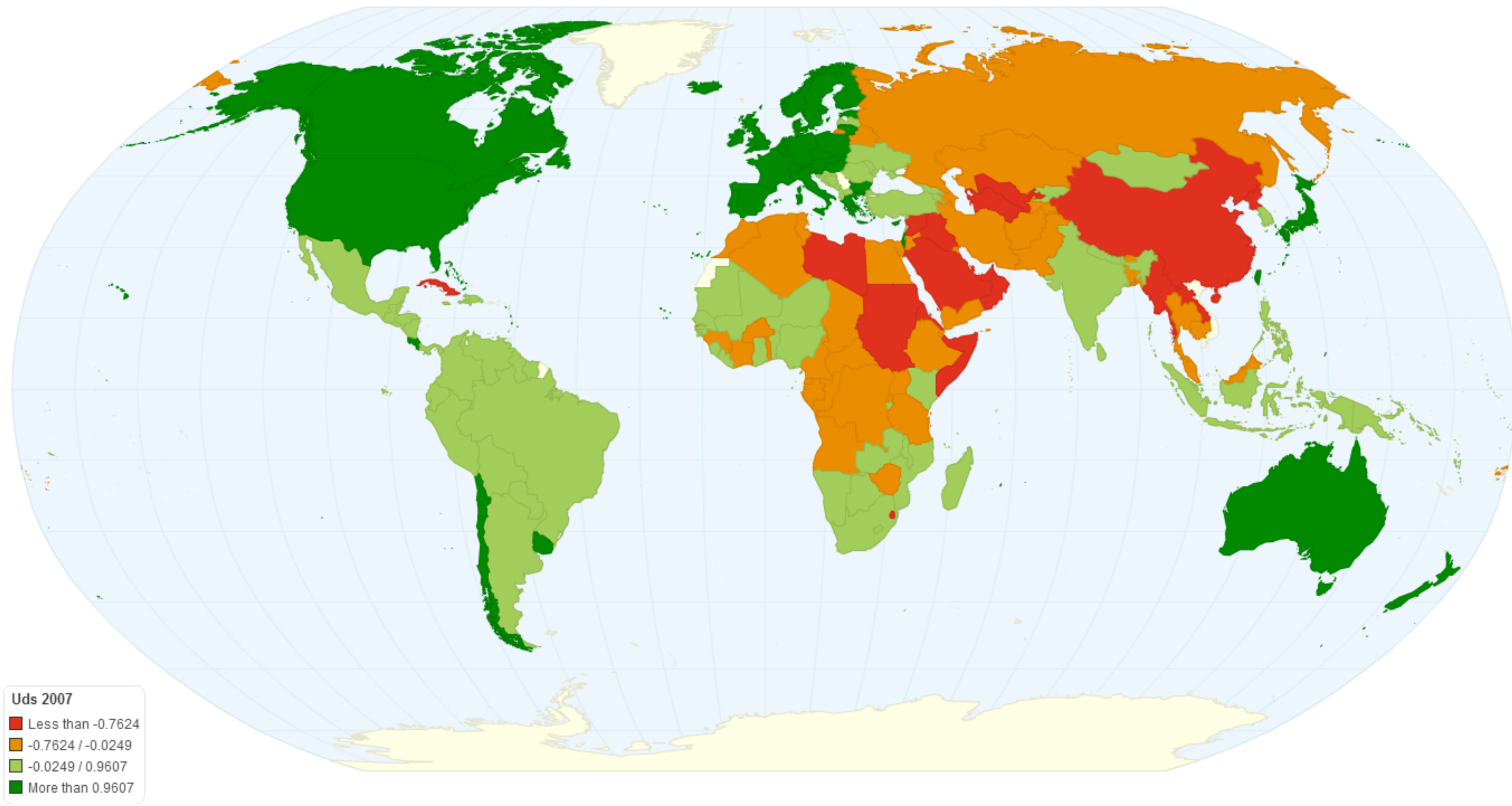
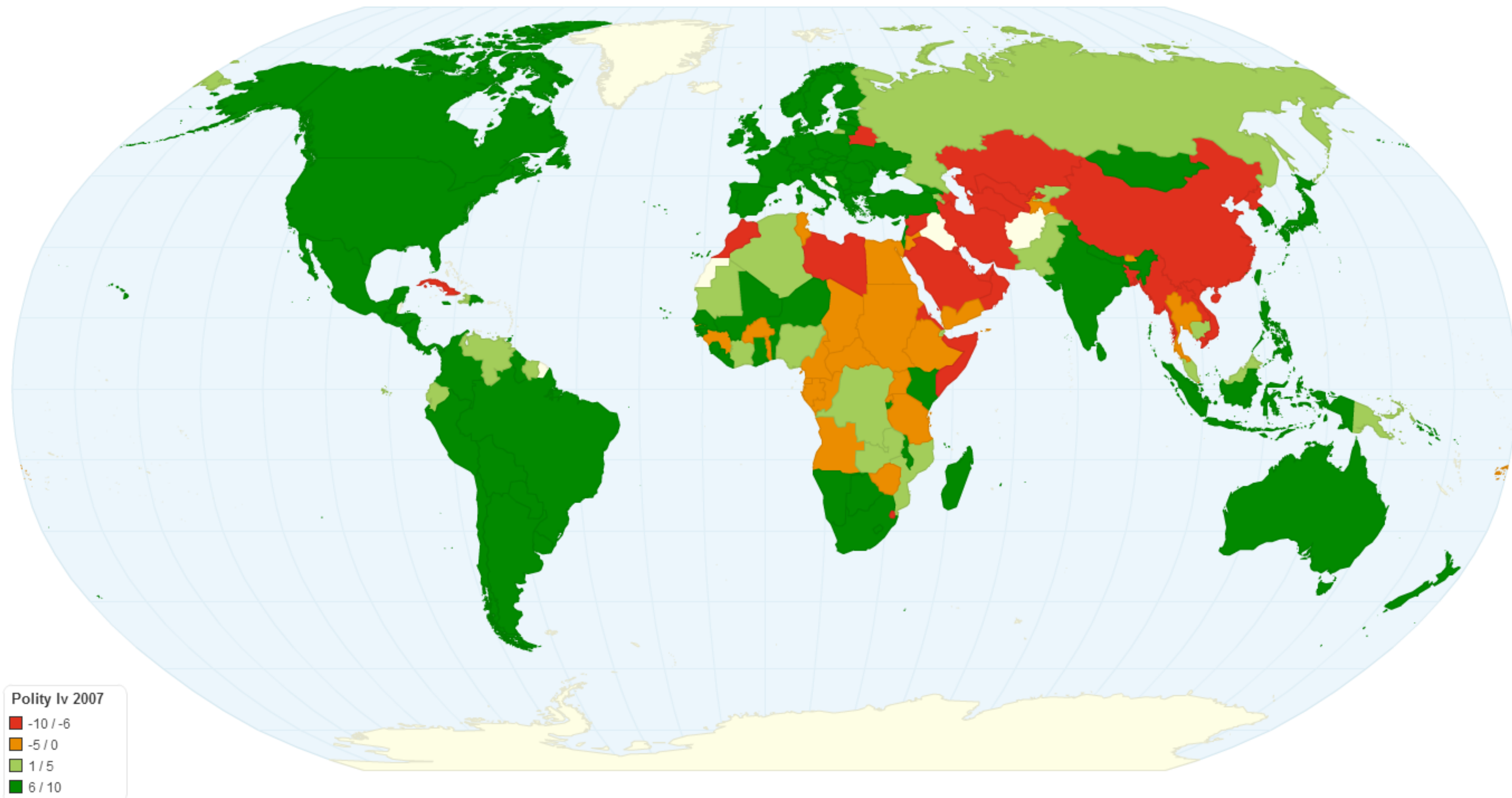


Figure 3.6 – Democracy in the world in 2007, Polity IV



3.6. Conclusion

In order to measure levels of democracy or autocracy, I choose to rely on two indexes: the *polity2* variable from the Polity IV dataset, but corrected as indicated in section 3.2, and the Unified Democracy Scores.

The two indexes agree most of the time, and almost all of the time with regards to the “region” where any country appears to reside with regards its levels of democracy or autocracy. However, when the two indexes tend to disagree, they do so more for countries at the middle and middle-to-higher end of the political regime spectrum, which is also a place where many open autocracies would appear to reside (see Chapter 4).

Employing the two indexes in some of my empirical models in Chapter 6 will therefore be an important test for robustness, allowing me to see whether results change in any meaningful way when I employ alternatively one index or the other.

Chapter 4. Measuring Autocracy and Regime Openness

4.1. Introduction

In this Chapter, I shift my focus from levels of democracy from levels of liberalization in general, and then try to tease out the differences between "autocratic regime types". I make a relevant contribution to the study of autocracies by devising a novel way to conceptualise differences in "liberalisation levels" (what I term "regime openness") that go beyond the presence or absence of particular institutional features (such as legislatures, elections, etc.) in autocratic regimes.

This new operationalisation will also be central in Chapter 6, which will focus on empirically testing the hypothesis I derived from the formal model and the relevant literature on democratisation at the end of Chapter 2. While my model clearly posits the existence of three distinct "political regime types" (closed/repressive autocracy, open autocracy and democracy), many of the hypotheses present in the democratisation literature could be naturally extended and generalised to levels of regime openness, and it would be interesting to see whether longstanding theories withstand a test set in this new scene.

My attempt to arrive at an operationalisation of different autocratic regime types fits neatly within a growing body of literature that traces its origins back to the dissatisfaction in current measures of democracy, and tries to "unpack" autocratic regimes. I believe that this new way of differentiating between more and less liberal regime types, in order to test whether they correlate differently with many potential covariates, can potentially shed a new light upon some of the most interesting findings in the current democratisation literature and the fledgling new studies of autocratic regime types, and may produce evidence that corroborates previous findings, while showing that others may be less robust than we previously believed.

4.2. The limits of extant autocratic typologies

Continuous measures that try to capture some sort of "level" of democracy or autocracy and map them onto a unidimensional scale spectrum come with various benefits. For one, they are easy to use in a regression as they can be treated as interval-level variables. Differences of scale, not just type, are enticing as they allow for a much more fine-grained analysis.

It is therefore unsurprising that, as statistical methods and computational power increased in the last few decades, the literature progressively turned from categorizations that were perceived as too simple, or even coarse, towards something that was considered as the next generation of political measurement, liable to fine-grained statistical analysis.

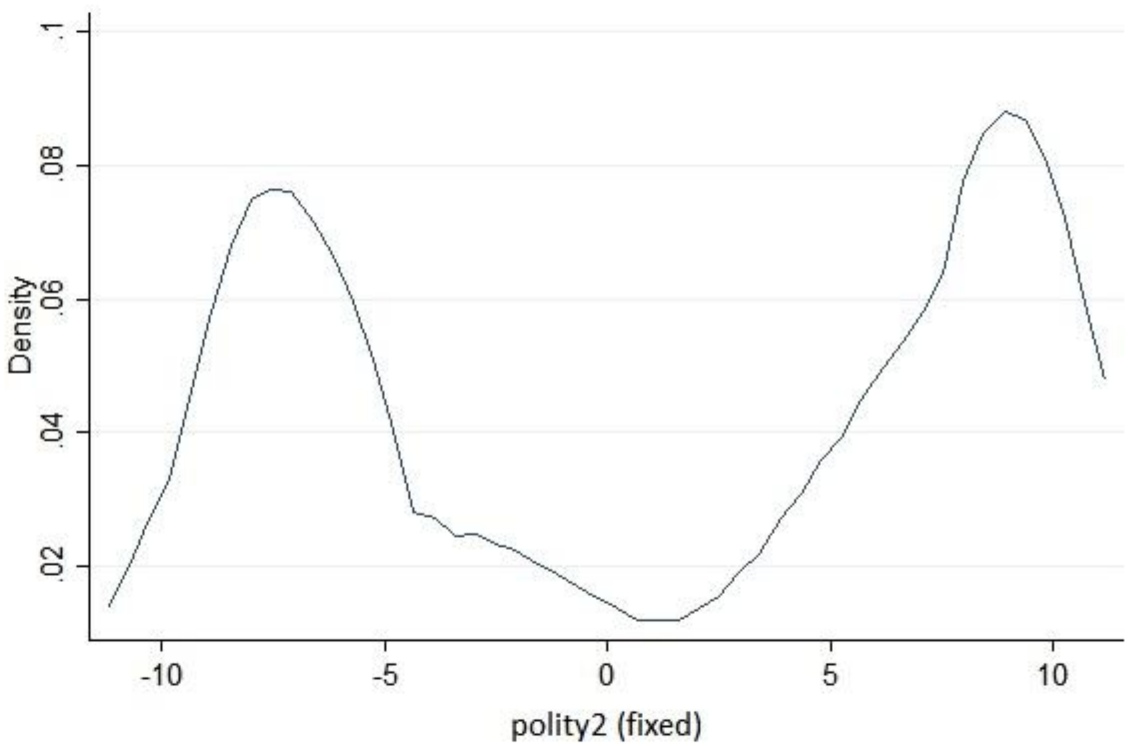
However, democracy indexes come with a number of strings attached. One of the most important liabilities is that they *can* be treated as interval-level variables, but they are such only by construction. Who is to say that the difference between a country that scored -6 and one that scored 2 on the Polity scale is the same as the difference between a country that scored 2 and one that scored 10 – especially given that the same Polity score can be reached through different scores along different dimensions of authority patterns?

A second major problem, which follows from the first, is that countries at similar levels of a univariate democracy scale might be different in some crucial respects, and that while the scale is suggesting researchers to conflate two countries into a single category, some fundamentally different characteristics between these countries may be lost in the analysis and could not be recovered.

The urgency to rethink the way in which we approach the quantitative analysis of comparative politics built up strongly during the last decade or so, with the renaissance of authoritarian studies. This new wave of literature, which focuses on authoritarian regimes and investigates the correlates of their durability, felt the urge to "unpack" countries that scored similarly towards the autocratic end of the Polity score spectrum. The problem was compounded by the fact that the Polity index tends to score countries towards both ends of the spectrum (see Figure 4.1), while countries that are in an intermediate position are both rare and may be confused with countries experiencing some other kind of polity failure (Plümper and Neumayer 2010).

Moreover, the Polity score would only allow the study of transitions from autocracy to democracy, or democratic breakdown, but it could not provide researchers with a tool to study transitions between different types of autocratic regimes. It would also not allow researchers to study times in which the political regime remained at a similar "level", but some other kind of transition occurred – such as from a military autocracy to the next after a coup d'etat.

Figure 4.1 – Kernel density estimate of the Polity score (fixed), 1970-2012



Possibly the largest and most consistent attempt to date that tried to reassess the literature on regime change and democratization by focusing on different types of transitions within and between autocratic regimes was published in 2014, as the ultimate product of a 5-year project financed by the US National Science Foundation (Geddes, Wright and Frantz 2014). The authors produced what they named as the Autocratic Regime Data Set, taking off from the premise that some transitions in authoritarian states may not be captured by traditional democracy scores. This may happen in two ways: either regime survival under new leadership, or an autocratic regime being replaced by a different type of autocratic regime.

In the first case, this kind of “transition” is similar to any democratic change of power after free, open and contested elections: clearly, and contrary to typical democratic elections, in autocracies these events may not be foreseen in advance, and their outcome in terms of regime survival might be much more uncertain. But when the outcome *is* regime survival under a new leadership, GWF consider that the transition has been successful in preserving the previous regime. Take, for example, the autocratic regime of Anwar al-Sadat, who was President of Egypt from 1970 until October 1981. Due to his efforts to reconcile his country and the whole of the Arab world with Israel (the Egypt-Israel Peace Treaty was signed in Washington in 1979), he was assassinated by Islamic extremists. After his death, however, it took the Egyptian civilian and military elite just eight days to select a new President, Hosni Mubarak, and swear him into office.

While the survival of the Egyptian autocratic regime was facilitated by Mubarak having been Vice President for six years before Sadat’s assassination, the outcome was a pretty smooth transition *within* the same authoritarian regime, and Mubarak would go on to be President of the country for the next 30 years, until the Arab Spring protests convinced the military to depose him. At the time of his demise, he was the third-longest serving leader of a Middle East and North Africa country, after Muammar Gaddafi (Libya’s leader, 40 years) and Ali Khamenei (34 years at the time of writing, still serving as Iranian Supreme Leader).

At the same time, transitions can also occur *between* different types of authoritarian regimes. Consider the 1979 Iranian Revolution, in which more than one year of protests culminated in the Shah Reza Pahlavi being ousted from power and forced to leave the country (he actually ended up being granted asylum in Egypt by Anwar al-Sadat himself). As the Shah fled, a completely different type of autocratic regime took hold in Iran, led by a different elite

(Muslim clerics) and in a style which was very different from the Shah's self-styled monarchic reign.

For their purposes, GWF define a regime as a set of formal and informal rules that "determine what interests are represented in the authoritarian leadership group and whether these interests can constrain the dictator". A transition, here, is defined as one in which the basic rules that denote the identity of the regime leadership group change.

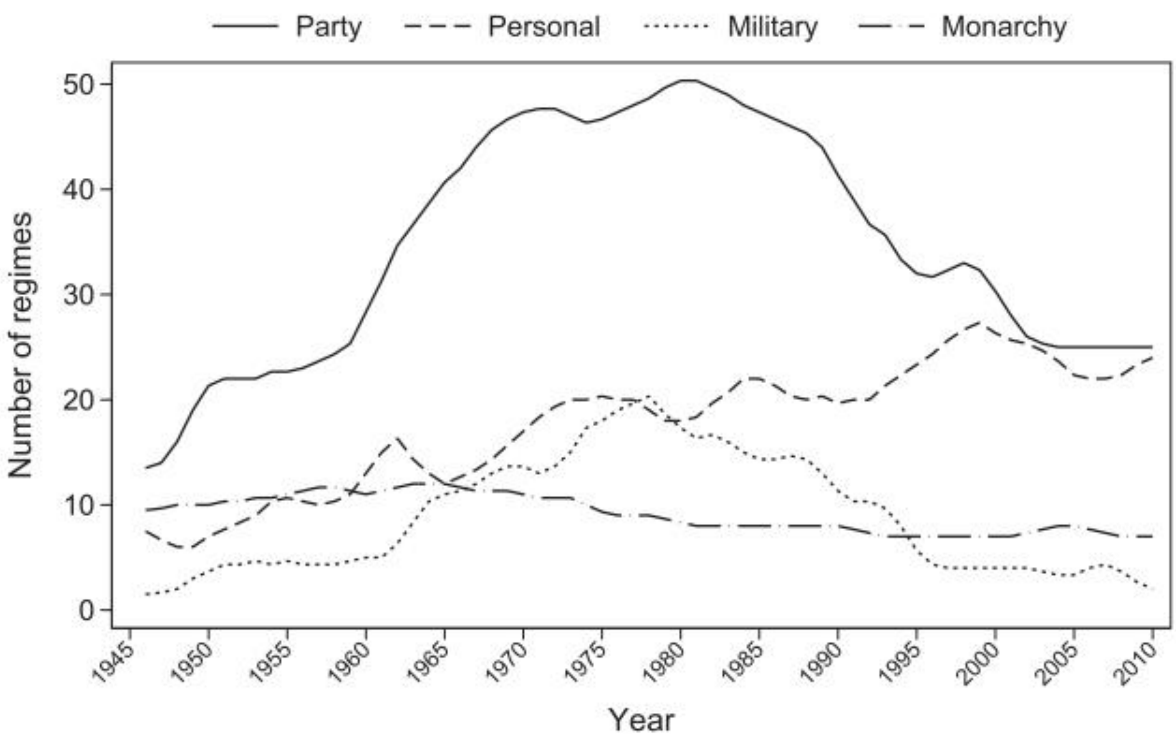
In their classification, an autocratic regime can be considered to fall within one of seven types, of which four are the most frequent:

1) a *dominant-party* regime, in which a single party and its leadership hold power and the security apparatus is also under their control;
2) a *military* regime, in which the control over policy and leadership selection is held by the military institutions;
3) a *personalist* regime, in which power is confined to a very narrow group of people centred around a single person; or
4) (d) a *monarchy*, in which political rule is invested upon a royal family and succession is institutionalised.

Figure 4.2 shows the evolution of such political regime types over time, starting after the Second World War, according to the GWF coding.

Apart from the merits of explicitly trying to devise a way to distinguish between different autocratic types, GWF's attempt has at least another important value: by comparing the coding with the Polity score, one finds that a small percentage (but not an insignificant one) of regimes that are coded as autocratic by GWF actually score between -3 and +5 onto the polity scale.

Figure 4.2 – Regime types over time: GWF categorisation, 1945-2010



Source: Geddes et al. 2014

At the same time, most autocracies falling into the four main GWF types score highly similar Polity scores. In fact, the median military, personalist and party type all score -7, while monarchies tend to score just a bit lower (-9 or -10). This goes to show that a single, unidimensional scale is sometimes unable to capture the ambiguities and complexity of the autocratic regime spectrum, and that at the same time it can struggle to paint a precise picture of variations in autocratic rule even for those countries that the expert consensus would confidently place at the end of the autocratic spectrum.

Before GWF published their index, others had gone down the path of classifying different regime types. I review some of these attempts here in order to highlight differences and similarities between them, but we do so only briefly because, as will become apparent soon, these important efforts all try to distinguish between different *types* of regimes, but do not try to measure in some way the degree of "openness" or "closeness" of political regimes.

A widely used data set on autocratic regime types is the so-called Democracy and Dictatorship Data Set (also known as DD Index, or CGV as per the initials of the last names of its three authors: Cheibub, Gandhi and Vreeland 2010). The authors offer their own take at how to tell democratic and autocratic polities (which they refer to as "dictatorships") apart. They choose a minimalist definition of democracy, but they add the requisite of at least one alternation in power. This way, countries that have a dominant party that has won all elections since they have been established fall under the "dictatorship" category, even if the elections happen to be free and fair. This is not ideal, but CGV decide to go down this road in order to minimize Type I error (false positives when identifying democracies) while ignoring Type II error (false negatives). At the same time, CGV acknowledge that other scholars might be more concerned with the inverse error and offer the opportunity to reverse the dataset by coding a dummy for dubious cases.

They then go on to distinguish between democratic types, resorting to the classical tripartite classification of parliamentary, semipresidential and presidential democracies. Finally, they classify "dictatorships" into three types: monarchies, military, and civilian dictatorships. Recognizing that "there is no clear agreement on the dimension along which dictatorships should be distinguished", they go on to argue that what is important is not, as it is for GWF, the way in which different autocratic regimes retain control over access to power and influence, but it is the way in which leaders are removed from power.

For CGV, "members of the ruling elite constitute the first major threat to dictators". Authoritarian countries can be told apart according to the characteristics of the elites that prop up each leader or group of people that rules within each country. Monarchies rely on family and kin networks to come to power and preserve it; consequently, according to CGV, around 70 percent of monarchs are replaced by family members. Military dictatorships almost always come to power following coups d'état, and tend to rule through small juntas. In around half of the cases, they are deposed by other members of the military. Finally, civilian dictators cannot directly appeal to the armed forces and need to co-opt high-rank figures from the military in order to secure their tenure and remain in power. They tend to rule through parties, in an effort to institutionalise their rule and give important roles to their allies. Their demise generally follows no clear pattern, because they may both turn more democratic, turn more repressive, or be deposed by different civilian or military rulers.

In practice, the CGV dataset is not only focused on the way in which autocrats lose power. The characterisation of each regime is much more fine-grained, and it may crucially serve important, different purposes. Their dataset is a treasure trove for comparative politics scholars, and that is exactly where I will turn to in order to create my “regime openness” variable in section 4.2.

Finally, a third important dataset is the Authoritarian Regimes Dataset, first proposed by Hadenius and Teorell (2007) and refined by Wahman, Hadenius and Teorell (2013). Their typology classifies any regime based upon the modes to access and conservation of political power: “(1) hereditary succession, or lineage, (2) the actual or threatened use of military force and (3) popular elections”. This allows the authors to distinguish between a first set of political regimes: monarchies, military regimes, and electoral regimes. The latter category can be further specified, based on how competition within these regimes takes place. Therefore, the authors classify regimes that allow multi-party competition (multiparty regimes), those that only allow the government party to compete (one-party regimes), and those that ban all parties (no-party regimes). Here, then, the authors focus on the institutional features that distinguish each regime, irrespective of the characteristics of their leaders, or ideology.

The datasets produced by GWF, CGV and WHT tend to agree for a large part. In particular, if we compare which regime they consider to be autocratic, and take the two categories that can be said to appear in all the datasets, the level of agreement is above 99% for all three pairs of indicators in the case of authoritarian monarchies, while they agree between 83% and 90% of the times when categorising military autocracies (Wahman et al. 2013).

When the datasets do not agree, the reason mainly rests in the fact that they are built upon different premises. On the one hand, GWF and CGV tend to be somewhat more similar in their approach to categorising autocratic regimes due to the fact that they both retrace the crucial difference between different types of autocracies in the “identity of the group from which leaders can be selected” (Geddes, Wright and Frantz 2014), or the “inner sanctums where real decisions are made and potential rivals are kept under close scrutiny” (Cheibub, Gandhi and Vreeland 2009). On the other hand, WHT prefer to start from the type of institutions that each autocratic leader and the elites supporting them decide to rely upon to regulate the use and the specific powers of public authority (with the ultimate aim to increase their chances to survive in power).

CGV and GWF are at odds over the threshold that needs to be overcome befrore any country can be considered democratic, as GWF requires that no large party is forbidden to take part to an election, while for CGV it is enough that more than one party participates in elections. However, CGV becomes far stricter when, as stated above, it requires countries to undergo at least one alternation in power before they can be considered democratic. At the same time, both datasets either do not impose a threshold (CGV) or set it at just 10% of total population (GWF) for the "participation" side of their democracies, so that a country may be labelled a democracy even when no universal suffrage is granted.

In turn, WHT relies on an empirically-calculated "threshold" of democracy scores, which combines Polity and Freedom House scores into a single 0-10 index, and then chooses a cut-off point based on the average of other datasets that already categorise democracies and autocracies in a binary way, before possibly typifying both into sub typologies. While interesting, this approach is highly ambiguous in practice, and tries to solve empirically the very important problem of where to precisely set the democratic threshold, at a time when many other scholars are trying to do away with empirical anchors and systematise the world through the refinement of concepts/requirements for types of democracies and types of autocracies.

There are other differences to these categorisations. For example, CGV code any regime where a former member of the armed forces is head of state as being military, while for GWF and WHT what is relevant are the institutions supporting the leader, not the leader himself. On the other hand, GWF uses a "personalist regime" category, while WHT does not. This is due to the fact that WHT only tries to rely on institutions for its categorisation, and therefore considers that the personalisation of a political regime cannot become an intrinsic institutional feature of such a regime, and should be regarded as separate and not relevant when it comes to distinguishing between autocratic regime types. Finally, WHT allows for hybrid regimes that cross more than one of their category: while this grants more latitude to researchers who are unsure over in which category exactly to place a specific polity within the classification, it also removes some precision and makes the dataset's use in quantitative analysis more complex.

Taking a step back from these attempts at categorising regime types, it is crucial now to make some more general considerations that stem from common features of most similar attempts to be found in the literature. For as useful as they may be, a key problem with these datasets is that they do not allow to assess whether a specific type of regime is more or less "close" or "open" than another in terms of political participation and voice of regime rivals (whether excluded elites or citizens). Therefore, these categorisations – though possibly making a conceptual step forward from interval-level democracy scores – are still some way off from what it would be needed in order to link the theoretical model described in Chapter 2 with empirical observations.

More important still, all these categorisations tend to be fixed and static once the institutions of the regime, or of their leader, have been set out. However, who is to say that within every single regime type, conditions do not change and leaders do not slowly open up the way in which they organise political relationships within the polity, or on the other hand crack down on opponents? A model that only distinguishes between autocratic regime types might miss relevant intra-regime dynamics, and therefore take us back to where we started in terms of the possibility to link our theoretical model on regime choice and regime stability with reality – in order to test its implications.

4.3. Conceptualizing and measuring regime openness

What exactly is "regime openness"? Can we get a sense of it for each country at each point in time? Is it possible to measure it, and if so, how? When reflecting upon this question, one has to decide what makes a regime more open or closed than others, and whether such selected features of a political regime are liable to measurement.

From a survey of the literature, it is hard to come by to even something similar to the concept of openness of a regime. Almost certainly, it has to be a multidimensional concept; but it is hard to get a sense of where to look for, and such an elusive concept risks being associated to everything and, in the end, nothing.

The risk – that I will decide to run – is that I am trying to capture something which does not represent the regime *per se*, but its manifestation at particular points in time. A regime's public order policies, for example, or its tendency to repress protests or to let them vent naturally, may be a function of different elements, some or most of which may be exogenous to the regime itself but just a reaction to some socioeconomic features of the polity, or to one-time shocks.

For example, the violence of a protest might be relevant for a political elite and leaders in order to decide whether to resort to repressive measures, and with what intensity; but the intensity of such violence might be the result of a number of factors that escape the specific control of the regime, and so it would not be wise to treat such intensity as a proxy for "regime closeness". At the same time, the frequency of protests, or of challenges to the regime in the past will likely affect the choice of which tools to use in order to appease demonstrating citizens. Another political occurrence which is liable to the same criticism as protests is, for example, the level of intra-elite infighting. At the same time, intra-elite discord or outright clashes would be even more difficult to capture, given that such feuds might brood for years without exiting the "inner sanctum" of the ruling elite, and thus be impossible to actually observe and measure – sometimes not even *ex post*.

Also, the level of closeness of a regime might be linked to the level and intensity of intra-elite infighting in complex ways: it may be a consequence of it, as the more a regime closes down, the more it may tend to exclude relevant/powerful elites; or it might be a cause for it, as more infighting pushes the regime to tighten up its policies in order to silence dissenters. Take China: is the current anti-corruption campaign by its President, Xi Jinping, a result of increasing dissent (possibly caused by the country's economic slowdown and the increasingly unsustainable debt burden), or is it a cause for the increasing number of persons within the Chinese Communist Party who appear to be more defiant, if not towards the President then towards the Prime Minister, Li Keqiang?

All these problems notwithstanding, I need a measure that is flexible enough without being too pliable. This way, if I make mistakes in conceptualising and operationalising my measure of regime openness, I will always be in time to tweak its main components, or the weight I give to each one of them, in order to test the robustness of my final results to validity/reliability problems. Ultimately, I need an index that is able to capture a range of policies and institutional settings that can be changed by the political leaders of each regime and that, if changed, would either weaken the autocrats' hold on power while allowing him to extract more information from citizens and non-ruling elites, or strengthen his hold on the regime while sacrificing some knowledge about his subjects.

For my attempt at capturing such an ever-fleeting concept as regime openness, I choose to rely upon existing datasets, searching for those that best approximated my idea of openness and that offered enough flexibility and sub-dimensions to choose from. At first, the NELDA dataset appeared to be fit for purpose. NELDA is the acronym for National Elections Across Democracy and Autocracy. The dataset, created and maintained by Susan Hyde and Nikolay Marinov (2012), provides detailed information on all election events from 1960 onwards. For each election, the authors captured 58 relevant features by relying upon a set of questions. Some of them are more traditional, like: "Was opposition allowed?", "Was more than one party legal?", "Before elections, are there significant concerns that elections will not be free and fair?". But some other offer much more latitude in order to grasp the degree of regime openness, such as: "Had the incumbent extended his or her term in office or eligibility to run in elections at any point in the past?".

Although a very interesting experiment, I ultimately judged the NELDA dataset to be not fit for my purposes because it only gives information for countries that allow for some kind of electoral competition to take place. While I consider this to be an important sign of regime openness, elections are not everything that there is to it.

For instance, an autocratic regime might choose not to hold elections but show to be open to its citizens/elites by letting them express more freely through other formalised means such as government meetings, assembly speeches, public discourses by opposition leaders, or through other direct or indirect, institutional or informal types of signalling.

Ultimately, NELDA was discarded, but it will be useful as a way to check for the validity of our preferred measure of regime openness, whenever a country does hold elections. For the same reasons, I also decided not to rely upon another data set of political regimes measuring electoral contestation (Boix, Miller and Rosato 2013).

I therefore turned to the CGV dataset, that has been introduced in section 4.1. There are many reasons for this choice, but the first and most important one is that the CGV dataset essentially captures institutional features of a given polity. However conservative the authors may be as to the number of actions that an autocrat or the ruling elite may undertake in order to “open up” or “close down” an autocratic regime, many of its dimensions might be useful in order to capture some of the features of a polity that are liable to change over time, even within a given regime, and that may affect the level of perceived openness of the regime.

At the same time, by relying upon institutional features of the regime (even when they are *de facto*, not legal features; see below), I can be more confident that my proposed measure is exogenous to potential correlates of regime type and regime change such as the frequency of repression, violent protests, or purge events.

Once I decide to go for the CGV dataset, I first use the GWF dataset to tell democracies and non-democracies apart – I prefer it to the CGV dataset because, as explained in section 4.1, the CGV tends to be too strict when deciding when a political regime can in fact be considered to be a democracy. This will be my first bipartite division, between democracies and autocracies. But I still have to generate (at least) two autocratic regime types, and tell them apart by some measure of regime *openness*.

In order to do so, from the CGV dataset I select 7 subcategories:

a) *exselec*: Mode of effective selection of the chief executive of the polity. It is coded 1 when the leader is directly elected (directly by popular vote, or by voting for committed delegates that are only selected for the purpose of nominating the chief executive); 2 when there is an indirect election (so that the selection of the effective chief executive happens through an elected assembly or an elected but uncommitted electoral college); 3, when it is nonelective;
b) *legselec*: Mode of legislative election. It is coded 0 when no legislature exists (it includes cases in which there is a constituent assembly without ordinary legislative powers); 1 when there is a non-elective legislature (e.g. because legislators are

selected by the effective chief executive, or on the basis of hereditary rules, or ascription); 2 when there is an elective legislature (at least the members of the lower house of a bicameral legislature must be elected, either directly or indirectly);

c) *closed*: it describes the status of the legislature. It is coded 0 when the legislature is closed; 1 when it is appointed; and 2 when it is elected;
d) *dejure*: it describes the legal status of parties. It is coded 0 when all parties are legally banned; 1 when the polity only allows a single, state party; and 2 when multiple parties are legally allowed;
e) *defacto*: it describes the de facto existence of parties, irrespective of their legality. It is coded as 0 when there are no parties; 1 if there is one party; 2 if there are multiple parties;
f) *defacto2*: it describes whether there exist parties that do not support the regime front. It is coded 0 when there are none; 1 when there are one party or multiple parties, but they belong to the regime front; and 2 when there are multiple parties, and not all of them belong to the regime front;
g) *lparty*: it describes how parties are actually represented within the legislature. It is coded 0 when there is either no legislature, or all members of the legislature are nonpartisan; 1 when the legislature is only composed of members from the regime party; and 2 when the legislature has multiple parties.

In choosing a method of aggregation, I wanted to rely on something that would create some kind of balance between the institutional and the *de facto* features of a polity, and that would be unbiased between degree of openness in the selection of the leader, and openness in the selection of a legislative assembly that might at least pose an indirect threat to the leader, for example by collecting his allies in a public body but also presenting them to the public and allowing for factions to form, or even directly challenge the leader himself (in case of legislatures that are vested with some actual powers).

I choose to do this by aggregating the seven variables through the use of the following equation:

$$(4 - exselec) + \frac{1}{2} * (legselec + closed + dejure) + defacto + defacto2 + lparty$$

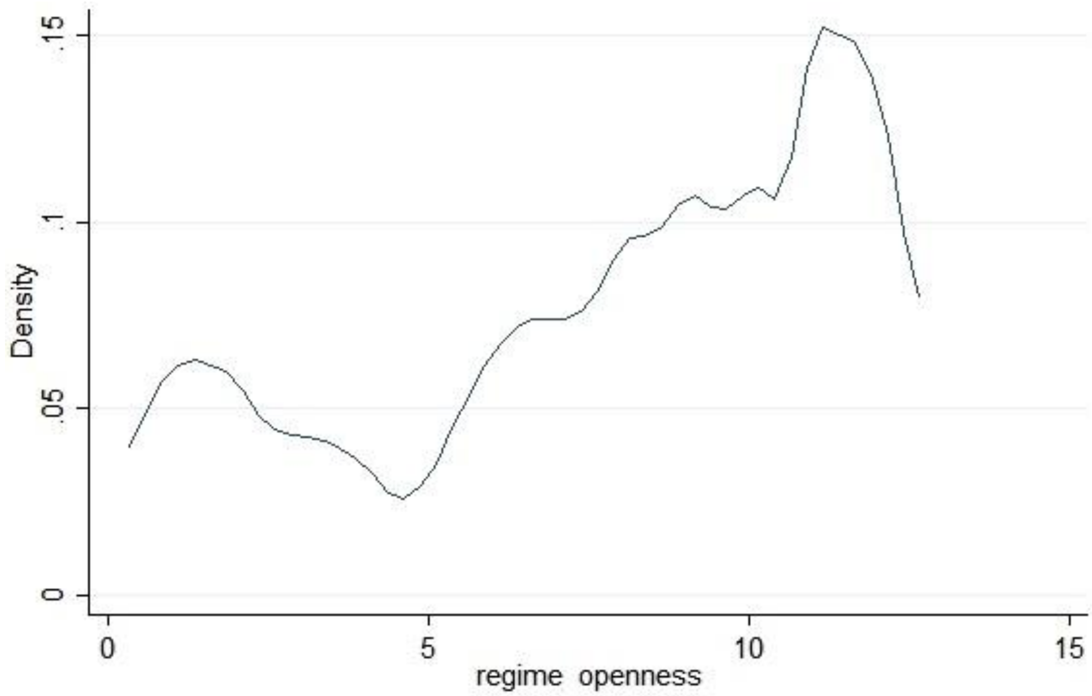
This way, *exselec* has been recoded in order to capture concepts that go in the same direction as the other variables (from least to most open), and it can vary on a 1-3 range, while legal characteristics of the legislature and the party system can vary 0-3 (it would have been 0-6 otherwise). The other variables, representing *de facto* situations, all vary 0-2, so that at most they can also reach a maximum value of 6.

I therefore obtain an index that varies in the range 1-12. As a preliminary consistency check I verify that, on this “regime openness” index, democracies (as categorised according to the CGV dataset) score between 10 and 12 in all country-years, with 99.3% of democracies scoring 11 (59%) or 12 (40%).

Conversely, for autocratic regimes, I am presented with the following distribution of scores in the period 1970-2007 as shown in Figure 4.3.

One of the interesting features of the way in which I tried to capture levels of “regime openness” is that I avoid the huge valley of very few observations between purely autocratic and democratic types that result from the *polity2* score. On the contrary, I generate a measure that appears to be sufficiently well-dispersed.

Figure 4.3 – Kernel density estimate of "regime openness" for non-democracies, 1970-2007



On the one hand, democracies still cluster to the far right of my measure, confirming either that democracies tend to share very similar elements to one another or the inability of "openness" scores to properly capture differences between democracy levels *within* democracies. On the other, the figure shows that the "autocratic spectrum" appears to be much more spread out over values ranging from 0 to around 8, so that even *prima facie* a distinction between open and closed regimes appears feasible and encouraged by data.

Thus, I establish thresholds in order to subdivide autocracies into possible "regimes types" that are linked to their measured level of openness. According to the theoretical model described in Chapter 2, there should be two ideal types of autocratic regime: an open and a closed regime.

The problem, then, is to identify some thresholds in the regime openness continuum that I may employ in order to set a clear and reasonable distinction between regime types.

Naturally, I cannot expect to make such a choice without repercussions: especially for those countries that may shift in one of two categories depending on the thresholds I set, I may be committing Type I or Type II errors by including or excluding them from the "closed autocratic regime" category. Even worse, I have very few ways (close to none) to ascertain whether I am acting correctly by including a particular authoritarian regime in a particular year in either of the two categories, apart for when this regime held periodic elections, and I have evidence that the political elites attempted or succeeded at manipulating them according to the NELDA dataset.

Given this far-from-ideal situation, I decide to tackle conceptual uncertainty by increasing my robustness checks. I do so by using a set of different thresholds in order to distinguish between closed and open autocracies. In the remaining part of this section, I describe four different ways to divide regimes into categories according to their measured openness. In Chapter 6, I will use all of them as dependent variables in my models and check whether shifting from one to the other yields different empirical results.

When selecting these thresholds, two "natural", atheoretical measures come to mind in order to distinguish between closed and open autocratic regimes: using the median (8.5) or the average (7.92) scores of a subsample of my dataset, including all those countries that are classified as autocratic in the GWF dataset. I therefore create the *regime1* variable by using the median (coding it 1 for closed autocratic regimes when a country scores 8.5 or less in a particular year, 2 for open autocracies, and 3 for democracies); and the *regime2* variable by distinguishing autocracies through their average score.

Two other atheoretical ways to set thresholds may be devised by visually inspecting the specific distribution of regime openness scores in the sample shown in Figure 4.3. From the figure, a first threshold seems to appear at a score of 5. Therefore, I create a *regime3* variable that differentiates between open or closed autocratic regimes by relying on this threshold. Finally, a fourth threshold, although less clear, is suggested by the end of the hump-shaped part of the curve at a value of 8.5 (which is also equivalent to the median score of non-democratic regimes). This is why, in order to increase robustness, I use this threshold and divide the regime-openness space into not just three but four "types" (coding 1 for years in which an autocratic country scored 5 or less, 2 when it scored between 5.5 and 8.5, 3 if it scored above 8.5 and is autocratic, and 4 if it is a democracy), creating the *regime4* variable.

Clearly, by categorizing countries in such a way, I assume that there is a change in kind whenever a particular country scores less or more than a particular threshold. At the same time, I remain agnostic as to whether a change in kind from a closed autocracy to an open autocracy can be considered "of the same size" than a change from a closed or open autocracy to a democracy.

4.4. Regime openness over time and space

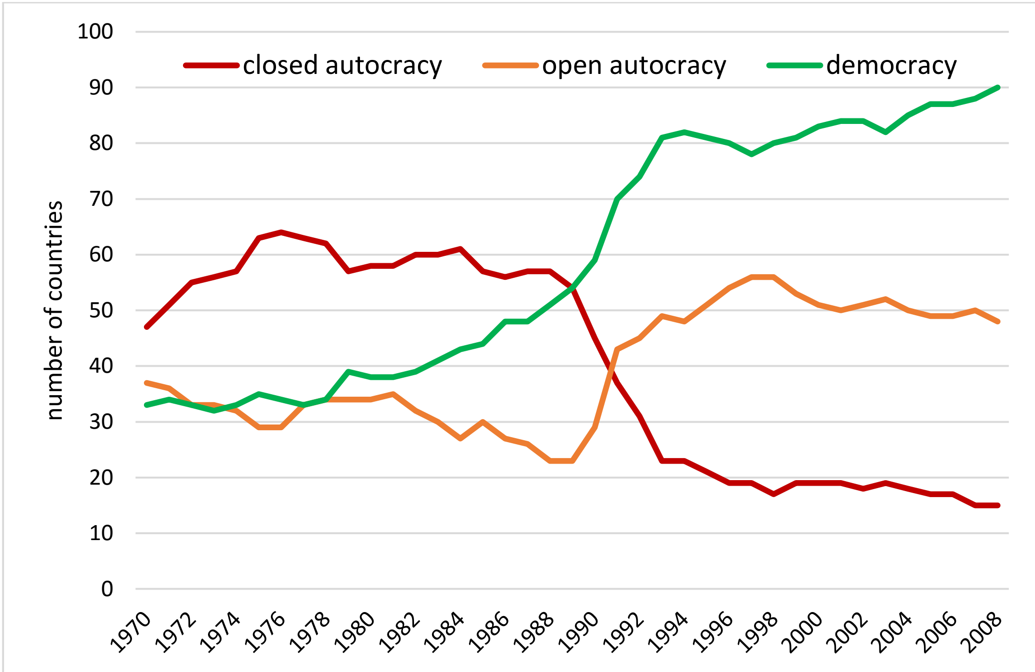
Depending on the thresholds I choose to categorise open and closed autocracies, I may obtain different results, particularly for countries that stay near or at threshold levels most of the time. For example, while the first three indexes (those that map autocratic countries onto a binary open/closed regime space) tend to agree most of the time, Table 4.1 shows that there can be significant differences, especially between the typology that employs the highest threshold (*regime1*) and the one that uses the lowest one (*regime3*). While there clearly is no problem for that part of the sample that is considered to be a "democracy" as per the GWF definition (around 43% of the sample), on the autocratic side of the sample, *regime1* and *regime3* tend to disagree in over 1 case out of 4 (this decreases to 16% if we take the whole sample into account – which is still a significant figure).

	Agree	**Do not agree**
regime1* v. *regime2	90.1%	9.9%
regime2* v. *regime3	82.6%	17.4%
regime1* v. *regime3	72.7%	27.3%

Total sample = 3,115 autocratic country-years (2,320 democratic-years excluded as all indexes agree by construction).

Table 4.1. Comparison of the first three typologies of open/closed regime.

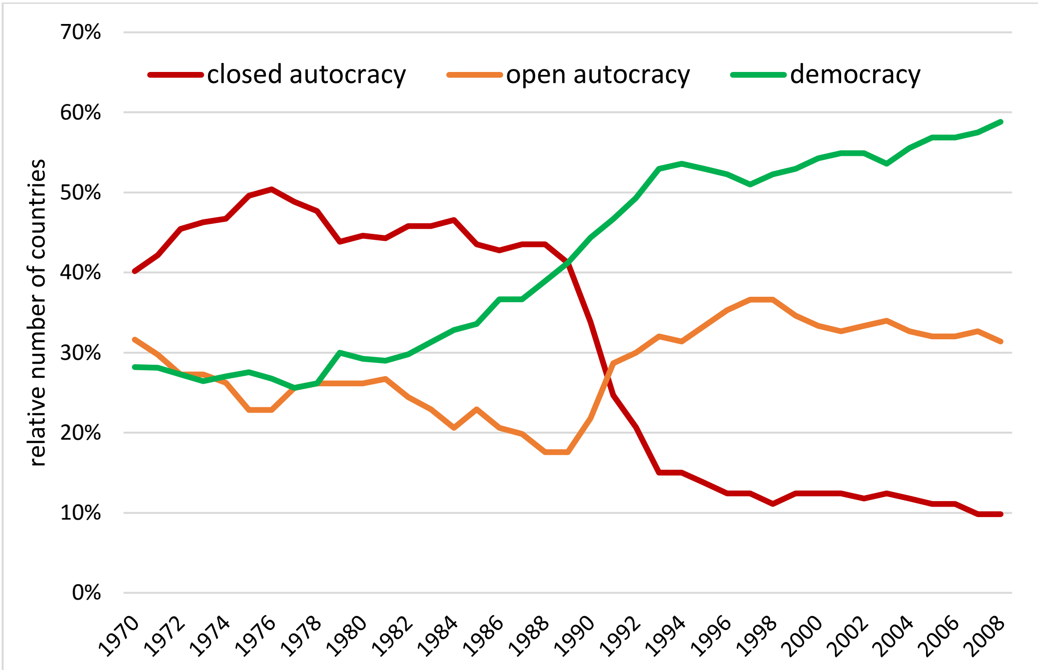
Figure 4.4 – Number of autocracies (by type) and democracies over time, 1970-2008



If one looks at the general development of "regime openness" over time through the 1970-2008 period, the three measures give a clear and unequivocal trend. For illustration purposes, I choose to rely upon our *regime1* coding. Figures 4.4 and 4.5 show, first, the clear "wave" of democratisation, which starts in the second half of the Eighties and ends in the early Nineties, both in absolute and relative number of existing countries in our sample (which increases from 117 to 153). Interestingly, the democratization wave coincides with many regimes shifting from a closed autocratic to an open autocratic setting.

After this period, the "liberalization" trend then bottoms out and remains pretty stable for some years, while a small positive trend appears at the end of the sample (contemporary studies show that this trend might have plateaued over the last few years).

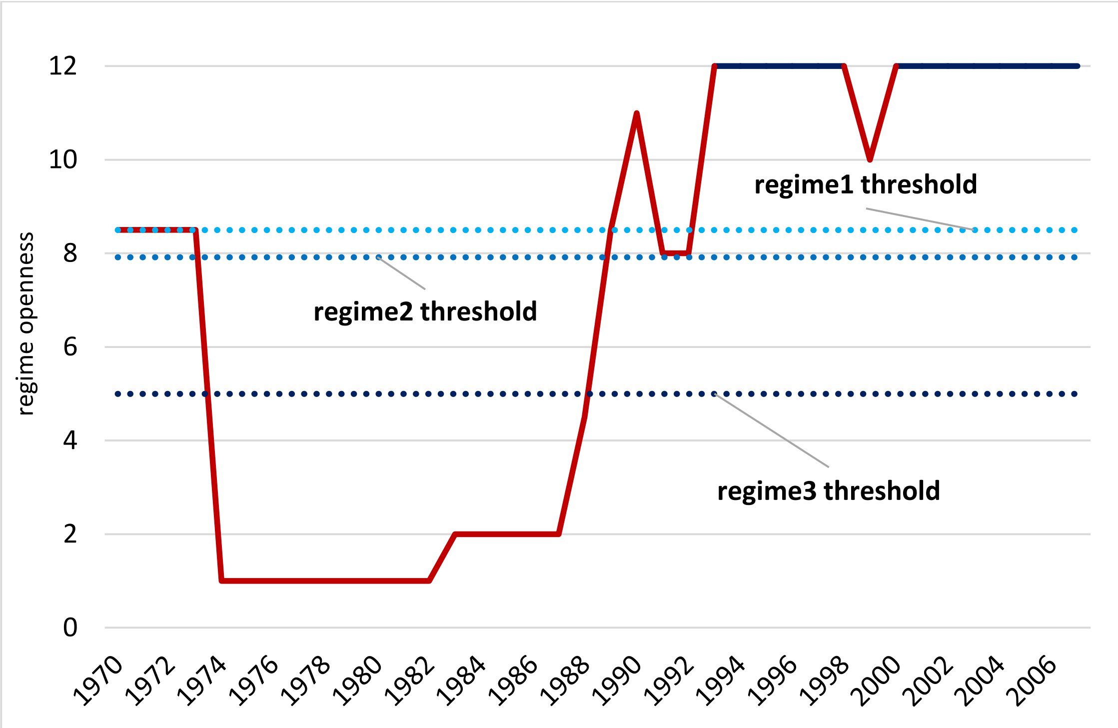
Figure 4.5 – Relative number of autocratic types and democracies over time, 1970-2008



What is even more significant for my purposes, however, is to focus on the increasing number of countries that do not complete their transition towards democracy and stay or turn into "open autocracies". While closed autocracies peak in 1975 at 50% of the sample and then decrease, reaching 10% in the 1999-2008 decade, the opposite is true for open autocracies, which experience a decreasing trend between 1970 and 1989, then shoot upwards during the 1990-1998 decade (doubling from 18% to 37% of the sample), and finally settle on a slowly decreasing trend during the last decade, but remain around a third of the sample (from 37% to 31%).

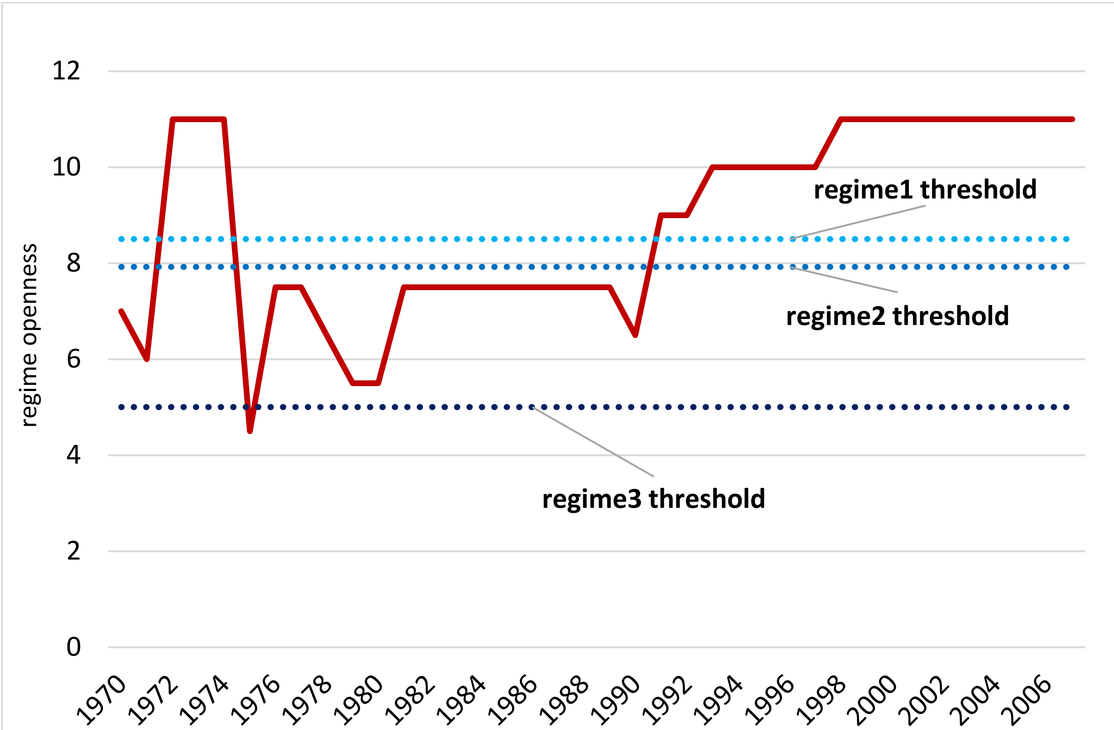
While the overall trend is clear, and appears to justify my approach in the study of the correlates of different types of autocracies at different liberalisation levels, I now focus on a few countries in order to point out cases in which the choice of a threshold makes a crucial difference, might skew my results, and therefore justifies robustness tests based on multiple operationalisations of the underlying regime openness concept.

Figure 4.6 – Regime openness and regime "thresholds" for Niger



Let's take Niger. Having gained independence in 1960, the country enters my sample right away. Figure 4.6 shows that Niger is considered an open autocracy between 1970 and 1973 by two out of three of my categorisations, but slightly fails the "open autocracy" test when using the *regime1* threshold. It then slips into closed autocracy for all three of my thresholds in the period 1974-1988 (with the 1988 score, at 4.5, coming just short of my lowest threshold). 1989 is again a case in which my highest threshold is a little too high to consider the autocratic regime as open, while the 1990 value (11) clearly allows all my *regime* typologies to classify it as an open autocracy. Finally, 1991 and 1992 are again a case in which Niger is considered an open autocracy by two of my thresholds (but just so for *regime2*, which at 7.92 relies upon the sample average), and only after 1993 all operationalizations allow me to unanimously categorize the country either as a democracy or an open autocracy.

Figure 4.7 – Regime openness and regime "thresholds" for Cambodia



The case of Cambodia is even more telling as to the relevance of different thresholds to describe open and closed autocratic regimes. As Figure 4.7 clearly shows, Cambodia spends a relevant part of its sample years (roughly until 1990, with just a brief interval of clear "open autocracy" status between 1972 and 1974) in an intermediate score that does not allow to consistently classify it across measures. Indeed, between 1976 and 1990 the country constantly scores between a minimum of 5.5 and a maximum of 7.5, thus being categorized as an open autocracy by my *regime3* variable, and a closed autocracy for both *regime1* and *regime2*. However, for a significant portion of this period (1981-1989) the country continues to score 7.5, which is barely short of my *regime2* threshold of 7.92.

To conclude: given how sensitive is regime type to different operationalizations, and sometimes even to very slight changes of the underlying threshold, empirical models might be especially sensitive to potential miscategorizations. However, given the fact that it would be very hard (and possibly improper) for me to select one out of the four different thresholds based upon theory or previous literature, I will have to rely upon all four of them as robustness checks in the empirical analysis that I carry out in Chapter 6.

4.5. Conclusion

In this Chapter I surveyed the current state of the art in the literature that tries to subdivide the autocratic space along different typologies. I then went on to propose a novel way to look at typologies across autocratic political regimes, and developed a new measure of "regime openness" that could be best suited to gauge levels of liberalization along the autocratic spectrum.

Having done that, I produce four original typologies of autocratic regimes: three of such measure are dichotomous along autocracies, dividing the space between "closed" and "open" autocracies. The fourth measure adds a measure of complexity by subdividing the autocratic space into three different autocratic types.

Along with democracies, which do not change across my measures, I find these typologies highly suitable to my analysis of the correlates of liberalization trends. Such analysis is different, and to many extent original, as compared to the classic democratization literature, but in many ways it runs parallel to it.

In Chapter 2, along with deriving my own set of hypotheses, I found some hypotheses within the democratization literature to be generalizable to a liberalization context and, therefore, amenable to testing, which will span the bulk of Chapter 6.

For now, I regard this as a first, important attempt at going beyond current measures of political regimes, with the potential of shedding new and important insights on the causes and correlates of political regime choice, stability, and change.

Chapter 5. Measuring Political Leverage of Fuel Rents

5.1. Introduction

In this Chapter, I make a novel contribution to the literature on the resource curse, by attempting to devise a measure that can gauge the potential "political leverage" of rents derived from the extraction and sale of hydrocarbons (crude oil and natural gas, in all their forms).

I review current measures, pointing out benefits and costs of employing each one. I then proceed to create my new proposed measure. My contribution to the operationalization of the leverage allowed to political leaders by the proceeds accrued to the state from hydrocarbon rents is twofold.

First, I attempt at measuring absolute rents by combining multiple source and then relying on multiple imputation methods (see Chapter 6.2). Having done this, I then use my new estimate in new, straightforward measures of fuel rents that, being relative to a country's per capita income, can more precisely gauge the latitude bestowed upon political leaders by these resources.

I will then take some time in showing the consequences of my choice in terms of how fuel rents vary over time and space, and focus on a few interesting features of fuel rents over my sample period (1970-2008 – although here I extend it a little forward, to 2012, this has no substantial effects on my findings).

5.2. The limits of extant measures of political leverage of rents

The literature on the resource curse postulates that countries dependent on some specific types of natural resources are not just blessed with them, but also suffer from various negative effects. The political interpretation of the resource curse theorises that the rents that political leaders derive from natural resources increase the overall wealth of the regime. Political leaders may then use such rents in order to buy legitimation from their subjects, buy out competing elites, or to pay for the loyalty and employment of their country's holders of repressive means.

These three mechanisms postulate that we should expect a stabilizing effect for autocracies, and possibly a reluctance of autocratic leaders to embrace the full spectrum of "legitimizing" means for an autocracy – including elections, or tolerance towards displays of dissent.

One could expect this effect to be even stronger for a very specific kind of natural resource, i.e. hydrocarbon (oil and natural gas) resources, and this for different reasons. First, hydrocarbon's high value compared to the overall gross domestic product of "rentier" countries, and their high-value content in terms of volume (differently, for example, from agricultural resources or timber).

Second, hydrocarbon resources require substantial capital investments at the identification, exploration, and development stages, but – at least for conventional oil and gas fields – after these early stages these investments allow production to flow for years, or even decades, at very low operational costs.

And third, these resources are "non lootable", meaning that in order to be extracted they need infrastructure, and they cannot be stolen or moved around as easily as other high-value resources such as gold or diamonds.

In order to measure the "leverage" that each political leader (especially autocrats) may gain from relying on hydrocarbon rents, I need to find a way to measure such rents in a meaningful, consistent, and reliable way. The literature on resource dependence offers an incredibly high amount of methods that may be employed in order to operationalize such dependence.

Table 5.1 reports ten such different measures: each of them has been proposed at various points in time over the last two decades. Digging deeper in the resource curse literature, along these measures one may find an an almost infinite amount of small variations.

At closer inspection, however, most of these measures suffer from a series of weaknesses that risk , biasing the results of any quantitative analysis. Some of them, such as a dummy that fires up in case of OPEC membership, are just a rough approximation of reality and leave many hydrocarbon-dependent countries out (such as Russia or Norway, for example). In fact, there is an even higher amount of oil-producing countries than there are OPEC members today, and most of these non-OPEC countries export some of their oil.

Clearly, other dummy variables that set some threshold to be passed before a country can be considered hydrocarbon dependent face significant risks of selection bias: why should a country that relies on 30.001% of its exports or GDP on oil and natural gas be considered dependent, while a country that relies on it for 29.999% of its exports should not? Moreover, dummies in general appear outdated, as sufficiently reliable data becomes increasingly available to researchers.

Continuous measures can suffer from problems as well; yet, most of them are simple to address. For example, measures of hydrocarbon dependence that rely upon the share of hydrocarbon exports over total exports cannot account for the varying relevance of overall exports themselves for different countries in the world. Bigger countries, even countries that are considered very open to global trade such as the United States, tend to export goods for a much smaller share of their GDP than smaller countries (according to the World Bank, in 2014 the US exported goods for a value equivalent to 14% of its GDP, while Estonia exported goods valued at 84% of its GDP). For this reason, this is a measure that cannot account for the actual economic leverage political leaders may gain from relying on hydrocarbon resources.

Measure	Main studies using it
OPEC membership (dummy)	Fish 2002, Fish 2005
Oil exports over 50% of total exports (dummy)	Gandhi and Przeworski 2006; Gandhi and Przeworski 2007
Oil revenues over thresholds of national income (dummy)	Davis 1995
Energy resource depletion over GNI	Abdih et al. 2008, Arezki et al. 2011
Oil exports over total exports	Jensen and Wantchekon 2004
Oil export revenues over GDP	Ross 2001, Smith 2004, Smith 2007, Morrison 2009
Oil deposits per capita	Alexeev and Conrad 2009
Oil discoveries per capita	Tsui 2011, Cotet and Tsui 2013
Oil value per capita	Aslaksen 2010
Oil and gas income per capita	Ross 2012

Table 5.1 – Measures of hydrocarbon dependence

Even measures that do away with this problem and directly measure oil revenues derived from exports over total GDP have some liabilities. One of these is the fact that they cannot account for the actual leverage over citizens that such oil rents bestow upon each country's political leaders.

Hydrocarbon rents should be compared to the share of citizens' per capita income that accrues to citizens outside from the "oil rents cycle", i.e. due to other economic activities. Moreover, such measures cannot account for the indirect income effect that the subsidised domestic consumption of fuels (products derived from crude oil, such as gasoline and diesel oil; fuel oil and natural gas used in power plants; gas used for heating, for those countries at sufficiently high/low latitudes, or hydrocarbons in general used in desalination plants) has on the citizens.

Until after the 2014 oil crash, a number of Middle East hydrocarbon exporting countries had no or very low income taxes (something allowed by the export of hydrocarbons), but also highly subsidised fuel prices. This is also true of countries that today consume a very large portion of what they produce at home, such as Russia, Indonesia (today a net oil importer, but still a large producer), or even the United States after the shale revolution.

Even more original studies, employing oil exploration and discoveries/deposits per capita, have their limits. First, hydrocarbon discoveries can take place years, or even decades, before any oil or gas comes into production in the country. Think about Uganda, where significant recoverable oil reserves were discovered in 2006; by 2016, the country still had to produce any significantly marketable amount of oil. While it is true that some income may accrue to citizens during the exploration activities carried out by international oil companies, such amounts are so marginal that they generally do not account for any significant portion of per capita income.

Second, the variability of reserves is undoubtedly endogenous to political conditions (like the decision to allow IOCs into the country, or the presence of international sanctions that target the hydrocarbons or overall commodity sectors), general business activity in the country, the region or the world, and technical estimates (the amount of hydrocarbons that is economically recoverable varies with the international and regional price of the resource).

Given that proposed measures have so many liabilities, and at the risk of adding to this cacophony with a new off-key note, I will try to use an original method in order to build my preferred measure: one that aims at directly gauging the political leverage bestowed upon political leaders by hydrocarbon revenues, but still remain as simple as it may possibly be.

5.3. Estimating absolute resource rents: primary and secondary sources

How can we measure resource rents? way In 2011, Stephen Haber and Victor Menaldo authored a new study on the relationship between authoritarianism and natural resource dependence (Haber and Menaldo 2011). The study's results were fairly controversial, finding no general effect from natural resource dependence on authoritarian stability.

The Haber and Menaldo (2011) study employed an empirical model which has been gaining traction in the recent comparative politics literature (to the best of our knowledge, Aslaksen

2010 was the first to propose a dynamic panel model to study the political variant of the resource curse literature), but which I will criticize in other parts of this work (see e.g. section 6.3). Moreover, the Haber and Menaldo study had several serious drawback: it implies that natural resource dependence starts at a country's time of independence, not of nationalization of the resource (so that rents flowing out of the country were in fact measured as if accruing to the country's government); it adds together rents from hydrocarbons and minerals alike; it uses a "fiscal reliance" variable which is clearly endogenous to state capacity; and it goes back to 1800, so that the biggest part of the dataset is comprised of countries whose reliance on oil and natural gas was clearly nil for over a century – sometimes even a century and a half. Despite all these drawbacks, Haber and Menaldo's study is relevant for the wealth of data that it produced, and for making that data swiftly and freely available to other researchers.

Before Haber and Menaldo, the only reliable source for estimating natural resource rents (whether oil, natural gas, coal, or agricultural) in a sufficiently long and non-discontinued time series was the World Bank's "natural resource depletion" data (Hamilton and Clemens 1999, World Bank 2006, World Bank 2011). The World Bank estimates natural resource rents as a percentage of GDP of a country (it is therefore straightforward to calculate their value in nominal or real dollars). The rents are directly calculated as the difference between the value of the resource being exported and sold at (some average of) world prices, and total costs of production.

Haber and Menaldo, meanwhile, go back to a long list of primary sources, and build upon them in order to rebuild an alternative dataset, taking great care in preserving consistency over time. Despite flaws in their analysis, their dataset appears to be a great alternative source to check for any discrepancies in the World Bank dataset.

At the same time, in the next section I build my own preferred version of hydrocarbon rents through an entirely different source: the United States Energy Information Administration's "International Energy Statistics". The three of them will be combined in order to arrive at at the best compromise among different sources as to the exact extent of "fuel rents" of 150+ countries in the world, between 1970 to 2012.

5.4. Measuring the political leverage of hydrocarbon rents

In order to build my measures of hydrocarbon rents and their “political leverage” I need to select a primary source, and then choose whether and how to modify it, with the aim to improving it to the best of my means.

I start from the World Bank measures of rents for oil, natural gas, coal, mineral, and other natural resource rents. The World Bank’s rent measures are obtained by subtracting the estimated total costs of producing a unit of the natural resource, to the estimated total revenues acquired from it sale (assuming an average world price), multiplied for the quantity sold each year. However, this is just a rough approximation of each countries’ actual revenues, primarily for two reasons:

> (a) total revenues are calculated using international benchmarks, which are somewhat more accurate when commodities are indeed globally priced, such as oil or coal (although significant regional or country variation may be hidden there as well), but not when markets are regionalized, as in the case of natural gas. In both instances, moreover, each country’s price tends to fluctuate at a premium or at a discount from the benchmark, depending on the quality/blend of the natural resource sold;
>
> (b) total costs are always a best guess, and they tend to be endogenous to the price of the resource. For example, when oil prices are high, services companies or international oil companies investing in exploration and production activities in a given country can ask for more rents per barrel, on average. On the other hand, when the price crashes, the first to lose out tend to be the “middlemen”, as countries do their best to maximize the part of revenue that accrues to their coffers, while competition among upstream and midstream companies rises (Cabrales and Bautista 2014).

Moreover, the World Bank estimates did not aim at being very precise, as they were extrapolated from studies whose aim was to measure the total wealth of each country in the world. These studies were more concerned with present asset values and these, in turn, in order to be calculated could be spread out in the (discounted) future: short-term price fluctuations are less of a concern to such studies.

Noticing how hard it is to reach sufficiently reliable and precise estimates of resource rents, this only reinforces my belief that it would be very difficult to use rent variation over time in order to try to capture its effect on political outcomes: at the intra-country level, the noise in the apparent yearly deviations from a country's average may well drown out the signal, giving rise to spurious and biased correlations (see Chapter 6.3).

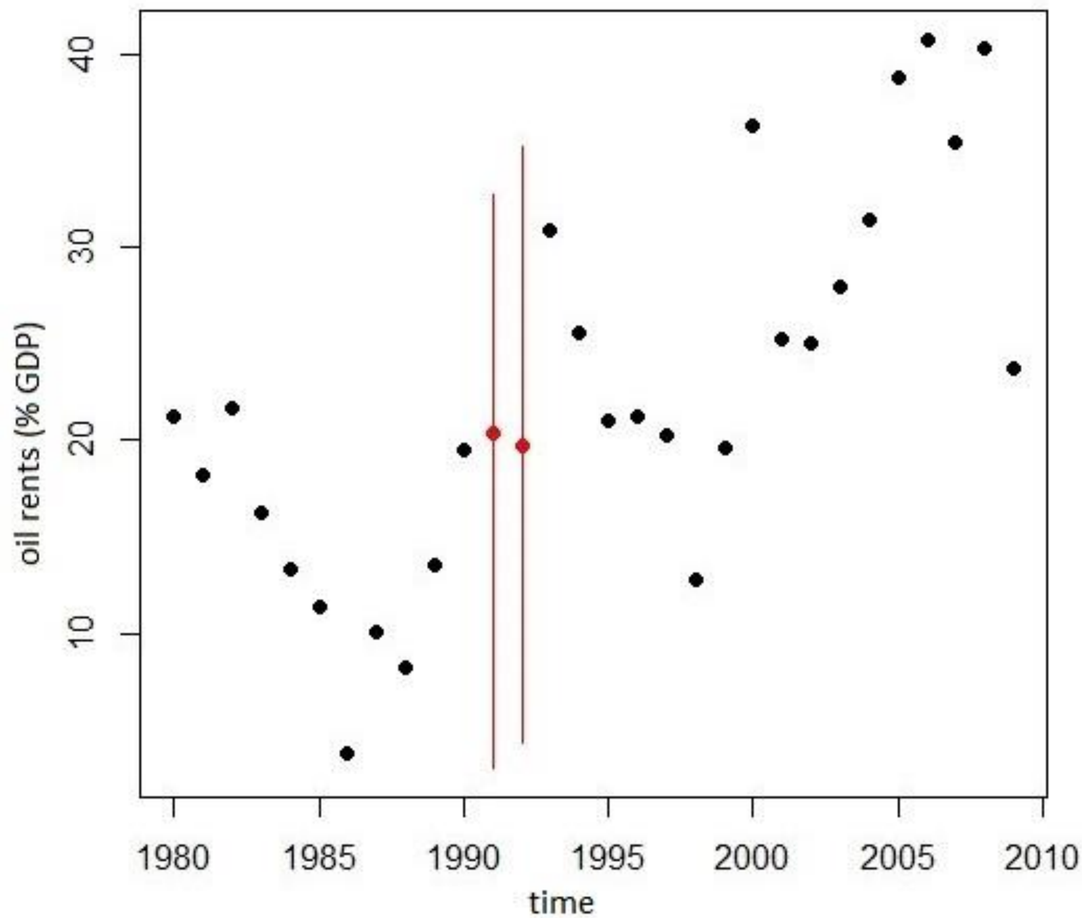
Also, the original World Bank dataset reports a huge number of missing values. Most of the time, these represent values for countries whose hydrocarbon and/or natural resource rents are equivalent to zero. However, this is not always the case.

For instance, data for Saudi Arabia data starts in 1991, while Kuwait's starts in 1995. In order to recover estimates for previous years, I first compare the World Bank data to Haber and Menaldo (2011) and to other sources (such as Smith 2004 and Ross 2012) to check which data point can be confidently set to zero. Whenever I expect, instead, that a missing data point is signalling a non-zero actual missing value, I leave the value as missing and prepare the dataset for multiple imputation (for a description of multiple imputation inference and analysis, see section 6.2).

I include Haber and Menaldo estimates within my dataset, so that the technique can account for that data and its correlation with my estimates: this will allow me to greatly improve the efficiency of multiple imputation techniques and impute much more reliable data to replace missing values, all the while shrinking the estimated uncertainty around that data point.

Multiple imputation includes time-series cross-section dependencies, and uses one time lag of significantly time-dependent variables (oil rents included) in order to estimate probable values of missing data. It also includes dozens of other variables that may correlate or affect oil rents, such as the recorded occurrence of major episodes of political violence within a country, or the oil price per barrel. The procedure generates 10 multiply-imputed datasets.

Figure 5.1 – Oil rents in Iran (1980-2009)



Data: WB, modified and imputed

In order to show the usefulness of the multiple imputation procedure in this specific case, below I report oil rents as a percentage of GDP for Iran and Iraq for the period 1980-2009. Black dots represent non-missing data, while red dots represent non-zero missing observations in the original dataset. Multiple imputation methods allow me to estimate the probable values of rents in cases of missing data, along with the estimated 95% confidence interval.

In the case of Iran, Figure 5.1 shows that the series has an unexpected break for the years 1991 and 1992, but nothing “strange” appeared to happen in the country in those two years, suggesting that data was missing due to unavailability or lack of reporting rather than a “real” exogenous shock (despite the fact that the first half of 1991 was plagued by the 1990-1991 Gulf War).

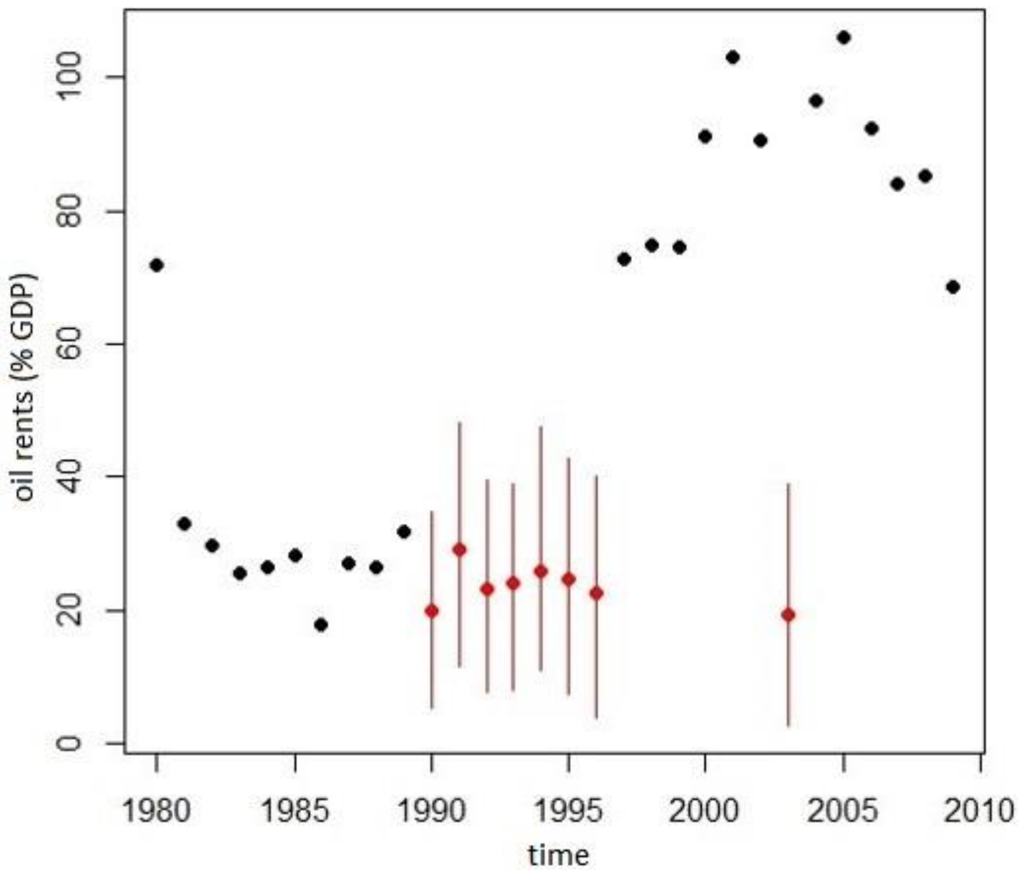
Accordingly, multiple imputation analysis estimates that rents remained pretty much unchanged in 1991 and 1992. Given the uncertainty around the reason for missingness, the estimated uncertainty around the most plausible estimates is very high, ranging from the minimum value observed through the 1980-2009 period for Iran, to the 75% percentile of the Iran sample. What is interesting to note, then, is that uncertainty is incorporated within the dataset and is not at all lost through the imputation process: this allows me to avoid listwise deletion due to missingness, and also to simply impute a value as if no uncertainty was involved in the imputation process (potentially biasing the following analysis).

Multiple imputations, and the hard process involved in estimated country-level data as shown here for fuel rents, suggest that it is quite fictitious to assume that our observed, non-missing values have a zero error around them. However, this error is conceivably much smaller than the one we make when imputing plausible estimates for missing data. Again, note that this uncertainty around yearly data can tend to bias fixed-effects models or models employing differenced data.

Moving to Iraq, the country reports missing data for the period 1990-1996 and for 2003. It is common knowledge that Iraq's history after 1990 was troubled. This knowledge is grounded on historical facts – which are unknown to a multiple imputation program, unless we feed it with sufficiently useful data!

In August 1990, Iraq invaded Kuwait, ultimately leading to Operation Desert Storm being launched in January 1991 by a US-led coalition of 34 countries. While the operation was successful in liberating Kuwait, Iraqi leader Saddam Hussein was left standing in Baghdad. Also due to this, UN sanctions against Iraq – which had been imposed already in 1990 – remained in place throughout the Nineties, making Iraq a *de facto* international *pariah*. Interestingly enough, afer the 1990-1996 break in the data, information on hydrocarbon rents become available again from 1997, two years after the launch of the UN Oil-for-Food Programme. Finally, the clear blip in the data for 2003 corresponds to the US invasion of Iraq that deposed Saddam Hussein.

Figure 5.2 – Oil rents in Iraq (1980-2009)



Data: WB, modified and imputed

Iraq offers a real test for my multiple imputation analysis: if I simply decided to impute data, I should have relied either on expert knowledge (which is scant when international statistics are unavailable), or to a seemingly "impartial" method to impute data, such as interpolating, by taking the values of the most recent past year with non-missing values, the most recent next year with actual observations, and divide it up for the years of missingness and attributing the same yearly change to every year reporting missing data.

If I were to choose interpolation, then, I would assume that the 1990 value was higher than the 1989 one, that 1991 rents were higher than in 1990, and so forth until 1997.

Instead, the software continues to impute very low levels of oil rents, which are highly plausible given international sanctions. In fact, bear in mind that this is a measure of rents as a share of GDP. This means that as GDP varies, oil rents could shoot up not because of much higher rents from oil, but also because of an exogenous shock made GDP collapse. This is something that we can assume happened throughout the Nineties. However, GDP data is also missing for Iraq until 1997, so that the software will simultaneously estimate GDP and oil rents as a share of GDP. The resulting low level of oil rents over GDP is thus the result of the software not estimating a total collapse in GDP (although uncertainty in GDP figures, not shown, is estimated to be so high that it could surely include a collapse).

Given that I will not use oil rents as a share of GDP directly in my analysis (see below), even if the software may be slightly underestimating oil rents as a share of GDP (compare the 1996 to the 1997 value), if it also slightly overestimating GDP the final output will be a much better estimate of absolute rents. Most importantly, multiple imputation also allow me to recover an estimate of uncertainty around unknown values.

Even more testing to the imputation procedure is the 2003 value: here. Figure 5.2 shows the huge benefits deriving from multiple imputation, aside from recovering uncertainty estimates, i.e. that even without "expert judgment", the program is able to guess quite correctly thanks to the many correlations included within the original dataset.

Specifically, despite 2002 and 2004 values being at levels near 100% of GDP, the 2003 value is estimated to crash to around 19%, with the 95% confidence interval estimating variation between 2% and 38%. This estimate is most probably a result of the fact that my dataset I fed to multiple imputation software also included war and civil war episodes variables, reporting both occurrence and intensity. It is therefore highly likely that the software picks up this and other instability signals, that in other parts of the dataset are correlated with an observed crash in economic activity and/or low values of fuel rents. Looking closely, even the 1990 point estimate for Iraq is much lower than 1989, possibly due to the dataset signalling the occurrence of the Gulf War.

Multiple imputation allows me to recover a number of otherwise missing rents data, such as those for the whole Libya time series, for Syria after 2007, for Turkmenistan's natural gas rents (which are spotty), UAE oil rents in the period 1971-1974, and some point estimates for Kuwait, Qatar, and Afghanistan (coal and natural gas rents).

Multiple-imputed methods thus allow me to recover plausible estimates for missing values and to continue to account for the uncertainty around estimates that replace missing data. After the imputation step (which, see section 6.2, also serves to recover all missing values for other important covariates in my dataset that suffer from a high degree of missingness), I am ready to calculate my preferred measures of the political leverage deriving from hydrocarbon rents.

First of all, I calculate ***rents per capita*** by multiplying relative resource rents by that year's GDP country estimate, and then dividing this value for country population data. This measure, though notably improved thanks to multiple imputation, is for most purposes analogous to the one used in Ross 2012. As explained in section 5.3, I consider this to be a step forwards if compared to just relying upon natural rents as a share of GDP, as this accounts for the number of citizens/subjects over which rents should be spread out in order to secure consensus. Although it can vary by country also as a function of the political regime, the level of rents per capita could be correlated to the potential political leverage from rents when the autocrat wants to target them not to the whole citizenship, but just to the selectorate (Bueno De Mesquita et al. 2003). Lacking reliable measures of the selectorate of each country, however, I am left with a measure of rents over the population.

All this notwithstanding, while this measure is importantly exogenous to GDP change, it can only go so far in estimating the actual and potential leverage accruing to political leaders over their citizens, because the same rents per capita would matter much more in, say, Angola or Mozambique than in, say, Norway. This is because in Norway the populations' wellbeing is already high, so that it would take much more resources and revenues per capita for a political leader to be able to either "buy" consent from the population or pay the police and military apparatus to stifle dissent.

I therefore calculate what I refer to as the ***political leverage from rents***, by taking the gross rents per capita and dividing them by the population's overall level of GDP per capita at purchasing power parity (PPP). This is the first of my preferred measures of the political leverage enjoyed by each country's leader thanks to hydrocarbon rents.

There is also another way I can tweak this measure, in other to account for other specificities of the "political leverage" proxy. This has to do with expectations and the tendency of rentier states' citizens to become slowly accustomed to the benefit they can enjoy. This may modify their perceptions of the benefit they may get from the regime, despite the actual level of benefits being higher.

Although domestic consumption of abundant resources in natural-resource exporting countries is generally subsidized, in this case I consider subsidies to be simply "lost" autocratic rents, meaning that the existence of subsidies is actually a lost revenue for the country at the long-run equilibrium. This is because I expect that when political leaders approve a consumption subsidy, this is noticeable to the population in the first few years, but tends to act as a one-time shock that will soon be "embedded" within the expectations of the general population. As time goes by, citizens will tend to take this subsidy for granted. I therefore check for this "addiction" effect and test for both total hydrocarbon rents per capita as a percentage of total GDP per capita, and for rents per capita only accruing from exports (subtracting rents lost from the domestic consumption of hydrocarbon resources).

In order to separate export rents from total rentss, I rely upon the United States' Energy Information Administration production and consumption data for 1980-2012, which accounts for all the countries in my sample, for oil, natural gas and coal. I then proceed to calculate net exports of each resource, fixing them at 0 when they are negative (this means that countries are in fact net importers for that given year). Then, I calculate the share of exports over total production (a value ranging between 0 and 1) and multiply such share my my calculation of oil, natural gas and coal rents.

I finally improve this absolute proxy by calculating the ratio of these rents per capita to GDP PPP per capita. I call this measure, which accounts for an "addiction" effect of rentier countries, my ***political leverage from export rents*** variable.

It is important to bear in mind that all these calculations are still a rough approximation of actual yearly rents. In many cases, rents accruing one year may actually appear in the state's coffers one year later (in the form of taxes, royalties, etc.), and part of them may simply disappear due to corruption or be outright stolen. Consider, for example, two recent cases in two very different countries: Nigeria and Brazil. In 2014, Nigeria's central bank governor estimated that the state-owned Nigerial National Petroleum Corporation had failed to pay upwards of $16 billion to the state's treasury (Onapajo et al. 2015, Hackett 2016). In the same year, the largest corruption scandal in Brazil's history started to emerge, involving the semi-public oil company Petrobras in the payment of owards of $3 billion in bribes, with some estimates of revenue lost due to corruption reaching $20 billion.

Estimates of yearly rents become increasingly less precise with the development of sovereign wealth funds in many autocratic countries, starting in the last decade of the XX century. Sovereign wealth funds allow political leaders to:

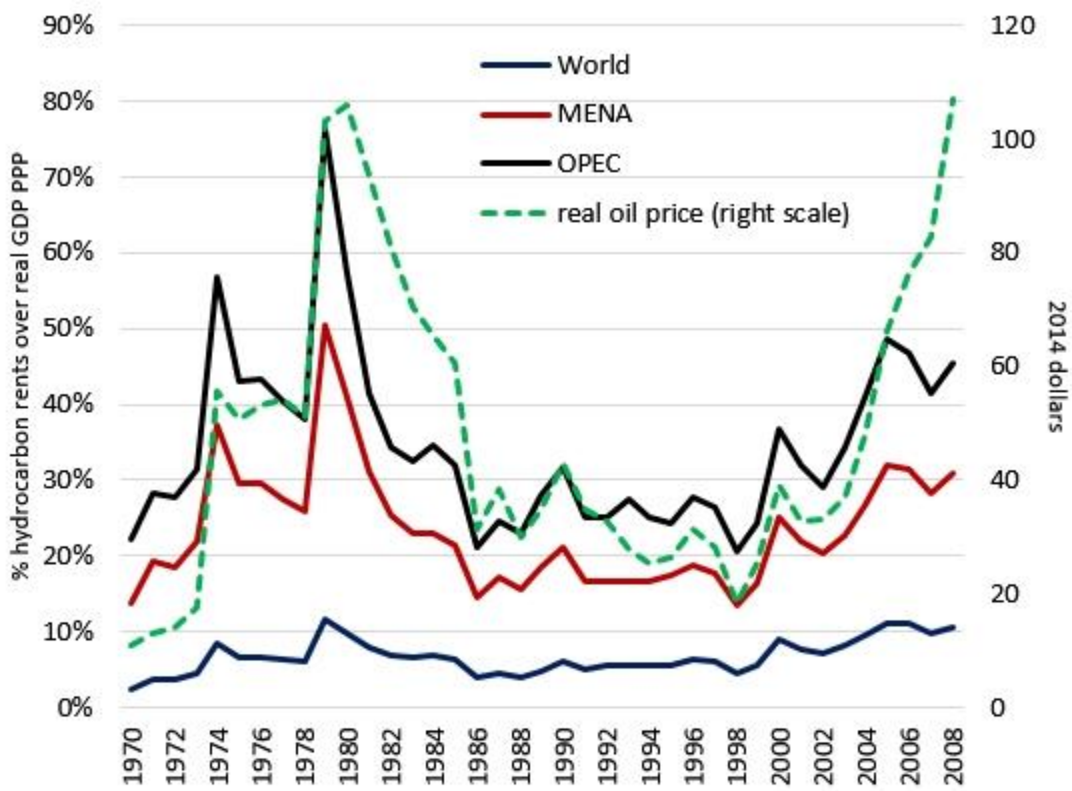
> (a) smooth out the leverage of rents over a longer period, assuaging the effects of potential downturns while at the same time removing from political leaders and elites alike some potential "rent leverage" in boom times. The effect is stronger during downturns, given that in times of exceptionally low international prices or of any domestically-induced crash in hydrocarbon-related revenues, the accumulated financial resources can be called upon in order to avoid the excessive or too rapid enactment of fiscal austerity measures;
>
> (b) use financial economies of scale and invest the revenues in foreign assets or financial products, in order to extract a higher level of total rents from the same level of hydrocarbon rents over a longer period of time. This partially counteracts the fact that natural resources deplete over time, helping the country extract more revenue for each dollar of resource sold over the long run.

Considering all that, it is important to highlight the fact that, even after my best efforts, the estimated amounts of yearly natural resource rents are still a rough approximation of actual rents. This is again one of the reasons why empirical models relying upon fixed effects, while potentially gaining in accuracy by excluding the effect of time-invariant omitted variables, suffer from a fundamental liability (see section 6.3). The fact that our numbers remain very rough estimates at the country-year level suggests that, for as much as we would prefer to have sufficiently reliable data available, this is still not the case in today's comparative politics field. Using yearly changes from the overall country sample mean risks biasing our results because so much noise is embedded in our data when we decide to zoom in at the within-country level (Jerven 2013, Jerven 2015).

5.5. Political leverage of rents over time and space

Hydrocarbon rents per capita as a share of GDP PPP per capita have fluctuated a lot over the last four decades. In order to show how they have evolved over time, I use my multiply-imputed dataset and employ a special Stata command (*mim*) to recover descriptive statistics out of an imputed dataset.

Figure 5.3 – Evolution of hydrocarbon rents over time

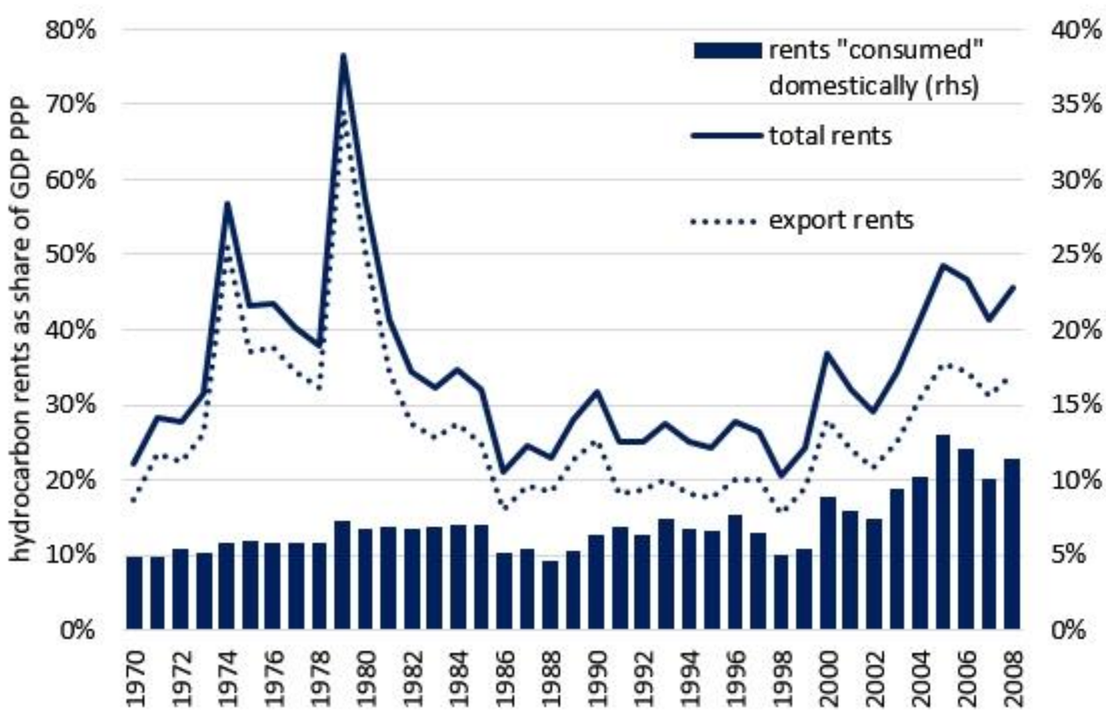


Data: WB, EIA, modified and imputed; BP *Statistical Review of World Energy 2015*

As Figure 5.3 shows, the share of hydrocarbon rents has mainly varied according to changes in the real oil price (as natural gas contracts have remained overwhelmingly oil-indexed throughout my sample period). The figure shows that average hydrocarbon rents for all 154 countries in my sample rarely exceeded 10%. In fact, they made up at most 1% of the share of GDP PPP per capita in over 35% of my sample. At the same time, the upper tail of the distribution in hydrocarbon rents is long, and it is mostly represented by a small number of countries that display a high level of "rentierism".

As shown in the figure, these countries are mainly found in the Middle East and North Africa region. As expected, moreover, OPEC countries (whether within or outside the MENA region) reach the highest levels of hydrocarbon rentierism than any other country grouping in the sample. One interesting thing to note is that, even for OPEC countries as a group, the level of rentierism has tended to flatten out since the mid-Eighties, resulting in a plateau at around 30% for two decades between 1985 and 2004. It then rose with the oil price throughout the end of my sample years, but never reached the highest peaks of the Seventies and early Eighties.

Figure 5.4 – Average OPEC hydrocarbon rents as share of GDP PPP



Data: WB, EIA, modified and imputed

This may be due to two related evolutions. First, even OPEC countries have tended to grow somewhat more diversified over time. While these countries have remained dependent on the export of their high-value hydrocarbon resources on average for over 90% of their exports, the composition of their domestic economy has changed, mainly thanks to oil-driven industrialization, nontradables such as housing and, in the last decades, financial services (IMF 2015).

Second, oil exporting countries have experienced both a rise in their country's overall GDP per capita levels, and a rise in population, which means that the same absolute value of rent accruing to political leaders now must be spread over a larger population, and that each dollar is now worth less compared to their citizens' living standards.

A third pressure determined by a growing number of more affluent domestic citizens on oil exporting countries' governments and upon the political leverage from hydrocarbon rents is best exemplified by Figure 5.4.

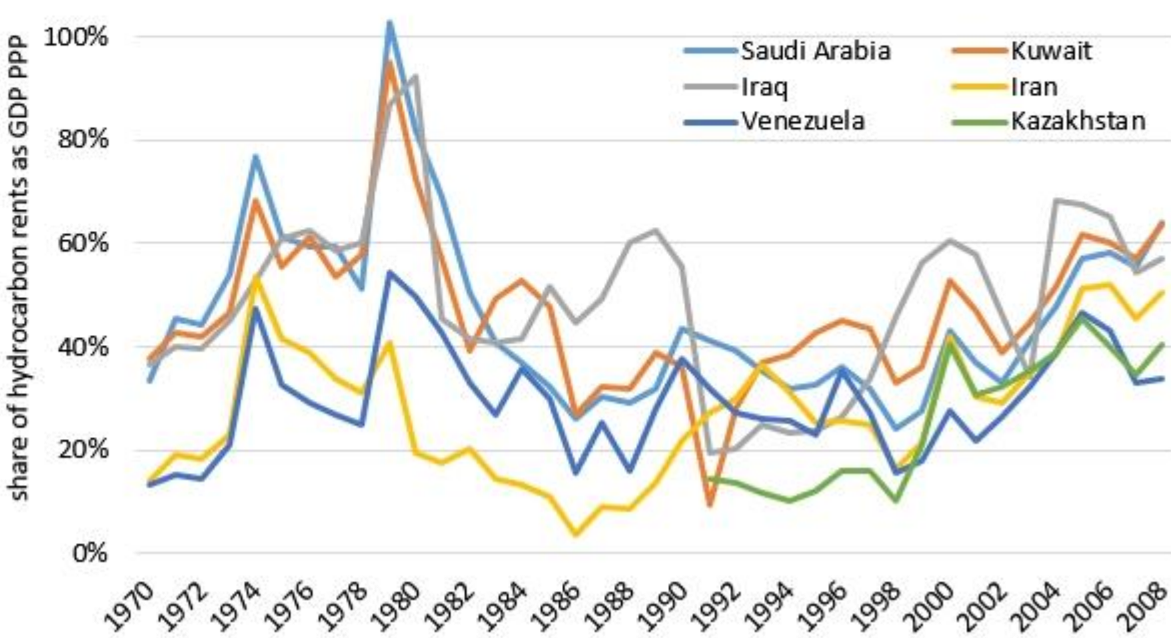
As the figure shows, over time an increasingly larger share of rents in OPEC countries has been "consumed" domestically. The share of domestically-consumed hydrocarbon rents rose from about 5% in 1970 to over 10% from 2004 on, and the trend has gotten stronger during my sample's final decade.

Generally, this means that an increasingly larger share of rents does not directly accrue to political leaders, but is used for domestic energy consumption. Moreover, domestic consumption is mostly subsidised, so that the difference between export rents and total rents may not be totally recovered through domestic use, but may even constitute a net loss to the public finances. It is hard to quantify how much of the hydrocarbon revenues is actually lost to subsidies and how much is simply direct consumption.

A second, indirect effect of subsidies is they incentivize domestic consumption even more than the normally "depressed" price of a domestically produced resource. Therefore, subsidies have the perverse effect of eroding public revenues over time, especially as demographic pressures increase, and tend to keep the system from reaching the optimal balancing point that would maximise both revenues (from the international sale of hydrocarbons) and the standard of living of the country's citizens (Coady et al. 2015).

Moving on, Figure 5.5 shows the evolution of hydrocarbon rents for a sample of oil exporting countries over time. The figure shows both common trends and country-specific shocks. The 1973 and 1979 oil price spikes are very present, although they appear to have a different effect over different countries. Also, both the general decreasing trend in oil rents of the Eighties, the plateau of the Nineties and the growth of the 2000s are plainly visible. However, the decreasing trend affecting post-revolutionary Iran during the Eighties (the time of the Iran-Iraq war) shows the problems faced by the country during that decade; these in turn almost tunred Iran into a "non-dependent country" in 1986. By the early 1990s, though, Teheran's oil rents were again picking up, bringing Iran in the middle of the represented bunch by the end of that decade.

Figure 5.5 – Political leverage from export rents, high-leverage countries

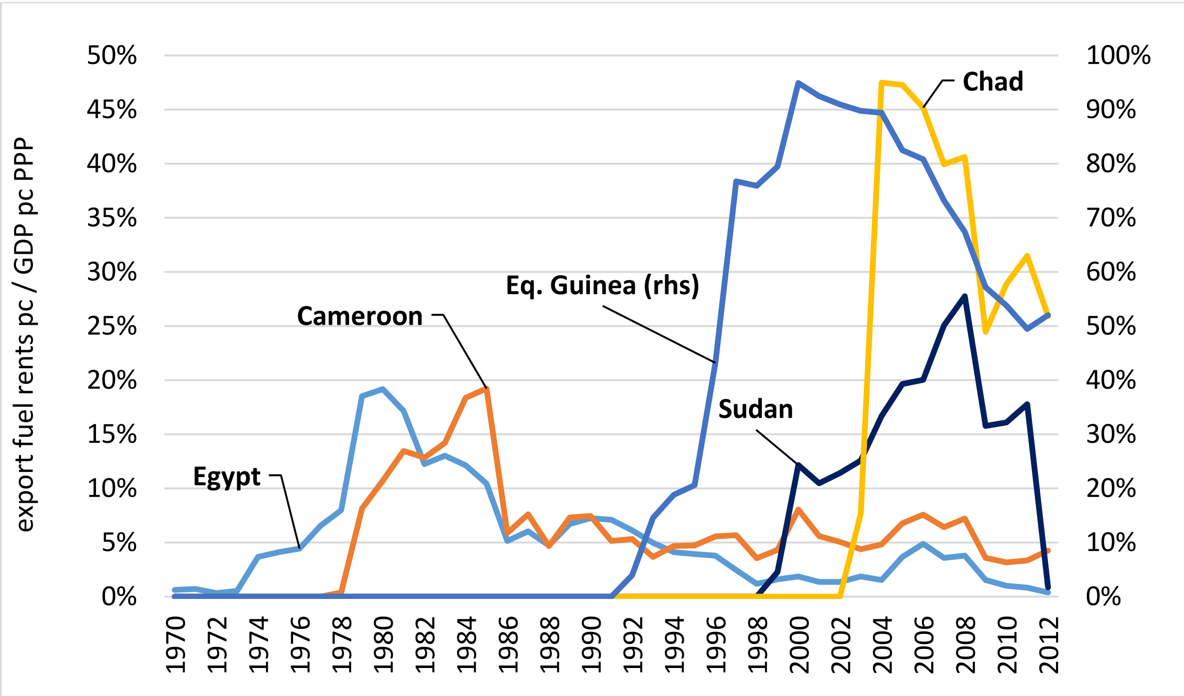


Data: WB, EIA, modified and imputed

From the figure, Iraq also stands out, with the effects of the 1991 Gulf War and the sanctions period being plainly visible, as well as those of the 2003 US-led invasion. Finally, Chávez's

Venezuela experiences a stronger shock than others in 2007, and a smaller recovery one year later, setting the country upon a decreasing trend (not visible because out of sample).

Figure 5.6 – Political leverage from export rents, African countries



Data: WB, EIA, modified and imputed

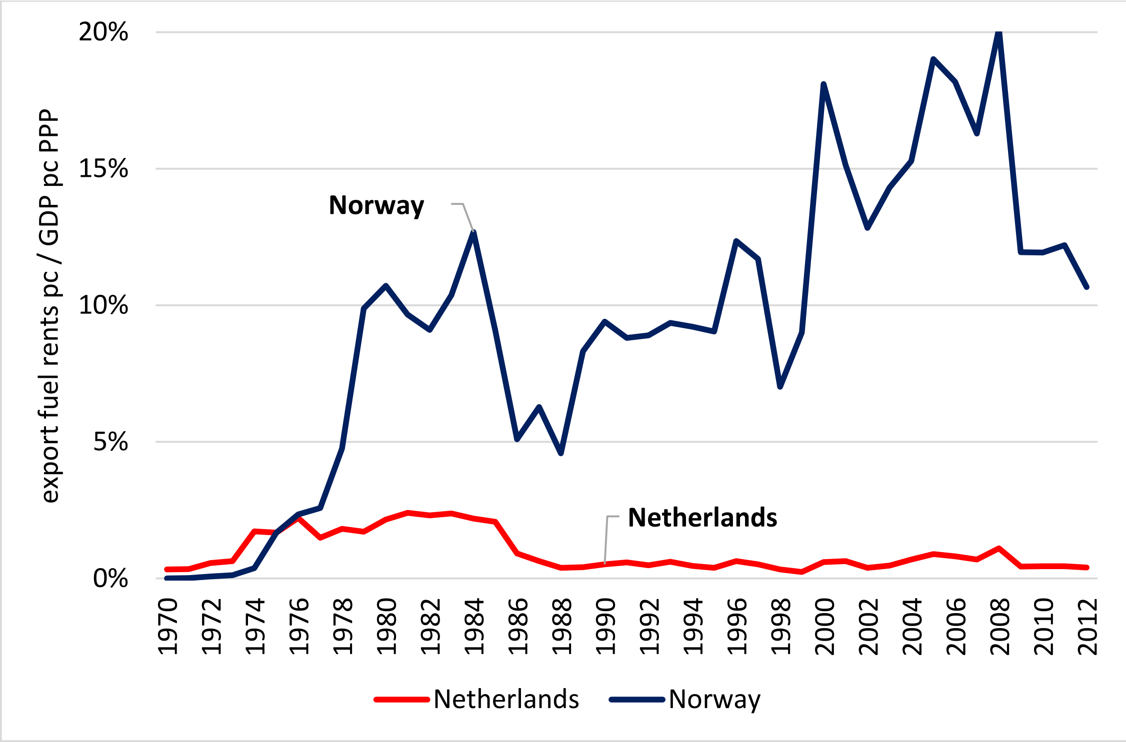
One can also focus on specific patterns of "rentierism" over time, as the extraction and sale of hydrocarbon resources can affect different countries differently, especially if compared to Figure 5.5, which shows countries that remained rentier states throughout my sample period.

Figure 5.6 shows how some countries experienced much shorter, boom-bust cycles. These bouts of "rentierism" appeared at different moments in time, with Egypt and Cameroon becoming temporarily dependent on hydrocarbon resources respectively by the late-Seventies and early-Eighties, while others, like Equatorial Guinea, Chad, and even Sudan, experienced much more recent hydrocarbon booms.

Also, some of these countries have seen the hydrocarbon bubble deflate quite rapidly, despite maintaining a certain level of rentierism up to these days (2012 for Sudan was a one-off zero rents year due to South Sudan's independence and the ensuing civil/international war compounding the oil crisis). Other countries, while possibly already having passed their initial hydrocarbon rents peak, remain heavility dependent on the resource up to this day. Extending the analysis to out-of-sample years, Chad appears to have experienced a very short boom period, but in 2014 was still as dependent to hydrocarbon exports as Egypt and Cameroon were at the peak of their boom years (my measure of political leverage from export rents records a figure a little over 20% for the country in 2014). Meanwhile, Equatorial Guinea is already past its peak, but can still count upon a level of political leverage from export rents which amount to over half her citizens' GDP per capita PPP.

Finally, Figure 5.7 shows a peculiar consequence of the way in which I measure the political leverage from hydrocarbon rents. Historically, the literature on the (economic) resource curse was based upon the observation of the experience of nationalizations across Gulf Arab Countries (Beblawi and Luciani 1987), but also on a peculiar case of an economically advanced country experiencing a natural gas boom: the Netherlands during the Seventies and Eighties. Thus, the popularization of the theory and empirics of the economic resource curse as "the Dutch disease" (Davis 1995). However, if one tries to measure the political leverage from hydrocarbon rents across this period, he cannot but acknowledge that, even at its peak, rents per capita accruing to the Dutch government via hydrocarbons never exceeded 3% of GDP per capita PPP.

Figure 5.7 – Political leverage from export rents, Norway and the Netherlands



Data: WB, EIA, modified and imputed

Even compared to Norway, a country whose rents still remained at the lower range of the political-leverage spectrum of hydrocarbon rents (compare Norway's maximum oil rents to GDP per capita PPP of 20% to Figure 5.5, where the average country stays above the 40% mark for most of the sample period), the Netherlands appear as a rare bird. It is peculiar that such an extensive literature decided to focus on a case which, though being key in showing that resource dependence (as low as it could be) could crowd out industrial development in other sectors, import high levels of inflation, and cause recessions or lower potential economic growth in the longer run, is ultimately not an outstanding example of a country experiencing high levels of rent dependence.

5.6. Conclusion

This chapter shows that the picture of hydrocarbon dependence is complex, varying wildly both over time and within countries. This is also the reason why it is important to develop measures that can gauge, to the best of our means and knowledge, the political leverage that an elite/government may exert upon its citizens through rents accruing to the state's coffers. Throughout the Chapter, I propose two novel ways that in my opinion can best capture the leverage bestowed by hydrocarbon rents to a country's governing elite: the "political leverage from rents" variable and the "political leverage from export rents" variable.

I find this measure to be highly correlated with the international price of energy. However, country-specific trends can set in and affect the overall trend levels both as one-time shocks and in the form of longer-term dynamics.

I advise against inferring too much from year-on-year changes of my variables, especially given the fact that, as extensively discussed above, it is hard to pinpoint the exact extent of yearly rents, and there are many unobservable factors that may complicate the picture. A complex mix of business- and country-specific factors, such as the choice of contractual agreements with international oil companies or each country's leeway in force better or worse conditions, and its ability to smooth rents over time through sovereign wealth funds and conservation policies (not to mention the level of corruption or outright theft), can affect the share of rents that remains withing the government of a country or is brought abroad.

The precise figure of rents accruing to the political governing elite can hardly be estimated precisely, and therefore we should always take each data point as a best estimate of the true values, not as highly accurate data.

At the same time, to the best of my knowledge, this attempt at estimating rents from multiple sources, multiply imputing missing data by employing a very large dataset with dozens of potential covariates, and the proposal of two new pretty simple measures to estimate the political leverage bestowed upon each country's leader by hydrocarbon rents are original efforts that may make a significant contribution to the literature of the political resource curse.

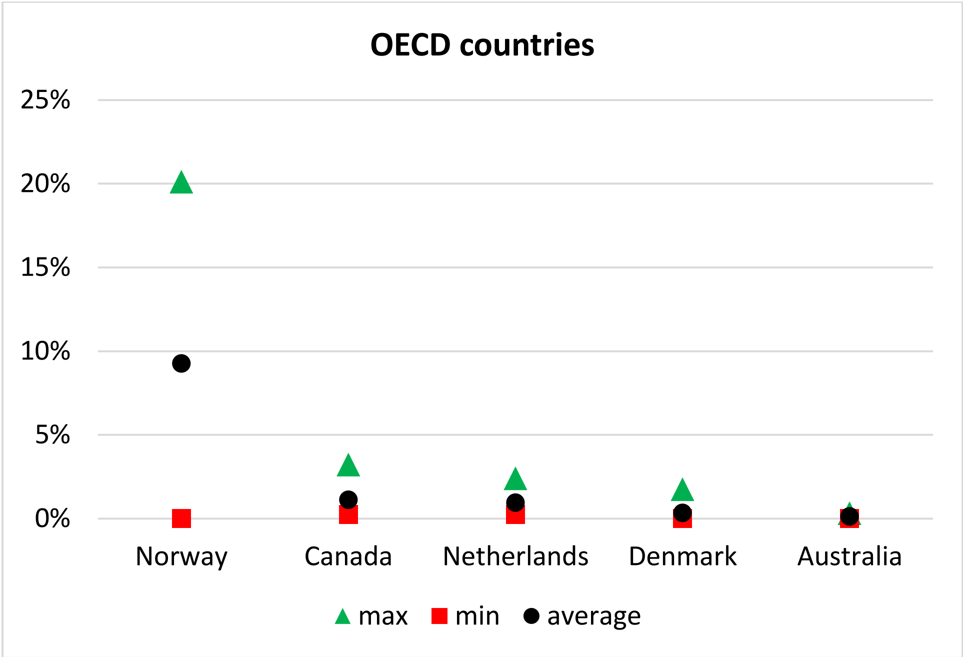
Appendix – Political Leverage from Export Rents in Major Rentier States

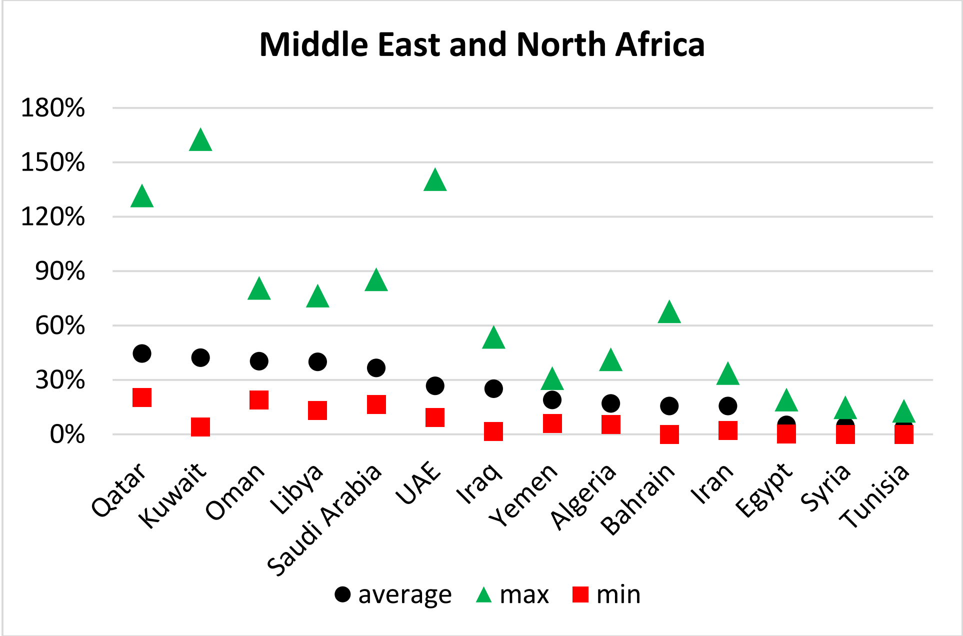
In this brief appendix, I present a number of graphs showing the extent of average political rents accruing to every country throughout the 1970-2008 sample period, together with intra-country variability. The purpose is to show the extent to which rents tend to vary along the country and time dimension.

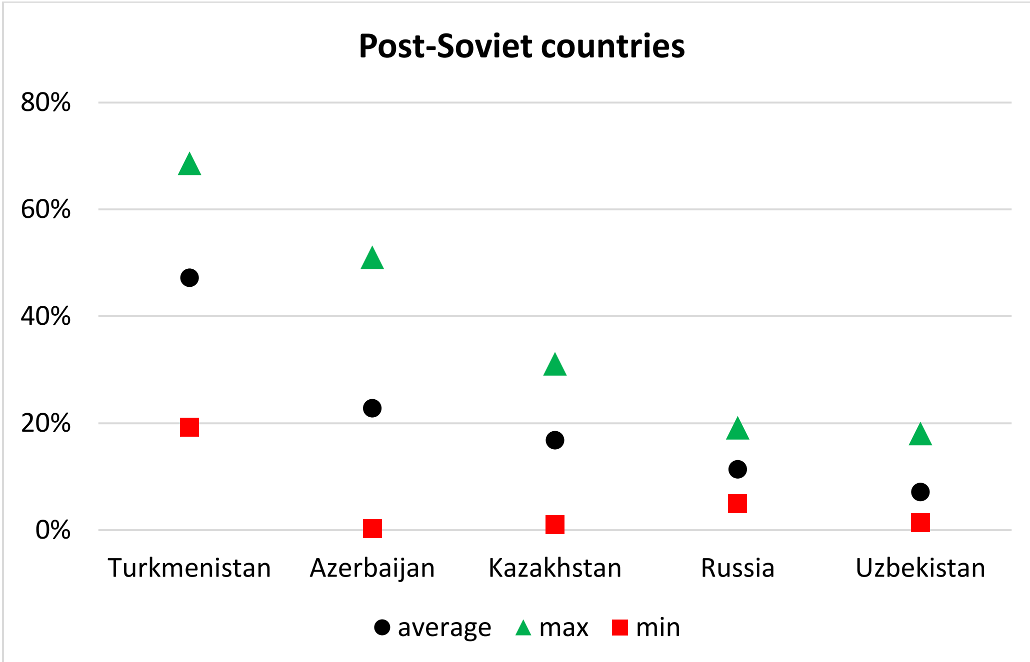
Of note, for example, is the small political leverage that hydrocarbon rents bestow upon advanced countries, which here I grouped under the OECD label. Apart from Norway, which reaches an average 10% value throughout the period and a peak of 20% for one year, the second country (and big oil and natural gas exporter), Canada, does not reach even 5% of each citizen's GDP per capita PPP at its peak, and overs at an average below 2% throughout the sample period. Even Mexico, a middle income country with a well-developed oil sector, earns less hydrocarbon revenues from exports that are equivalent to less than 10% of its GDP per capita PPP all the time between 1970 and 2008.

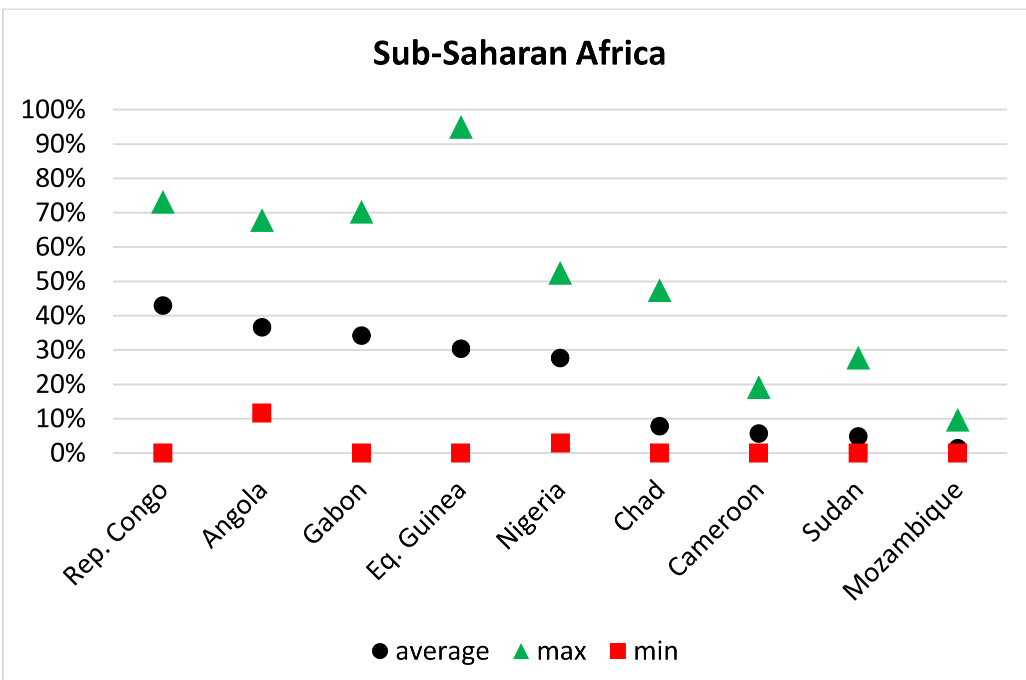
At the opposite end of the spectrum, MENA countries display very high potential political leverage from hydrocarbon exports, and some of them display considerable fluctuations over time, with years in which rents even surpass 100% of each citizen's GDP PPP. Some African countries also display high potential political leverage, together with some Post-Soviet countries (Russia being at the lower end there), while only a few countries in South and Central America (Venezuela, and Trinidad and Tobago) display a relatively high level of political leverage from export rents.

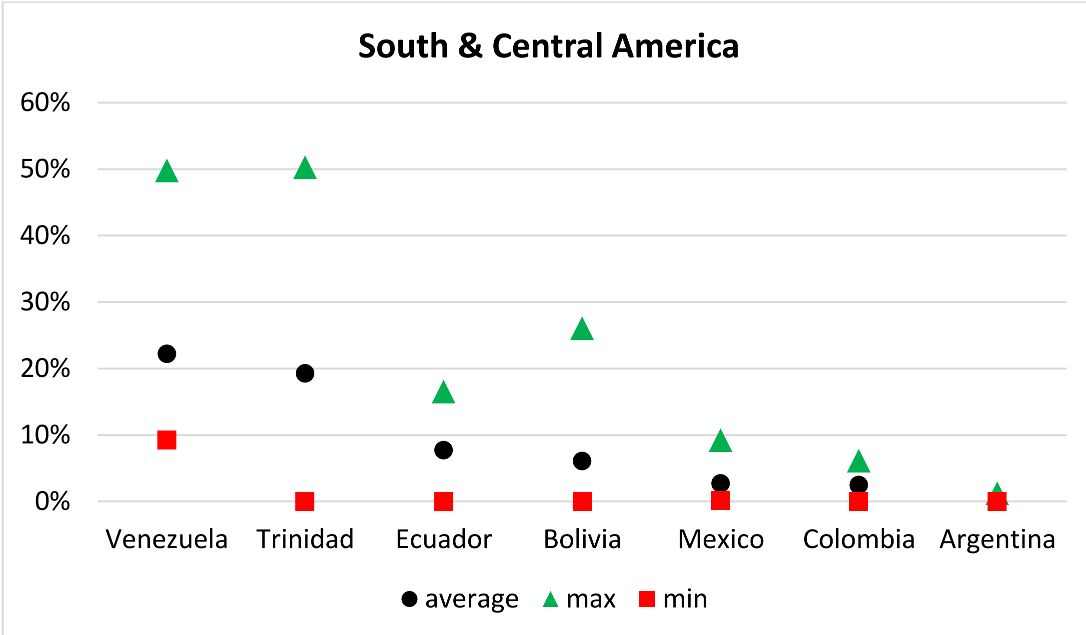
Figure 5.8 – Political leverage from export rents, selected countries (1970-2008)






Post-Soviet countries
80%
60%
40%
20%
0%
Turkmenistan
Azerbaijan
Kazakhstan
Russia
Uzbekistan
average
max
min


Sub-Saharan Africa
100%
90%
80%
70%
60%
50%
40%
30%
20%
10%
0%
Rep. Congo
Angola
Gabon
Eq. Guinea
Nigeria
Chad
Cameroon
Sudan
Mozambique
average
max
min


South & Central America
60%
50%
40%
30%
20%
10%
0%
Venezuela
Trinidad
Ecuador
Bolivia
Mexico
Colombia
Argentina
average
max
min

Chapter 6. Testing Regime Choice and Change

Democratization, Liberalization, and Their Correlates

6.1. Introduction

In this chapter, I make a number of contributions to the literature that empirically tests the correlates of political regime choice and stability. My main aim is to test the hypotheses on the correlates of liberalization that I derived from my theoretical model in Chapter 2, by making full use of the operationalisations of my main variables of interest in Chapters 3-5. Most importantly, I am interested in testing whether the traditional correlates of democratization are robust, and whether these same correlates can be extended to broader liberalization trends. Meanwhile, I also aim at testing several other theories and hypotheses, in particular those related to the causes of regime "failure".

Throughout the analysis, I employ robustness checks to make sure that my findings are not dependent on a wide range of different operationalisations of my main variables, and to other estimation strategies. Different models and techniques also enhance the likelihood that I will be able to study how findings vary as research questions slightly change. In particular, I am interested in studying whether the correlates of political regime type (across different levels of liberalization) and the correlates of regime failure are the same or different, and how they may interact with one another. As will be shown in Section 6.3, my findings over the extended modernization hypothesis (for liberalization rather than democratization trends) are particularly crucial in that higher GDP per capita may increase the likelihood that actors within a polity choose more liberal regime levels, but is also correlated with higher regime stability overall (possibly through repression and co-optation mechanisms), so that the outcome of per capita economic growth on political regime choice will be the result of a complex fight between two opposing forces generated by a similar underlying process of increasing well-being *and* increasing (but possibly perverse) state capacity.

My empirical analysis wholly relies upon multiple imputation techniques. Multiple imputation strategies are not novel, but they are still used very rarely. This is unfortunate, because multiple imputation allows researchers to make the best use possible of the information contained within the collected data, even when some important variables are plighted by the problem of a relatively large share of missing data. Multiple imputation does away with some of the worst scenarios for estimation strategies (listwise deletion, especially in panel data settings, or dropping variables with a high share of missingness from the model) without resorting to possibly biased and massively cumbersome human-based imputation. This technique is explained in detail in section 6.2.2, along with the way in which I apply it to my original dataset.

Moving to the proper empirical testing, I regard the models described in section 6.3.1 as especially relevant to the hypotheses I derived from the formal model. This is so because these models rely upon an ordinal, tripartite (or occasionally quadripartite, as in the case of the *regime4* variable) categorization of political regimes, dividing the political regime space into democracies, on the one hand, and autocracies at different levels of "openness" on the other. The findings will confirm all of the hypotheses I advance in section 2.6. Possibly the most important finding is that political regime choice across the tripartite (or quadripartite) liberalization spectrum is even more strongly correlated with classical correlates of democratization than levels of democracy (see models in section 6.3.2). Moreover, by using the latest version of a measure of inequality that can be considered the state of the art in that literature (and multiple imputation that help filling some gaps), my empirical tests allow me to adjudicate between two competing theories' implications over how inequality should correlate with more liberal regimes. Namely, I find support for theories that imply that regimes that are more liberal should be found at average levels of inequality, and therefore reject theories that postulate that more liberal regimes should be found at low inequality levels.

In section 6.4, I shift the research question and ask whether the same or different factors appear to be at work in determining political regimes' failure/transitions, and how so. This is relevant because, as confirmed several times throughout my empirical analysis, once the political regime settles upon a particular political path (i.e. a liberalization level), its institutional setting tends to be highly persistent. Therefore, while particular autocrat-citizen dynamics and their relationship, as well as socio-economic correlates and other structural conditions, might be relevant in shaping a political regime's current and future path, it might be that other forces are at work in determining specifically *when* or *how often* a certain political community may experience regime change. In the following analysis.

Asking whether political regime change is correlated to a certain set of political, socio-economic, historical or geographic factors allows me to expand my sample to a sufficient number of cases of regime change. At the same time, the mechanisms underlying the direction of regime change might vary according to whether one is interested in studying the causes of a polity to "liberalise" (from a closed autocracy to a democracy, or simply to a more open form of autocracy), or whether one chooses to investigate the reasons why a polity might run into new impediments and experience democratic retrenchment. This is why in the final sections of this Chapter I will try to shed important, while still preliminary, light upon this question.

6.2. Working with multiple imputations

6.2.1. Why multiple imputations?

Missing data is a diffuse problem in the social sciences. Many interesting or potential correlates to our dependent variables might be available only inconsistently, or be outright absent, for a number of reasons, ranging from research design to uncontrollable events to chance. When we deal with data at the country level, as I do here, data may not have been reported to the collection agency, and the problem might have arisen at multiple levels: from the absence of reliable statistical collection agencies upon a country's territory, coupled with the impossibility or unavailability of international organizations/groups that take up that role; to outright violence that might totally preclude data collection over a single or multiple years. Sometimes, generally with slow-moving variables like demographic ones (total population, fertility, life expectancy, etc.), these numbers may be estimated, or even simply interpolated. Think, however, about the evolution of Rwanda's total population: having settled upon an increasing trend for 30 years, it went from 2.8 to 7.2 million people between 1960 and 1989. In 2001, Rwandan population had reached 8.8 million people. Yet, between 1990 and 1995 the country's population lost about 1.6 million people because of the Rwandan genocide and the conflict in general, as many died and others were displaced in other countries. Simple interpolation would be a disaster in cases like these: and it is just similar cases that increase the likelihood that data will be missing.

The problem gets even more serious in a multivariate, panel-data setting. This is due to the fact, while simple interpolation can be unreliable and can hugely bias estimation results, scholars have often resorted to the standard response to missingness, which is listwise deletion.

This implies that every time a variable that one believes should appear in the true model is missing an observation, the whole country-year row will be omitted during estimation. The more variables one adds to the model, the higher the risk of excluding a huge chunk of the dataset. As the number of covariates increases, the risk of biasing our analysis increases even when just a single variable contains a small amount of missingness, as the combination of small amounts of missingness may lead to a large fraction of the observations being omitted from subsequent analysis (or to having to drop some covariates, this way leading to omitted variable bias).

Even worse, whenever one is interested in investigating time dynamics, or thinks that the model should include at least one lag of a regressor, the impact of missingness from that regressor increases by a factor equivalent to the number of lags that are added to the model.

At the same time, whenever there is a reason for an observation to be missing, omitting it from the model or trying to impute it through any simple method like interpolation risk biasing our results. In the comparative politics field, some variables may be missing because of the occurrence of international wars, civil wars, or social disturbances of any kind: if

missingness is correlated to such social phenomena, and we do not take into account during the analysis, it is most likely that our results will be biased in some non-measurable way.
Luckily, in the last decades statisticians have developed a number of methods to analyse datasets with missingness without resorting to traditional and problematic solutions. Specifically, multiple imputation was first proposed by Rubin (1976) to treat missingness in a public use survey data setting.

6.2.2. Methods of multiple imputations

Here I introduce some notation to formalize the multiple imputation procedure, and go into some detail as to how this technique allows us to recover best estimates and levels of uncertainty around best estimates for our missing data.
At its most basic, multiple imputation techniques consist in three-step procedures. In the first, the "imputation step", the researcher creates plausible values for missing observations, which also incorporate the uncertainty around these plausible values. To do so, one has to employ regression models over the whole dataset and use a set of statistical techniques to estimate each missing value and the error around it, making full use of the whole information contained within the original dataset. In order to include uncertainty around plausible estimates, the procedure requires that the same estimation technique is used more than once over the original dataset, creating multiple "complete" datasets. For a general rule of thumb, users of multiple imputation techniques generate $5 \leq M \leq 10$ complete datasets, depending on the circumstances (as explained later, during the estimation step one can check whether the M-complete datasets are enough or more are needed.
Once these complete datasets have been created, the "estimation" step is to analyse them using ordinary, complete-data methods such as regression models. Finally, the "combination" step consists in taking the results of each complete-data procedure and to combine them into a unique result that can account for the variability in the results, given the variability in the imputed data within each complete-dataset.

As stated, then, multiple imputations requires estimating a preliminary, "multiple imputation model" in order to assign plausible values to missing observations and to recover the uncertainty around these plausible values. Therefore, it is important that this initial model is well specified in order to avoid that the following analysis suffers from the bias introduced during this step. Note however that, as compared with other approaches such as interpolation or listwise deletion, multiple imputation techniques perform way better: even with a highly unspecified multiple imputation model, these techniques are almost always to be preferred to other methods for dealing with missing data (McIsaak and Cook 2014).

Going back to notation, suppose for simplicity that we are operating in a cross-sectional setting, and we are interested in recovering the coefficients $\boldsymbol{\beta}$ and other distributional parameters from a model that relates our response variable of interest to a set of covariates, $f(Y_i|\boldsymbol{X}_i, \boldsymbol{\beta})$, where letters in bold express a set of observations and coefficients in matrix form. Suppressing the i indicator, suppose that our covariates have missing observations for some subjects i. For each subject, Denote $\boldsymbol{X}^{obs}$ that set of observations that are observed, and $\boldsymbol{X}^{mis}$ those that are missing. Also call $\boldsymbol{M}$ a set of dummy variables that, for each $\boldsymbol{X}$, take the value 1 of the observation is observed, and 0 if it is missing, and suppose that the value of $\boldsymbol{M}$ is conditional on a set of parameters $\boldsymbol{\varphi}$. In plain words, this means that we are supposing that missing observations are missing for a reason, and that there exist some parameters that predict when to expect to find missing observations in our dataset.

When missingness is due to a process of missing completely at random (MCAR), we have:

$$P(\boldsymbol{M}|Y, \boldsymbol{X}) = P\left(\boldsymbol{M}\middle|Y, \boldsymbol{X}^{obs}, \boldsymbol{X}^{mis}\right) = P(\boldsymbol{M}|\boldsymbol{\varphi})$$

where $\boldsymbol{\varphi}$ and $\boldsymbol{\beta}$ are assumed to be different. This entails that this kind of missingness is not related in any way to our observed covariates, and also that it does not influence them in any way (i.e. that missingness is not related even to unobserved covariates, just to chance).

Given the implausibility of such an assumption, multiple imputations are motivated by an assumption of missing at random (MAR), where we suppose that we can identify the cause(s) for missingness and that we can make an attempt at modelling them. The model thus becomes:

$$P(\boldsymbol{M}|Y,\boldsymbol{X}) = P(\boldsymbol{M}|Y,\boldsymbol{X}^{obs},\boldsymbol{\varphi})$$

MAR assumes that missingness only depends on observed quantities (both outcomes and covariates) and that this completely determines the likelihood of observing a missing observation. Although the MAR assumption is certainly superior to MCAR, this does not mean that it is safe: the potential for omitted variable bias is reduced, but can never be completely eliminated. In fact, given that we cannot hope to capture every conceivable mechanism influencing our outcome variable and to model it properly, choosing the correct functional forms for each covariate, we need to commit to a portion of bias. In any case, studies show that by employing an “inclusive strategy” in which we include in the imputation model a much higher number of potential covariates and causes of missingness, instead of just our variables of interest and a few covariates, bias tends to be reduced by a great deal (Collins et al. 2001). Also, the inclusion of the information about our outcomes of interest (although, as will be shown, they will not be subject of imputation but only serve to impute missing data for covariates) can greatly improve the imputation step of our procedure (Moons et al. 2006).

Going back to our imputation model with the MAR assumption, we can reduce notation further down to:

$$P(M|D) = p(M|D^{obs})$$

Where M is a missingness matrix, D is data (including both X and Y), and D^{obs} are observations that are non-missing. As stated above, multiple imputation assumes that the complete data follows a multivariate normal distribution: $\sim\mathcal{N}_k(\mu,\Sigma)$, which is also why I will transform categorical, limited dependent and other data types in order to make this assumption plausible. In any case, there is evidence that assuming multivariate normal distributions works well enough in most instances (Schafer 1997). As per Honaker and King 2010, the estimation strategy to impute plausible values in place of missing data in my initial dataset is based on the following reasoning. First, the likelihood of my observed data, and assuming that missingness follows a MAR data generating process, we can write:

$$p(D^{obs}, M|\theta) = p(M|D^{obs})p(D^{obs}|\theta)$$

After a series of manipulations, Honaker and King 2010 arrive at the identification of the posterior for the distribution of the parameters that we want to estimate, which are included in θ (assuming a flat prior):

$$p(\theta|D^{obs}) \propto p(D^{obs}|\theta)dD^{mis}$$

Once the posterior has been determined, they still need to find a way to take random draws of μ and Σ from these posterior densities so as to recover the estimation uncertainty around imputations. They solve the problem by devising an algorithm that further improves on the expectation maximization importance sampling (EMis; Dempster et al. 1977), relying on a bootstrapping technique that reduces computational burden by several orders of magnitude.
The multiple imputation process applied to my dataset is described in the section below. Meanwhile, here I focus on the way to combine the analysis done on multiply-imputed datasets into a single estimate for the coefficients, and how to correct the recovered standard errors of the coefficients by accounting for the larger uncertainty contained within observations that have been imputed.
Recovering the multiple-imputation coefficient estimates is straightforward: it is enough to average estimated parameters over all m-multiply imputed datasets, $\beta_j(j = 1, \ldots, m)$:

$$\bar{\beta} = \frac{1}{m}\sum_{j=1}^{m} \beta_j$$

The variance of the coefficient estimate is the sum of the *within* variance (the average of the m estimated variances) plus the sample variance in the point estimates *across* all datasets. The latter must be multiplied by a factor that corrects for the bias given that $m < \infty$. So, the estimated standard error will be:

$$SE(q) = \sqrt{\frac{1}{m}\sum_{j=1}^{m} SE(\beta_j)^2 + \left(1 + \frac{1}{m}\right)\sum_{j=1}^{m} \frac{(q_j - \bar{q})^2}{m-1}}$$

6.2.3. Multiple imputations in practice: from a single dataset to multiply-imputed datasets

For the imputation step I rely upon Amelia, an R package developed by James Honaker, Gary King and Matthew Blackwell. I build a dataset including all my variables of interest, plus a great number of variables that will not enter the final analysis but will help me decrease the likelihood that the data generating process around missingness is not MAR (see 6.2.2). Namely, this step includes 91 variables: together with all my outcome variables and all my covariates of interest, I add a number of variables accounting for demographic, economic, social or political factors such as: the occurrence of civil, international or ethnic violence or wars; the occurrence of coups d'état; many possible types of regime or government transitions and measures of levels of democracy; urbanization rate, population density, fertility rate, age dependency ratio, share of the working age population, migration rate; percent of population employed in agriculture, industry, and services; any possible measure of GDP and GDP per capita, GDP growth, poverty rate and unemployment rate; any possible measure of natural resource rents; imports or exports of goods; military expenditures; land area and the extent of rough terrain.

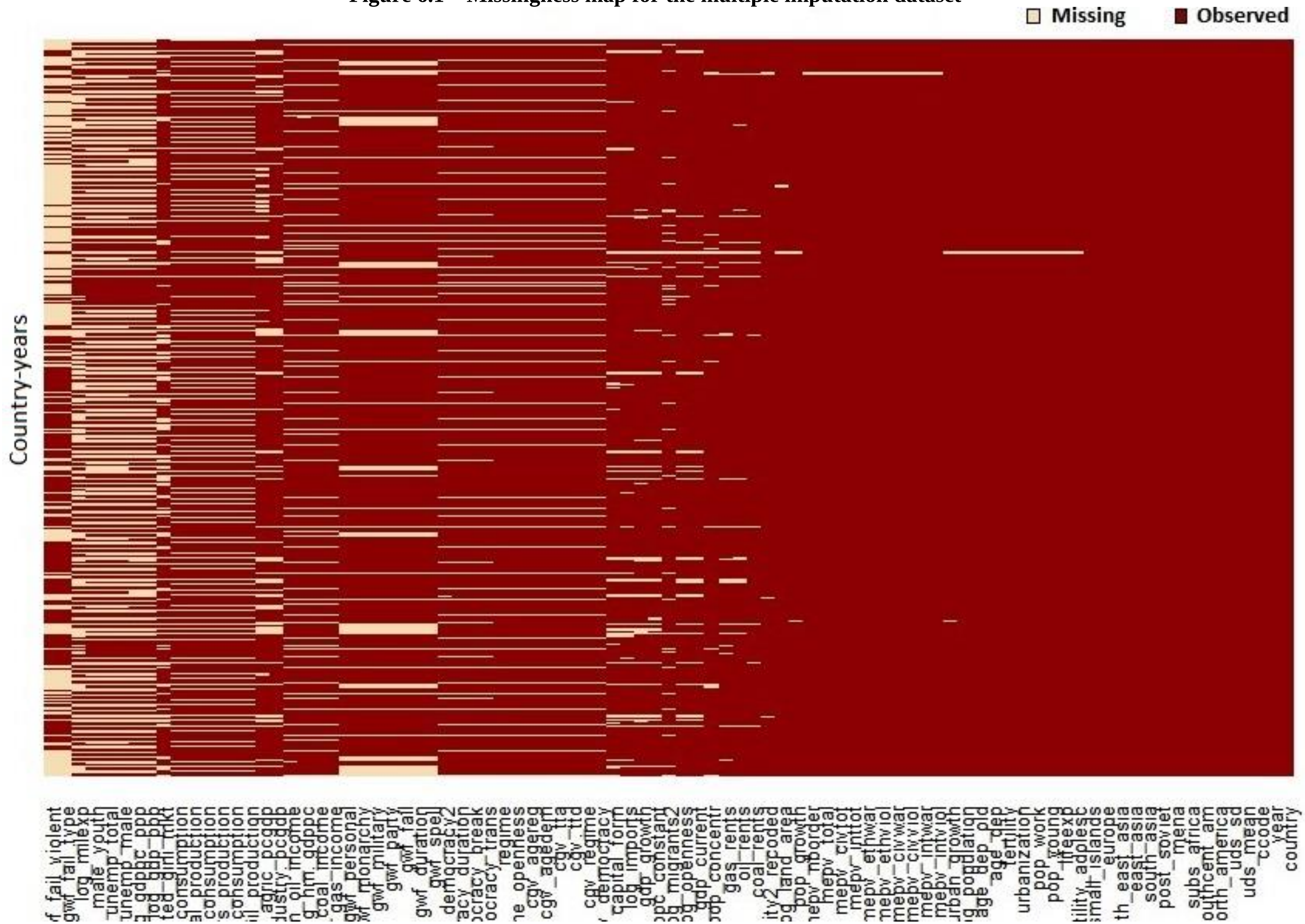
Figure 6.1 shows the so-called missingness map, which is a graphical representation of the missingness within the dataset. Variables are ordered from left to right, from the one that misses most data to the least missing. Overall, missingness is not too relevant a factor apart for some specific variables: at the same time, the map clearly shows that missingness can be spotty, meaning that missingness does not tend to be consistent throughout variables, but tends to vary, leading to at least some data missing in most of my country-year observations despite over 85% being present on average for every observation.

One complication of the multiple imputation step in my case is that I am using panel data: my analysis relies on 154 countries in the period 1970-2007. This requires me to specify a multiple imputation model that can account for correlations along the time and country dimension. First, I add a series of time polynomials to my covariates in order to account for general patterns within variables across time. Amelia lets me also include country dummies to account for potential fixed effects, and interacts my time polynomials in order to account for important country-specific time dynamics that might escape the overall dataset.

Finally, I also specify appropriate transformations for categorical, ordinal, and limited dependent variables before kick-starting the imputation process. Time polynomials and country fixed effects highly increase the accuracy of the imputation step, at the cost of a higher computational burden. In particular, the EMB algorithm – as fast and agile as it is – risks not converging, especially when some included variables contain a high degree of missingness, such as in my case. The time polynomials and country fixed effects add $3 * 154 - 1 = 461$ parameters that require estimation.

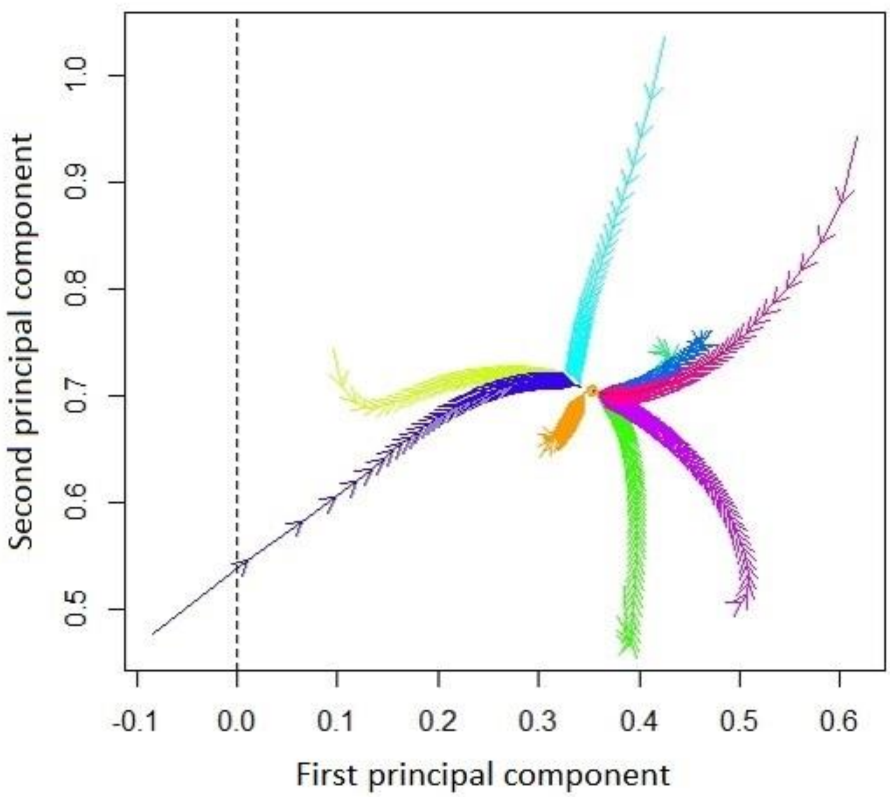
In such cases, Amelia's codebook recommends adding a ridge prior, which shrinks covariances between variables towards zero without changing their means. In short, this choice adds a percentage of observations to the dataset that have the same means and variances as the existing (observed) data but with zero covariances. This imposes a little more structure upon the data, although it tends to drag imputed estimates a little towards the observed averages of each variable.

Figure 6.1 – Missingness map for the multiple imputation dataset



The Amelia codebook suggests adding between 0.5% and 1% of observations to stabilise the dataset and ensure convergence, while stating that even 10% would be an “acceptable upper bound”. In order to keep this value as low as possible, we choose a 0.1% ridge prior. This ultimately ensures convergence (in my case, the imputation process takes around 30 hours to converge).

Figure 6.2 – Testing for convergence of overdispersed starting values



In order to make sure that the EMB algorithm properly converged (i.e. that the algorithm has managed to find a global maximum of the likelihood surface), and that starting values in the iterative process have had no effect upon the imputation results, I can use a visual diagnostic tool offered within Amelia. Namely, the program allows to run the EM chain from multiple starting values that are overdispersed from the estimated maximum, and then shows a graphical representation of how the algorithm behaved. Given that the likelihood moves in a high-dimensional space, only the first and second principal component are displayed in order to give a good rule of thumb for convergence. As shown in Figure 6.2, the algorithm does indeed appear to converge towards a single global maximum, irrespective of the starting point. I can therefore be confident that the multiple imputation procedure was well-behaved. After this process, I am left with 10 multiply-imputed datasets. Each of them is a complete dataset with no missing data, and I can use all of them in order to proceed with the rest of my analysis, having avoided to lose any information due to missingness and, hopefully, having minimized bias.

6.2.4. Inferring from multiple-imputation data and its limits

As a general introduction to the following sections, it is important to delve into how to read model output when using a multiply-imputed dataset, and to underline benefits and costs of a multiple imputation analysis at the estimation stage.

As explained in section 6.2.1, multiply-imputing a dataset is useful in order to avoid any listwise deletion while at the same time making full use of the information contained within a dataset. After multiple imputations, in my case, we are left with a set of 10 multiply-imputed datasets containing the same number of country-year observations as the original dataset (6,045), but with no missing values apart from the few cases in which I decided that some variables were not to be imputed. For example, variables that have not been imputed include democracy scores, regime openness scores and political violence scores, which are not liable to be imputed being the result of expert judgment and direct observation. These values are also most likely to be missing for a very specific reason, and not being missing at random, thereby risking to invalidate our multiple imputation analysis. Recall that in section 3.1 I recoded the *polity2* variable instead of letting multiple imputation do its “magic”.

For a rough estimate of the benefits from multiple imputation analysis, bbeing able to do away with listwise deletion in the context of my models, just consider that the inclusion of all variables in Model 1 (see Table 6.2 and section 6.3.1) and the employment of listwise deletion would have led to the use of just 2,339 country-years instead of the 5,101 I can rely upon via multiple imputation: a net loss of 54% of the observations included in the dataset. Also, some covariates would have automatically been dropped due to collinearity. Also, the disappearance of a lot of observations would almost forbid the modelling of time dynamics, rendering panel data modelling almost useless.

In the face of such huge benefits, one should not underestimate the fact that multiple imputations come at some considerable cost. One liability concerns the estimation process itself, while other regard post-estimation and prediction. By increasing the complexity of the underlying data structures, multiple imputation models require considerably more computational power and time in order to converge towards final estimates. Also, while I took considerable care during the multiple imputation step in order to best preserve the initial relationships between data, imputed values are never true values, and the estimated uncertainty around them is only valid if we can be reasonably confident that we accounted for dependencies within the original data and for the data generating process that generated missing values in the first place. Recall that, in order to best approximate my model to the true underlying DGP I modelled time dynamics by adding time polynomials, and included country fixed effects to account for the panel nature of my data.

In practice, as explained in section 6.2.2, given that the following estimated models come from multiply imputed data, I need to adjust coefficients and standard errors for the variability between imputations. This “between” component is a function of the variability in my dataset – from which the efficiency of multiple imputation depends upon. Thus, the precision of multiply-imputed estimates depends not only on sample size, but also on the number of imputations and on the structure inbuilt in the original dataset.

This implies that, even with a large sample size, important factors to the precision of coefficient estimates are both the fraction of missing information (the larger it is, the less precise our estimates will be) and the number of multiple imputations the model can rely upon (the smaller it is, the less precise the recovered estimates). As the number of imputation increases, however, the efficiency of multiple imputations decreases exponentially, so that we must calibrate M (the number of imputations) in order to maximize precision while preserving efficiency.

Below each of the multiply-imputed models, I report the "Average RVI" and "Largest FMI" values. "Average RVI" refers to the average relative variance. It reports the relative increase in variance of the estimates which can be attributed to missingness, averaged over all coefficients. The closer this value is to zero, the less effect missing data will have on the estimate's variance.

Note however that the average RVI is zero when one estimates a model with just the non-imputed dataset: in such a case we are not missing any information due to missingness, but we are also not using a potentially large chunk of the dataset due to missingness itself. Also, in multiple-imputation models the RVI value is not only a function of the quantity of missing data, but also of the effectiveness of the multiple imputation model (due to the pre-existing structure of the data) in recovering the underlying "true" values for each missing data point.

The second value reported below the model is the largest fraction of missing information (FMI): it simply reports the largest fraction of missing information for all coefficient estimates due to nonresponse. This number can be used to get a sense about whether the number of imputations M is high enough for the analysis. A rule of thumb is that $M \geq 100 \times FMI$ provides an adequate level of reproducibility in the MI analysis. In the subsequent analysis, keep in mind that I am working with 10 multiply-imputed dataset; however, whenever the "Largest FMI" value exceeds 0.10, I estimate the model using 20 multiply-imputed datasets, which can be considered enough even for the outlier Model 29 (see Table 6.10), for which the largest FMI is 20.1%.

Another feature of multiple imputation that is important to recall is that, unlike usual estimation techniques, the reported significance levels and confidence intervals of each coefficient are based on DFs that are specific to each coefficient, and vary with their missingness level. Therefore, it may happen that the model shows non-significant results not only because data is unsupportive of a correlation within our sample, but also because missing data is too frequent for that specific variable to let us recover a significant result. This may for example be true for inequality data, which is missing for 23.6% of our sample. Therefore, the measure for inequality needs to pass a much higher threshold than other more complete variables, due to its relative high level of missingness. That said, this is exactly what we aim for when employing multiple imputation techniques: we want to make the best use of the information within our dataset, but we also want to obtain reliable estimates that do not overconfidently estimate standard errors around our main variables of interest due to some biased imputation procedure.

To get a sense of how much the missingness in each variable influences standard errors a diagnostic option in stata (*dftable*) shows the percentage increase in the estimated standard error attributable to missingness. Sticking to inequality, which is the variable with most missing values in my models below, the increase in standard errors floats at around 3% in all models, which is a considerable feat given that around a quarter of the observation are missing. Sometimes, other variables suffer from a larger increase of standard errors due to missingness, but it is never exceeds 15%: overall, no coefficient in none of my models becomes insignificant due to missingness alone.

Finally, also of importance in multiple imputation analysis is to note that postestimation and prediction calculations tend to be not directly applicable to models estimated through multiple imputation techniques. While estimation methods are well-defined within each individual completed-data analysis, they may not have a clear interpretation or consistent large-sample properties when the individual analyses are combined in the final MI step (Carlin et al. 2008; White et al. 2011). MI estimates may not even have a valid variance-covariance matrix associated to them when the number of imputations is smaller than the number of estimated parameters. So, although I will show some post-estimation and prediction calculations, these should be taken with a grain of salt, as they come with very clear caveats that they can only serve as illustrative examples. That said, some standard postestimation commands have been proven to remain valid even under a multiple imputation framework, such as joint tests for significance (which I will employ below) and estimates of predicted values for the units of the original dataset (Carlin et al. 2008).

6.3. Correlates of regime choice: political liberalization and democratization

In this section, I employ my 10 (or 20, as explained in section 6.2.4) multiply-imputed datasets in order to test the Hypotheses I derived in section 2.6. The dataset I rely upon contains a sample of 154 countries over the 1970-2007 period. For countries, I followed an inclusive strategy, deciding to leave out only states with too few or no observations. This is the case for dozens of small islands and micro-nations (like Liechtenstein, Andorra, St. Lucia, etc.), Taiwan, Cuba, North Korea, Myanmar and Somalia (for lack of data), and too recent states (like Montenegro and South Sudan). With regards to the time span, I choose to be somewhat conservative in order not to bias my analysis through unreliable data – whether too recent or too old. I choose to stop at 2007, before the Great Financial Crisis hits the global economy and years before the Arab Spring (attempted) transitions, because the full effects of both might not have come to full bear. An obvious avenue for future research is to extend the following empirical analysis to more recent years.

I choose 1970 as the earliest cut point because before that date, data becomes highly spotty or unreliable – especially data that matters most for my analysis, such as inequality measures or fuel rents. Moreover, before the great wave of nationalizations of the rentier states' oil industry, for the major part rents from the sale of oil and natural gas did not accrue to the governments of these countries, but were "expropriated" by international oil companies. Extending the analysis to earlier years, as others have done (Haber and Menaldo 2011, Ross 2012) would therefore bias the analysis by greatly overestimating the political leverage of fuel rents accruing to most countries in my sample before the nationalization "waves" started.

As for the variables I employ, my dependent variables are based upon my four operationalisations of political regime type, as explained in Chapter 4, on the CGV dataset for a binary measure of democracy (Cheibub et al. 2010), and on my recoding of *polity2* and the UDS scores, as explained in Chapter 3.

On the other hand the main independent variables that I will employ throughout the following analysis are:

- Political leverage of fuel rents: by default, I use my favourite measure which is based on exports rents per capita as a share of GDP, as described in Chapter 5. In general, results do not change using total fuel rents per capita as a share of GDP, unless when specified;

- Inequality: I rely upon the Standardized World Income Inequality Database (SWIID; Solt 2009, Solt 2014), version 5.0 developed in 2014. Specifically, I employ the Gini market value before redistribution, and fill in some missing values by relying on the Global Peace Index database for later years. Missing observations in earlier decades (1970-1989) have been imputed at the imputation stage described in section 6.2;
- Economic variables, such as GDP per capita PPP and GDP growth, are taken from the World Bank's World Development Indicators database;
- Political violence is measured via the Major Episodes of Political Violence dataset, employing the *actotal* variable (which aggregates all conflicts on a 0-14 occurrence/intensity scale; Marshall and Cole 2014).

Table 6.1 reports descriptive statistics for the main non-binary, non-lag regressors that I chose to include in the following analysis (for brevity, I do not report descriptives for potential substitutes for each of them). A host of other variables has been included at various stages: given that they proved to be non-significant and do not change the substantial effect of significant variables, they have been dropped from the subsequent analyses.[3] In non-linear models such as logit or ordered logit, all these variables have been standardised to facilitate interpretation of substantive effects.

In the models below, all standard errors are always panel-data corrected. This is because the normal variance estimator would hugely underestimate the variance within panel data, treating all observations as independent, while we know that observations for each country tend to depend on observations of the same country in previous years, and are not to be treated as independent from each other. This implies that we expect standard errors to be correlated among country observations.

[3] These controls include: total population (logged), population density, urbanization rate, fertility rate, share of young population, age dependency ratio, population growth, urban population growth, net migration rate, unemployment (total), youth unemployment, male unemployment, poverty rate, primary sector (% GDP), industry (% GDP), military expenditures (% GDP), gross capital formation (% GDP), coal rents (% GDP), mineral resource rents (% GDP), and forest rents (% GDP).

	Range	*Median*	*Mean*	*Std dev*	*% missing*
Inequality	18.4 – 79.4	43.8	44.5	9.2	23.6%
Fuel rents pc / GDP pc	0 – 76.8	0	4.21	10.98	45.2%
GDP pc PPP (logged)	4.96 – 11.80	8.88	8.80	1.28	43.9%
GDP growth	-62 – 150	3.94	3.77	6.64	7.7%
Life expectancy	19.5 – 85.2	66.6	63.7	11.2	0.4%
Political violence	0 – 14	0	0.77	1.86	0.5%

Table 6.1. Descriptive statistics for non-binary regressors in the original dataset

Before delving deeper in the analysis, it seems best to introduce here a reasoning on fixed effects estimation in panel data. As will be clear from the following sections I will estimate some fixed-effects models, reporting and discussing results, but I will not employ them for the bulk of my analysis, for example to test my main hypotheses.

Fixed effects models come with a great benefit: they allow researchers to account for the effect of any unobservable non-changing variable. This is a great opportunity that investigators should not miss, because time-invariant unobservables are legion in the social sciences.

At the same time, fixed effects come with a number of liabilities, which are too often ignored or too easily dismissed in the recent comparative politics literature that has tested for the correlates of regime type and transitions (see for example Acemoglu et al. 2008, Boix 2011). First, fixed effects change the research question. Adding country dummies to the estimated equation is equivalent to demeaning each variable for the country’s average, which is in turn equivalent to asking how is a change in the output variable affected by changes in the values of the independent variable. Therefore, when we are interested in the effect of the values of our independent variables upon our dependent variable, fixed effects are unsuitable for answering this question.

Recent methods to deal with omitted variable bias in a panel-data setting without doing away with between-country variation have been proposed in the literature (Fayad et al. 2012, Wright et al. 2013). However, the estimators employed in both models are still liable to criticism as to their large-sample properties, and cannot be incorporated yet in multiple imputation models.

Researchers are left with two options: either completely change their research questions, abandoning questions that have to do with levels and just investigate effects of changes; or leave fixed effects aside.

A second liability of fixed effects models is that they assume that observations are highly reliable. Unless this is the case, changes from one year to the next might be the result of measurement error just as likely (or even more likely) than the result of true yearly changes. While every social scientist would prefer this not to be the case, most social science data tends to be unreliable at the yearly level, with even highly reliable economics data being consistently revised from one year to the next. In some developing countries, even numbers as reliable as GDP get revised by huge amounts (think about 2014, when a country as big as Nigeria revised its GDP upwards by 89%; that same year Kenya, Tanzania, Uganda and Zambia all revised their GDP figures up by 25% or more). In fact, numbers often tend to be more unreliable where – at least for the testing of democratization and democratic transition – matter most, i.e. in autocratic developing countries (Jerven 2013, Jerven 2015). Lacking sufficient precision, yearly estimates are a best guess that is useful to make inference over their level and in large samples: their reliability greatly decreases when they serve as the basis to infer effects at the country level.

Finally, especially when we are dealing with economic data, yearly variations are seldom interesting per se. A large swath of unobservable and non-measurable elements can affect data that appear to be similar on first sight. Think, for example, of the effect of an oil price slump on fuel rents, and compare Venezuela and Saudi Arabia. Being endowed with a very large sovereign wealth fund, Saudi Arabia can smooth out the effect of the slump over an estimated 5-7 years at the current rate, and can therefore afford to weather the storm, while Venezuela is already on the brinks of outright economic default. The same shock, and comparable initial levels of oil dependency, mean two very different things for two different countries according to their ability to withstand single-year shocks. Again, level equations are much more resistant to such smoothing out, while fixed effects would be unable to account for such differential ability to withstand the same shock unless much more information was supplied to the model via new independent variables.

Given all that, as said, our main models will rely upon regressions in levels. In order to avoid to introduce bias, we will try to account for many of the potential and measurable covariates of interest, and that theory suggests that should be included in the "true" model.

6.3.1. Testing the implications of my theoretical model

In this section, I will test the implications of my theoretical model (see Chapter 2.2), namely the hypotheses that I drew from it and the literature (see Chapte 2.6). Overall the chapter, I will search for answers to the question: what makes a country choose more or less liberal political regimes than others? The research question is novel, given that previous studies focused only on democratization or autocratic stability/retrenchment, and answering is only feasible because I am relying upon my four operationalisations of political regime types, which define a typology of regimes ranked by liberalization level: closed autocracies, open autocracies, and democracies (the fourth operationalization subdivides the autocratic space into three liberalization levels, adding democracy, for robustness).
My main test relies upon ordered logit models (Models 1-4, see Table 6.2). Ordered logit is a regression model for ordered dependent variables. Just like the logit, the ordered logit is a model that uses a latent / censored variable approach, in which the true data generating process for political regime openness is assumed to be continuous:

$$y^* = x'\beta + \varepsilon$$

but in the data collection phase one can only observe its discrete outcomes:

$$y = \begin{cases} 0 & if\ y^* \le \mu_1 \\ 1 & if\ \mu_1 < y^* \le \mu_2 \\ 2 & if\ \mu_2 < y^* \le \mu_3 \\ & \dots \\ N & if\ \mu_N < y^* \end{cases}$$

The model assumes that the distance between different levels of regime openness may not be equal, meaning that the (conceptual) distance between a very closed autocratic regime and an open autocratic regime is not necessarily the same than the distance between an open autocracy and a democracy. Given that my outcome variables are not interval-level, but discrete ordinal variables, this assumption is the safest: an OLS regression would most probably be unfeasible apart from the highly unlikely case in which the measurable distance between the three (or four, in the case of the *regime4* operationalisation) categories were the same.

One important thing to keep in mind, however, is that ordered logit regressions assume proportional odds, i.e. that the relationship between each pair of outcome groups is the same. In short, it assumes that the same mechanism that increases the likelihood by a certain amount of ending up in the open autocracy or democracy category while being in closed autocracy is the same as the mechanism increasing the likelihood of that same amount of ending up in a democracy while being in an open autocracy.

This assumption is also called the "parallel slopes" assumption, and it is only thanks to this that we only need to recover one set of coefficients. If this assumption did not hold, we would need to estimate a much greater set of coefficients in our model.

In the estimation output for an ordered logit (see e.g. Table 6.2), the cut points, or intercepts, are estimates indicating where the latent variable was cut to make the three (or four) groups that we observe in our data.

All tables reporting estimation outputs for ordered logits or logits all display odds ratios, for ease of interpretability. Odds ratios are simply the exponentiation of the recovered log odds (or ordered log odds) coefficients, and can straightforwardly be interpreted into an increase or decrease in probability of being in a higher (if they are above 1) or lower (otherswise) category, given a 1-unit increase of the regressor, holding all other regressors constant.

	Model 1 DV: regime1		Model 2 DV: regime2		Model 3 DV: regime3		Model 4 DV: regime4	
Regime$_{t-1}$ (lag of DV)	251 (65.0)	***	296 (75)	***	622 (165)	***	147 (37)	***
Inequality (std)	1.07 (.06)		1.12 (.07)	*	1.09 (.09)		1.08 (.06)	
Inequality squared (std)	0.95 (.03)	.	0.93 (.03)	*	0.93 (.04)	.	0.93 (.03)	**
Hydroc. exports rents per cap. (std)	0.88 (.04)	**	0.87 (.05)	**	0.83 (.05)	**	0.90 (.03)	**
GDP pc PPP (logged, std)	1.19 (.09)	*	1.25 (.09)	**	1.33 (.11)	***	1.21 (.09)	**
Political violence (std)	1.00 (.04)		0.98 (.05)		1.00 (.07)		1.00 (.04)	
Post-Cold war	3.21 (.97)	***	3.26 (1.05)	***	2.94 (1.34)	*	2.46 (.64)	***
South & Central America	0.99 (.44)		0.85 (.38)		0.81 (.34)		0.81 (.32)	
Sub-Saharan Africa	0.41 (.17)	*	0.40 (.16)	*	0.28 (.11)	***	0.38 (0.14)	*
Middle East & North Africa	0.29 (.12)	**	0.26 (.12)	**	0.15 (.06)	***	0.24 (.09)	***
Post-Soviet countries	0.40 (.17)	*	0.40 (.18)	*	0.30 (.13)	**	0.33 (.13)	*
South Asia	0.63 (.29)		0.54 (.26)		0.26 (.11)	**	0.48 (.21)	.
East Asia	0.68 (.37)		0.61 (.34)		0.50 (.29)		0.55 (.27)	
South-East Asia	0.46 (.21)	.	0.40 (.19)	.	0.38 (.17)	*	0.41 (.17)	*
Europe	1.84 (.80)		1.63 (.74)		1.59 (0.67)		1.37 (.56)	
Year dummies	YES		YES		YES		YES	
Latent var: 1st cut	6.87 (0.55)	***	6.83 (0.57)	***	6.91 (0.58)	***	5.76 (0.49)	***
Latent var: 2nd cut	12.40 (0.76)	***	12.82 (0.76)	***	14.65 (0.76)	***	10.63 (0.66)	***
Latent var: 3rd cut							16.07 (0.90)	***
Average RVI	0.56%		0.61%		0.70%		0.68%	
Largest FMI	5.8%		10.7%		7.0%		6.5%	
Observations	5,101		5,101		5,101		5,101	

Notes. Results of ordered logit models. The table reports odds ratios. Standard errors adjusted for 153 clusters (countries) in parentheses. (std) variables have been standardized to facilitate interpretation. The initial dataset has been imputed 10 times.
Significance levels: *** = .001 ** = .01 * = .05 **.** = .10

Table 6.2. Determinants of political liberalization levels.

In an ordered logit, odds ratios are called proportional odds ratios, and we can interpret them much in the same way as we would interpret odds ratios for a binary logistic regression – with a simple tweak. So we will say that for a 1-unit increase in a covariate, the odds of the highest category (democracy) happening are [proportional odds ratios] greater, assuming all other variables in the model are held constant. Likewise, the odds of (democracy + open autocracy) compared to closed autocracy are again [proportional odds ratios] greater.

Looking at the results for Models 1-4 allows me to check how my hypotheses fared against real-world occurrences. Here, I run over them one by one. Hypothesis 1 stipulated that regime types evolve slowly over time, and that "there is a huge inertia from one year to the next". From the results, one can see that the lag of the political regime is highly significant, and that the estimated odds ratios tend to dominate the whole estimation process. This allows me not to reject Hypothesis 1.

In general, each country has an overwhelmingly higher probability of being in the same regime type category as it was in the year before. Note that throughout model estimation I will be using a one-year lag; given the huge inertia of the political regime type process, this soaks up a lot of variability from the data: this can serve as a further robustness test for my remaining variables. Taking a five-year lag greatly decreases the lag's explanatory power (while keeping it highly significant), and in general enhances the effect of all other regressors. I use the one-year lag as a further robustness test to my findings.

Moving on, Hypothesis 2 suggested a higher likelihood to find more liberal political regimes at average levels of inequality. Looking at the models' results, I find the quadratic coefficient for the measure of inequality to be broadly significant, while the linear term drifts in and out of significance. Recall that, given that I am using standardised values, the linear coefficient is estimating the effect of inequality over liberalization at the sample average level of inequality. Using non-standardized coefficients and shifting the inequality measurement so that it has a minimum at 0 rather than at 18.4 (this is important so that the quadratic relationship is always estimated in-sample) makes both terms return significant ($p<.05$) or highly significant ($p<.01$) p-values. Also, even with standardized values, a test of joint significance rejects the null hypothesis in Models 2, 3 and 4.

Substantively, the coefficients suggest that low and high levels of inequality decrease the likelihood of choosing a more liberal political regime as compared to average levels. The maximum of the quadratic curve (i.e. the maximum positive effect) is estimated to occur at inequality levels of between 50.1 and 52.2 across models:, while in my sample displays values ranging between 18 and 79, and both median and average values are at about 45. Therefore, the estimated "peak liberalization effect" is just above average values.

This appears to be broadly consistent with my theoretical model, but also with Acemoglu and Robinson's (2001, 2006) and other political economy models based on a conflict between citizens and elites. In turn, this finding appears to contradict Boix's (2003) model suggesting that more liberal regimes should be more likely when inequality is low. I consider this to be an important finding because it broadens the scope of the effects of inequality, moving from democratization to liberalization levels.

Hypothesis 3 postulated that the post-Cold War period (which I define as a country being in the 1990-2007 period) enhanced the incentives for liberalisation, encouraging autocrats to lead (even incomplete) liberalizations. Empirically, I find the relationship between the post-Cold War period and more liberal regime types to be highly significant: the 1990-2007 period has countries around 3 times as likely to choose more liberal political regime levels than during the two decades between 1970 and 1989.

As to Hypothesis 4b, acknowledging an indeterminate relationship between resource rents and liberalization levels, the models appear to show that there is a significant and negative relationship between the two. For each standard deviation increase in political leverage of rents, a country has significantly less chances to liberalize: its chances drop on average by between 10% and 17% in our four models. Given that a country like Saudi Arabia has average levels of fuel rents at around 40% throughout the sample period, and that one standard deviation increase in fuel rents is roughly equivalent to an 11% increase, this suggests that Saudi Arabia is today 40% to 87% less likely to be democratic than if it had no hydrocarbon rents.

Turning to Hypothesis 5, relating levels of economic development to levels of liberalization, I fail to reject it too: indeed, I find that for each standard deviation increase in log GDP per capita at purchasing power parity, the chances of a political system landing in a more liberal category increase by between 19% and 33%.

As for Hypothesis 6, which is concerned with political regime types clustering in space, this appears to be the case and to confirm that less liberal political regimes are mostly likely to be found in Sub-Saharan Africa, the MENA region, and within Post-Soviet countries (see the appendix to this Chapter).

Finally, as for Hypothesis 7a, which states that specific years might have been more conducive than others to liberalisation due to a domino-like contagion effect that is not dependent on other socio-economic correlates, I do not report year dummies for lack of space, but I describe here the patterns that appear from the estimation. In particular, I find support only for liberalising and not for anti-liberalising waves (across models, only 1998 flares up as a year of significant autocratic retrenchment – and this only in Model 4). Liberalising waves tend to be consistent across models, with a cluster of significant pro-liberalising years between 1986 and 1991. However, this finding is starkly different from Huntington's account of a "third wave" of democratization starting in 1974 and still occurring at the time of writing (1991): the "pure" domino effect that I can account for in my model is much smaller and is overwhelmingly concentrated around the end of the Cold War and the early Nineties.

It is interesting to note that, while pro-liberalisation years appear to cluster in what I categorized as still being the Cold War period, and then stop abruptly after 1991, still the post-Cold War period reports a much higher likelihood for polities to choose in favour of more liberal regimes. This goes some way to showing that, at least for the period 1991-2007, while no new "political regime wave" has occurred, countries liberalized steadily throughout the period – and that there was not just a domino-like effect at play, but a host of other causes that are being picked up by the correlates included in the models.

	Model 5 *DV: democracy (CGV)*		*Model 6* *DV: democracy (CGV)*		*Model 7* *DV: democracy (CGV)*		*Model 8* *DV: democracy (CGV)*	
Regime$_{t-1}$ (lag of DV)	2,142 (574)	***	156 (46)	***				
Regime$_{t-5}$ (lag of DV)					3.12 (.74)	***	1.84 (.41)	**
Inequality (std)	1.00 (.12)		0.59 (.15)	*	0.79 (.15)		0.80 (.15)	
Inequality squared (std)	0.97 (.07)		1.04 (.12)		0.85 (.08)	.	0.87 (.08)	
Hydroc. exports rents per cap. (std)	0.69 (.11)	*	0.71 (.40)		0.41 (.14)	**	0.44 (.19)	*
GDP pc PPP (logged, std)	1.26 (.16)	.	0.17 (.22)		2.01 (1.23)		0.32 (.37)	
Political violence (std)	0.93 (.11)		0.65 (.12)	*	0.78 (.10)	.	0.72 (.10)	*
Post-Cold war	4.38 (3.71)	.	120 (159)	***	12.4 (8.2)	***	27.0 (19.9)	***
South & Central America	2.15 (.91)	.			42.7 (107)			
Sub-Saharan Africa	0.50 (.19)	.			0.00 (.01)	**		
Middle East & North Africa	0.27 (.12)	**			0.00 (.00)	***		
South Asia	0.94 (.54)				5.31 (17.2)			
East Asia	0.99 (.64)				1.54 (8.54)			
South-East Asia	0.73 (.34)				0.32 (.98)			
Europe	3.68 (1.69)	**			3,158 (7,351)	***		
(constant)	0.01 (.01)	***			0.01 (.01)	***		
Year dummies	YES		YES		YES		YES	
Fixed effects	NO		YES		NO		YES	
Average RVI	0.15%		1.22%		0.70%		1.40%	
Largest FMI	3.3%		14.4%		8.4%		23.6%	
Observations	5,101		2,089		4,490		1,791	

Notes. Results of logit models. The table reports odds ratios. Standard errors adjusted for 153 clusters (countries) in parentheses. (std) variables have been standardized to facilitate interpretation. The initial dataset has been imputed 10 times.
Significance levels: *** = .001 ** = .01 * = .05 **.** = .10

Table 6.3. Determinants of democracy and democratization.

After reporting the results for my main models, I compare the correlates of liberalization and liberal retrenchment to the correlates of democracy and autocracy. I therefore use the Dictatorship and Democracy dataset (or CGV, for Cheibub et al. 2010), which divides the political regime space between democracies and non-democracies.

Models 5 to 8 (see Table 6.3) report results for a logit regression with the CGV variable for democracy as the dependent variable. Models 6 and 8 also employ fixed effects.First of all I find that, again, political regimes tend to be highly static and inertial, as the highly significant lag of regime type shows. The very high levels of regime resilience convinced me to test for Models 7-8 that employ a five-year instead of a one-year lag, so as not to totally soak up variability due to a potential unit root process.

Differently from findings about liberalisation levels, and irrespective of the chosen model, inequality does not appear to have a significant effect upon the choice between democracy and autocracy. This goes a long way to show the importance of choosing the appropriate research question, and of gauging liberalization levels as well as democratization levels: movements *within* autocracies towards higher or lower liberalization levels would go unnoticed in models only relying upon transitions towards and away from democracy – as in this case.

On the contrary, fuel rents appear to have an even larger anti-democratic effect, decreasing the probability of a polity to be democratic by around 30% as they increase by one standard deviation in Model 5, and by a whopping 55-60% in Models 7 and 8. Going back to my Saudi Arabia example, if the country had no fuel rents it would be expected to be about 3 to 6 times more likely democratic. What is interesting to note here is that, even after controlling for fixed effects, the correlation is still significant and highly negative, implying that the anti-democratic consequences of fuel rents are present even within and not just between countries. In particular, movements towards democracy are estimated to be 55% less likely as fuel rents increase by one standard deviation.

Interestingly as well, in this democracy-autocracy setting I find smaller support for the modernisation theory effect. Model 5's estimated 26% higher likelihood of being democratic for a one standard deviation increase in GDP per capita PPP is at the higher range of the 19%-33% spectrum previously estimated for liberalization, but has less significance overall. What is even more daunting is that, within countries, the effect disappears overall (even changing in sign).

On the other hand, the "incentives to democratization" hypothesis appears to be standing even in this setting, as the models suggest an even higher likelihood of choosing a democratic regime type (or, for fixed effects, transitioning towards democracy) than just liberalising.

As for "regime waves", again I find support in favour of a single democratic wave in the 1985-1990 period. In the models with country-fixed effects, two "autocratic moments" are suggested in 1998 and 2004, but this finding is unsupportive of any cluster of autocratic waves.

Finally, it is interesting to note that political violence appears to have a role – especially within countries. Specifically, a one-standard-deviation increase in the level of political violence appears to discourage transitions towards democracy by between 28% (Model 8) and 35% (Model 6).

6.3.2. Levels of liberalisation and democracy

In this section, I move back to interval-level measures of political regime openness and democracy. While I am interested in checking whether some effects appear to be robust and consistent to attempts at gauging political regimes in a more fine-grained way, caution must be applied due to the fact that attempts at such a refined analysis may in fact fail to capture subtle regime changes and may be more likely to be affected by expert judgment bias and expert disagreement (Martinez i Coma and van Ham 2015, Gervasoni 2010).

Again, I report fixed effects results, but see benefits and liabilities of fixe effects in section 6.3.1. However, it may be interesting to see whether results change when moving from one modelling framework to the other.

Starting with levels of regime openness, Models 9 to 12 (see Table 6.4) show the results of a linear model with panel-corrected standard errors, with Models 9 and 11 using the whole sample of country-years, and Model 10 and 12 only sticking to determinants of the level of regime openness within autocracies. Models 11 and 12 also employ fixed effects.

As in Models 1 to 8, regime levels appear to be highly resilient across time, so that the one-year lag of regime openness has a significant and substantive effect over the expected current level of regime openness.

Moving on to inequality, the quadratic relationship appears to be significant both in Model 9 and 10, with the relation being more significant when considering changes in liberalization levels within autocracies rather than for the whole sample. Substantively, the maximum "liberalization effect" is estimated to appear at an inequality level of 55 for the whole sample, or at 57 for autocracies (in Model 12 employing fixed effects, the effect for autocracies remains significant, but the peak is estimated to take place at 47). These are all just-above sample average levels, so that I again fail to refute Hypothesis 2 even considering liberalisation levels.

Fuel rents display an interesting pattern. While they appear to have a small anti-liberal effect across countries, when I switch to fixed effects, i.e. to the role they have within countries, their effect is estimated as being slightly liberalizing and highly significant. While this may be interpreted in the context of the controversial effect fuel rents may have in the interplay between autocrat and citizen in the model expounded in Chapter 2, one needs to keep in mind the huge problems we face in interpreting fixed effects, especially in terms of short-term data reliability, and the widely different abilities of governments to smooth the effects of fiscal shocks over a longer period of time.

Moving on, I find support for the modernization theory only when considering the whole sample, and not when constraining the analysis to autocracies. Meanwhile, the incentives to democratization appear to have a significant, high and consistent effect across models. Also, regional clustering and time effects appear to be present (with the only wave again being a democratic one, occurring between 1986 and 1991).

	Model 9 *DV: regime openness*		*Model 10* *DV: regime openness (autocracies only)*		*Model 11* *DV: regime openness*		*Model 12* *DV: regime openness (autocracies only)*	
Regime$_{t-1}$ (lag of DV)	.887 (.013)	***	.886 (.014)	***	.789 (.016)	***	.709 (.029)	***
Inequality	.026 (.013)	*	.059 (.022)	**	.017 (.020)		.058 (.031)	.
Inequality squared	-.0002 (.0001)	.	-.0005 (.0002)	*	-.000 (.000)		-.001 (.000)	*
Hydroc. exports rents per cap.	-.005 (.001)	**	-.004 (.002)	.	.007 (.003)	**	.010 (.003)	**
GDP pc PPP (logged)	.031 (.009)	**	.028 (.020)		-.119 (.077)		-.062 (.127)	
Political violence	-.012 (.011)		-.022 (.015)		-.049 (.018)	**	-.059 (.028)	*
Post-Cold war	.415 (.119)	**	.516 (.187)	**	.750 (.157)	***	.892 (.273)	**
South & Central America	.043 (.062)		.147 (.118)					
Sub-Saharan Africa	-.097 (.066)		.149 (.062)	*				
Middle East & North Africa	-.320 (.106)	**	.007 (.130)					
Post-Soviet countries	.070 (.070)	**	.430 (.116)	***				
South Asia	-.212 (.115)	.	-.233 (.160)					
East Asia	-.007 (.070)		.395 (.133)	**				
South-East Asia	-.106 (.081)	*	.120 (.132)					
Europe	.005 (.058)		.488 (.137)	***				
(constant)	.378 (.347)		-1.314 (.547)	*				
Year dummies	YES		YES		YES		YES	
Fixed effects	NO		NO		YES		YES	
Average RVI	1.15%		1.38%		11.03%		5.31%	
Largest FMI	9.65%		3.84%		13.88%		8.68%	
Observations	5,101		2,946		5,101		2,946	

Notes. Results of linear regression models. Standard errors adjusted for 153 clusters (countries) in parentheses. The initial dataset has been imputed 20 times.
Significance levels: *** = .001 ** = .01 * = .05 **.** = .10

Table 6.4. Determinants of the choice and change of political regime openness.

	Model 13 *DV: polity2 (fixed)*		*Model 14* *DV: UDS*		*Model 15* *DV: polity2 (fixed)*		*Model 16* *DV: UDS*	
Regime$_{t-1}$ (lag of DV)	.94 (.01)	***	.95 (.01)	***	.88 (.01)	***	.87 (.01)	***
Inequality	.001 (.003)		.000 (.000)		-.007 (.006)		-.001 (.001)	
Hydroc. exports rents per capita	-.006 (.001)	***	-.001 (.000)	***	.003 (.003)		.000 (.000)	
GDP pc PPP (logged)	.038 (.014)	*	.006 (.002)	***	-.346 (.144)	*	-.032 (.015)	*
Political violence	-.006 (.012)		-.005 (.001)	***	-.025 (.023)		-.008 (.002)	***
Post-Cold war	.691 (.181)	***	.012 (.025)		1.49 (.29)	**	.113 (.033)	**
South & Central America	.040 (.096)		-.006 (.012)					
Sub-Saharan Africa	-.310 (.111)	*	-.039 (.013)	*				
Middle East & North Africa	-.542 (.141)	***	-.057 (.016)	***				
Post-Soviet countries	-.575 (-.180)	**	-.059 (.018)	**				
South Asia	-.334 (.162)	*	-.031 (.014)	*				
East Asia	-.237 (.217)		-.032 (.024)					
South-East Asia	-.286 (.138)	*	-.029 (.018)					
Europe	.120 (.088)		.029 (.012)	*				
(constant)	-.042 (.238)		-.025 (.028)		4.68 (1.71)	*	.447 (.175)	*
Year dummies	YES		YES		YES		YES	
Fixed effects	NO		NO		YES		YES	
Average RVI	0.91%		0.52%		14.0%		10.5%	
Largest FMI	7.31%		6.85%		9.71%		12.1%	
Observations	5,058		5,101		5,058		5,101	

Notes. Results of linear regression models. Standard errors adjusted for 153 clusters (countries) in parentheses. The initial dataset has been imputed 20 times.

Significance levels: *** = .001 ** = .01 * = .05 **.** = .10

Table 6.5. Determinants of the choice and change of democracy levels.

Finally, also of note is that, within countries, the occurrence of political violence appears to have a significantly anti-liberalising effect, pushing them to reduce their liberalization level both in the whole sample and within autocratic types.

Leaving levels of liberalization aside and moving to levels of democracy, in Models 13 to 16 (see Table 6.5) I rely upon my two chosen measures of democratic level, i.e. my recoding of *polity2* and the Unified Democracy Scores. Models 15 and 16 also employ fixed effects.

I continue to find that actual regime levels tend to be highly resilient to previous regime levels. In addition, I find additional support for regional clustering and "democracy waves" effects. Also, the incentives towards democratization implied by the Post-Cold war period appear at work here as well.

In terms of important covariates, inequality does not appear to have any bearing on levels of democracy, both in its linear and in quadratic form (results for the quadratic are not reported, but are available upon request). On the opposite, fuel rents appear to have a consistently negative effect upon democracy, continuing to show their anti-democratic effect.

Especially of note, finally, are findings concerning the modernisation hypothesis. While across countries levels of economic well-being appear to have a consistently positive, pro-democracy effect, when I move the analysis to the within-country effect, an increase in GDP per capita PPP appears to have a completely different effect, decreasing the *polity2* democracy score by one third of a point for each one-unit (logged) increase in GDP. The effect appears to be quite strong, and *prima facie* it is hard to tell whether it is driven by unreliable data containing lots of noise or data is sufficiently reliable and that is a true effect.

6.4. Correlates of regime stability and change

In this section I shift my research question and ask: what affects the probability of political regime change? Are correlates of regime stability similar or different to the correlates of political regime type?

These questions are especially relevant since, as I consistently found in section 6.3, political regimes tend to be highly inertial, suggesting that once a polity settles for one of such regimes, it is then difficult to change it unless some particular conditions occur, even as covariates change (considerably) over time.

Among other things, there could be an important interactive effect at work. This may be the case if I find that some factor that increases the likelihood of a regime to change in the first place, also influences the choice of a political regime once regime change occurs, possibly shifting the long-run equilibrium in favour of liberalization/democratization, or anti-liberalization/autocratization. On the other hand, the correlates of regime change may be too different from the correlates of regime liberalization to suggest any potential long-run equilibrium.

6.4.1. What makes a regime "change"?

For models 17-20 (see Table 6.7), I use my operationalisations of regime types (see Chapter 4) but I create new variables from them. First, I build a dummy variable taking the value 1 whenever a political regime within a particular country changes compared to the previous period. Over my four operationalisations of political regime types, I find that regime change occurs between 6% and 6.6% of the time for the tripartite variables, and as expected it occurs a (slightly) larger amount of time (7.8%) for my quadripartite variable, *regime4*.

Given that from now on events become increasingly rare, it will also become difficult to pinpoint correlations. As King and Zeng 2001 show, logistic regressions are especially likely to underestimate the probability of rare events. Although the multiple imputation setting does not allow me to correct for such underestimation, I am comforted by the fact that, whenever I find significant results, they will remain significant or become even more significant in a rare-event setting. My analysis will therefore be more conservative, allowing me to tease out the most robust effects only from the rest.

Table 6.6 reports the correlation between the four regime change measures, showing some interesting variability between them. Ultimately, different thresholds at which I set the difference between closed and open autocracies make all the difference here (see Chapter 4).

	Δ regime1	Δ regime2	Δ regime3	Δ regime4
Δ regime1	1			
Δ regime2	0.89	1		
Δ regime3	0.74	0.77	1	
Δ regime4	0.91	0.83	0.87	1

Table 6.6. Correlation table between measures of political regime change.

I also create a "regime duration" variable: for each country-year, this variable takes the value of the number of years any particular "regime openness level" has been in place in a given country until that year. Basically, this is a count variable that resets to 1 each time a regime change event occurs.

Note that here the regime change and regime duration variables do not describe the durability of specific regimes, such as a military regime leaving the stage to a civilian dictatorship or a democracy. Whenever a highly-illiberal military regime cedes power to a highly-illiberal civilian dictatorship, my coding does not pick this up as a regime change. Meanwhile, if the same highly-illiberal military regime decides to move to an open autocracy setting while no actual institutional transition takes place, this will be coded as a "regime change" by my variables.

Therefore, the question that I am asking here is not whether a certain type of autocracy or democracy is more resilient to "failures" (i.e. to give ground to other political regimes in terms of institutions governing the system), but whether any regime in any country is more or less likely to open up or retrench, going down liberalization or anti-liberalisation paths, as conditions change and irrespective of whether that political regime also changes the institutional setting in the meantime.

	Model 17 *DV: Δ regime1*		*Model 18* *DV: Δ regime2*		*Model 19* *DV: Δ regime3*		*Model 20* *DV: Δ regime4*	
Regime duration	0.98 (.009)	*	0.99 (.010)		1.02 (0.01)		0.98 (0.01)	*
Semi-presidential democracy (CGV)	3.00 (1.22)	**	2.97 (1.23)	**	5.14 (2.44)	**	3.24 (1.35)	**
Presidential democracy (CGV)	2.28 (0.88)	*	2.48 (0.99)	*	3.98 (1.81)	**	2.35 (0.92)	*
Civilian autocracy (CGV)	1.51 (0.52)		1.22 (0.44)		0.49 (0.22)		1.79 (0.63)	.
Military autocracy (CGV)	3.07 (1.05)	**	2.90 (1.02)	**	2.90 (1.15)	**	4.82 (1.67)	***
Royal autocracy (CGV)	1.31 (0.64)		1.04 (0.55)		0.89 (0.59)		2.22 (1.05)	.
Inequality (std)	1.11 (0.10)		1.11 (0.11)		1.11 (0.14)		1.11 (0.10)	
Hydroc. export rents per capita (std)	1.06 (0.11)		1.05 (0.13)		1.12 (0.18)		1.01 (0.11)	
GDP pc PPP (logged) (std)	0.63 (0.13)	*	0.52 (0.13)	**	0.41 (0.13)	**	0.62 (0.12)	*
GDP growth (std)	0.80 (0.07)	*	0.80 (0.07)	*	0.74 (0.08)	**	0.82 (0.07)	*
Life expectancy (std)	0.83 (0.14)		0.83 (0.15)		0.87 (0.21)		0.84 (0.14)	
Political violence (std)	1.15 (0.08)	*	1.21 (0.09)	*	1.26 (0.12)	*	1.20 (0.08)	*
*GDP growth * political violence*	1.03 (0.03)		1.03 (0.02)		1.04 (0.03)		1.03 (0.02)	
Post-Cold war	0.80 (0.49)		0.84 (0.53)		0.33 (0.24)		0.53 (0.32)	
South & Central America	1.38 (0.55)		1.47 (0.65)		0.99 (0.60)		1.43 (0.60)	
Sub-Saharan Africa	0.70 (0.27)		0.65 (0.28)		0.63 (0.39)		0.77 (0.31)	
Middle East & North Africa	1.08 (0.45)		1.35 (0.64)		0.87 (0.59)		1.07 (0.47)	
South Asia	1.71 (0.77)		1.59 (0.81)		1.10 (0.83)		1.61 (0.80)	
East Asia	1.88 (0.97)		1.53 (0.94)		0.77 (0.74)		1.51 (0.89)	
South-East Asia	0.82 (0.37)		0.72 (0.37)		0.42 (0.32)		0.72 (0.35)	
Europe	0.80 (0.38)		1.08 (0.56)		0.52 (0.36)		0.85 (0.43)	
(constant)	0.02 (0.01)	***	0.02 (0.01)	***	0.01 (0.01)	***	0.03 (0.01)	***
Year dummies	YES		YES		YES		YES	
Observations	5,101		5,101		5,101		5,101	

Notes. Results of logit models. All regressors lagged one year. The table reports odds ratios. Standard errors adjusted for 153 clusters (countries) in parentheses. The initial dataset has been imputed 10 times.
Significance levels: *** = .001   ** = .01   * = .05   . = .10

Table 6.7. Determinants of change in political liberalization levels.

The models presented here and in the following sections will be panel-data logistic regressions. In this section, the dependent variable is the dummy for the occurrence of regime change, in any direction. All independent variables have been lagged one year in order to avoid mixing up the causes and consequences of regime change/transitions.

Results show, first, that the regime duration variable is only slightly significant and shows odds ratios below but very near to 1. This lends only marginal support to the hypothesis that regimes tend to consolidate over time (at least in terms of liberalization levels, not as regards their peculiar institutional settings). Therefore, the chances that any liberalization or anti-liberalization movement takes place within a country depend only slightly on the years that have passed under the previous regime.

Second, results show that some political regimes, this time classified according to their institutional setting (i.e. military, civilian or royal autocracy, and different democratic political systems) are significantly more likely to undergo changes in their liberalization level as compared to a parliamentary democracy (our baseline): namely, military autocracies, semi-presidential democracies and presidential democracies are the most prone to changes in their openness level. Although no conclusive evidence can be drawn from these results, this appears to show more risk towards autocratic retrenchment than towards liberalization openings, given that presidential and semi-presidential democracies can only fail "downwards" to lower levels of liberalisation, while an "open military autocracy" in principle may move in both directions.

Importantly, results also show that many of the correlates of political regime choice that have been found to be significant in section 6.3 do not matter at all in enhancing or stifling the probabilities of regime change. Inequality, the political leverage of rents, and incentives to democratization all disappear from significance, as do regional dummies for geographic clusters and time dummies.

On the opposite, three variables stand out for significantly and consistently affecting the likelihood of political regime changes: the level of economic well-being, GDP growth, and the intensity of political violence. The two economic variables both have a negative impact on the likelihood of political regime failure. As economic well-being increases by one standard deviation, the likelihood of a political regime change occurring is estimated to decrease on average by between 37% and 56% (see 6.4.2 for the decomposition of this effect and a discussion of the modernization hypothesis in light of this result). At the same time, as GDP growth increases by one standard deviation, the likelihood of political regime change drops by between 20% and 26%.

I also find that the occurrence of political violence significantly increases the chance of political regime change by between 15% and 26%. While political violence may well be endogenous to regime transitions, meaning that it may not be a trigger but a consequence of many such transitions, recall first that I lagged all variables in order to assuage the endogeneity problem. A second valid criticism could be that political violence is not just an "enabler" for political regime change, but may also be triggered by a set of covariates. This would again suggest endogeneity and, in turn, would make the model biased for lack of available instruments.

Note however that when dropping the political violence variable, all other model findings are consistent. Moreover, what I am most interested in this section is to find correlates of regime transitions that may allow me to gauge the likelihood of an actual transition occurring; in a prediction setting, endogeneity problems should be less prevalent than in a causal relationship setting.

All this notwithstanding, in order to further assuage potential problems caused by endogeneity, I interacted GDP growth and political violence, so that the two variables are left to influence each other (we may expect that as political violence increases, GDP growth decreases, and vice versa) and I can recover cleaner estimates of the direct effect of the two.

To conclude, I am left with a very interesting picture about the correlates to regime transitions from one level of regime liberalization to another, whereby political institutional settings, short-term (GDP growth) and longer-term (economic well-being) economic variables, and political violence all contribute to bolster or stifle the chances of transitioning from a level of political liberalization to another.

6.4.2. Explaining liberalization and political retrenchment

Section 6.4.1 paints an interesting picture of the set of correlates with regime transitions, and allows me to maximize the frequency of rare events in my dataset (political regime transitions) and recover better estimates. At the same time, I am still left wondering whether the correlates of transitions differ significantly with the direction of the transition itself. In this section I ask: are correlates of regime liberalization the same or different to correlates of liberal retrenchment?

Models 21-28 (see Tables 6.8 and 6.9) try to disentangle the two. Again, these are panel-data logistic regressions. This time, however, the dummy for occurrence of political regime change takes the value of 1 only whenever the level of liberalization of the political regime succeeding the "failed" one is lower (Models 21-24, Table 6.8) or higher (Models 25-28, Table 6.9). The regime duration variables are recalculated accordingly.

Political regime change events become even rarer this way. For movements away from democracy and towards a more autocratic setting, occurrences across models only vary between 1.2% and 1.7%. For changes away from closed autocracy and towards more liberal forms of autocracy or democracy, occurrences range from 4.8% and 6.1% of total observations within the sample. Again, recall that given that I am not correcting standard errors for the rarity of such events, the following tests will be interesting insofar as they will return a conservative picture of what potential real effects might be.

Results show that there indeed appears to be different forces at work: this allows me to distinguish between correlates of upwards movements towards liberalization, and correlates of downwards movements towards autocracy. More interesting still is the fact that most variables of potential interest disappear from significance, and we are left with just a handful of them that manage to withstand the higher significance threshold set by the fact that transitions are rare events.

The single common cause decreasing the likelihood of any political regime transitions, either upwards or downwards, is the very duration of a political regime: for every year that any level of liberalization has been present in a country, the probability that that country transitions upwards or downwards decreases (on average) by between 2% and 4% in both cases. This lends credit to the path-dependency hypothesis as interpreted by the comparative politics literature (see section 1.2.1): as regimes endure, they consolidate, requiring increasingly larger forces in order for change to occur in the first place.

On the other hand, while political violence was found to be correlated with the overall likelihood of regime transitions in the preceding section, it now remains significant only as an explanation for autocratic retrenchment, not pro-liberal transitions: as political violence increases, the likelihood of transitioning towards a more autocratic regime increases by 10-11%. This suggests that liberalization requires a concerted effort by many actors and some degree of consensus away from political violence to occur, while the more unrest spreads within a country, the less that country will be likely to liberalize during the transition process.

The main variable that is found to be significantly correlated with autocratic retrenchment is GDP growth. My models consistently find that as economic growth increases by one standard deviation, the likelihood of experiencing autocratic retrenchment decreases on average by 43-44%, and vice versa for periods of economic recession.

Therefore, while an economic recession may increase the chances of a political regime change, this change usually brings about an autocratic retrenchment, and is not the harbinger of progressive change for the country overall that it could have been expected to be at first. As a substantive example in reality, the near-default of Venezuela in 2015-2016 has been hailed as an opportunity for change in the country after over a decade of autocratic mismanagement, but according to my models the steep recession (not to mention the increasing inequality level, which is today estimated to be higher than my sample average) makes it much less likely that the country will be able to democratise in case the current Maduro regime embarks upon regime change or leaves the stage to other actors.

	Model 21 *DV: Δ regime1 (to less liberal)*		*Model 22* *DV: Δ regime2 (to less liberal)*		*Model 23* *DV: Δ regime3 (to less liberal)*		*Model 24* *DV: Δ regime4 (to less liberal)*	
Regime duration	0.95 (0.01)	**	0.97 (0.01)	*	0.96 (0.01)	**	0.96 (0.01)	**
Inequality (std)	1.16 (0.16)		1.06 (0.14)		1.07 (0.15)		1.14 (0.14)	
Hydroc. export rents per capita (std)	0.91 (0.16)		0.90 (0.17)		0.93 (0.19)		0.87 (0.15)	
GDP pc PPP (logged) (std)	0.88 (0.24)		0.82 (0.23)		0.77 (0.25)		0.85 (0.22)	
GDP growth (std)	0.58 (0.08)	***	0.58 (0.08)	***	0.58 (0.09)	***	0.57 (0.08)	***
Life expectancy (std)	0.85 (0.20)		0.80 (0.19)		0.92 (0.24)		0.88 (0.19)	
Political violence	1.10 (0.06)	*	1.11 (0.06)	*	1.11 (0.06)	*	1.10 (0.05)	*
*GDP growth * political violence*	1.05 (0.03)		1.03 (0.03)		1.03 (0.03)		1.05 (0.32)	
Post-Cold war	0.44 (0.48)		0.45 (0.50)		0.59 (0.67)		0.30 (0.33)	
South & Central America	1.24 (0.66)		1.82 (1.04)		1.54 (0.96)		1.25 (0.64)	
Sub-Saharan Africa	0.88 (0.49)		1.11 (0.67)		1.32 (0.84)		1.08 (0.57)	
Middle East & North Africa	1.14 (0.68)		1.75 (1.09)		1.26 (0.89)		1.33 (0.74)	
South Asia	1.99 (1.24)		2.34 (1.56)		3.36 (2.27)		2.22 (1.31)	
East Asia	1.46 (1.26)		0.89 (1.01)		1.01 (1.02)		1.23 (1.07)	
South-East Asia	0.97 (0.62)		1.11 (0.78)		1.11 (0.84)		0.80 (0.50)	
Europe	0.22 (0.17)		0.19 (0.22)		0.19 (0.21)		0.23 (0.19)	
(constant)	0.04 (0.02)	***	0.02 (0.02)	***	0.02 (0.02)	***	0.05 (0.03)	***
Year dummies	YES		YES		YES		YES	
Average RVI	0.81%		1.18%		1.05%		0.96%	
Largest FMI	13.3%		12.5%		15.4%		18.1%	
Observations	3,914		4,819		3,523		4,021	

Notes. Results of logit models. All regressors lagged one year. The table reports odds ratios. Standard errors adjusted for 153 clusters (countries) in parentheses. The initial dataset has been imputed 20 times.

Significance levels: *** = .001 ** = .01 * = .05 **.** = .10

Table 6.8. Determinants of political retrenchment.

	Model 25 DV: Δ regime1 (to more liberal)		*Model 26 DV: Δ regime2 (to more liberal)*		*Model 27 DV: Δ regime3 (to more liberal)*		*Model 28 DV: Δ regime4 (to more liberal)*	
Regime duration	0.98 (0.01)	*	0.97 (0.01)	**	0.97 (0.01)	***	0.96 (0.01)	***
Inequality (std)	1.01 (0.10)		1.07 (0.11)		0.95 (0.10)		1.01 (0.09)	
Hydroc. export rents per capita (std)	1.13 (0.13)		1.08 (0.14)		1.07 (0.14)		1.11 (0.11)	
GDP pc PPP (logged) (std)	0.53 (0.11)	**	0.52 (0.12)	**	0.63 (0.15)	*	0.59 (0.11)	**
GDP growth (std)	0.96 (0.10)		0.95 (0.10)		0.89 (0.11)		0.99 (0.09)	
Life expectancy (std)	0.80 (0.15)		0.84 (0.16)		0.84 (0.17)		0.79 (0.13)	
Political violence (std)	1.06 (0.08)		1.07 (0.08)		1.03 (0.09)		1.02 (0.07)	
*GDP growth * political violence*	1.03 (0.03)		1.06 (0.03)		1.06 (0.04)		1.03 (0.03)	
Post-Cold war	1.73 (1.31)		2.32 (1.84)		0.94 (0.80)		1.02 (0.71)	
South & Central America	2.42 (1.04)	*	2.23 (0.96)	.	2.06 (0.90)	.	2.24 (0.86)	*
Sub-Saharan Africa	0.92 (0.41)		0.86 (0.38)		0.99 (0.45)		1.05 (0.41)	
Middle East & North Africa	1.46 (0.70)		1.37 (0.67)		0.97 (0.51)		1.51 (0.64)	
South Asia	1.55 (0.78)		1.58 (0.79)		1.38 (0.72)		1.75 (0.79)	
East Asia	2.43 (1.41)		1.96 (1.20)		1.11 (0.80)		1.87 (1.04)	
South-East Asia	0.90 (0.48)		0.75 (0.41)		0.98 (0.55)		0.97 (0.46)	
Europe	1.88 (0.98)		2.06 (1.08)		1.29 (0.69)		1.72 (0.82)	
(constant)	0.01 (0.01)	***	0.01 (0.01)	***	0.02 (0.01)	***	0.03 (0.01)	***
Year dummies	YES		YES		YES		YES	
Average RVI	0.57%		0.60%		0.89%		0.63%	
Largest FMI	10.4%		7.8%		13.5%		10.3%	
Observations	4,971		5,101		4,667		5,101	

Notes. Results of logit models. All regressors lagged one year. The table reports odds ratios. Standard errors adjusted for 153 clusters (countries) in parentheses. The initial dataset has been imputed 20 times.
Significance levels: *** = .001 ** = .01 * = .05 **.** = .10

Table 6.9. Determinants of political liberalization.

Finally, moving to causes for political change in favour of liberalization, I find that GDP growth stops being significant, as well as political violence, while a different economic variable becomes highly and consistently significant: the level of GDP per capita PPP. However, the expected effect is exactly the opposite than modernization theory would suggest: a one-standard-deviation higher level of economic wellbeing is significantly associated to a decrease in the likelihood of transitioning towards more liberal political regimes by a substantive amount, on average by between 37% and 45%.

This has deep implications for the extension and testing of modernization theory to a liberalization setting: while we consistently find that, across countries, more liberal political regimes tend to be associated with significantly higher levels of economic well-being, the same higher well-being levels may increasingly stifle the likelihood of transitioning towards more democratic levels. For example, a country finding itself in a condition of open autocracy will have less and less chances of democratizing further as its income per capita grows. This may for example be the case for China, Russia, Singapore, Malaysia, Azerbaijan – all countries that have experienced strong economic growth over the 2005-2014 decade, but that according to all of my measures are still stuck in an open autocratic regime. This also does not bode well for closed autocratic regimes that have been increasing average income levels very fast over the last decade and a half: namely, Gulf countries and other rentier states, at least until the 2014 oil price crash.

One mechanism to explain this apparent contradiction is that, while economic well-being brings with it a number of socio-economic transformations that make any polity more likely to call for increasing participation and representation in political activity, increasing income levels at the same time bolster state capacity, making it less likely that the government of a country can be overturned – or undergo a liberalizing transition.

This is an important result, highlighting the complex relationship between some of the covariates of liberalization. While some of them proved to be crucial in determining the political regime choice in the first place, other factors appear to be at play when it comes to regime transitions. These “enablers” may sometimes overlap with covariates that have a substantive effect upon political regime type choices in the first place; other times, they may interact in unpredictable ways, as those correlates that make a country more likely to become more liberal, may also make it less likely to change its current liberalization level in the first place.

In the end, this does not mean that a polity will in some cases be stuck for a long time with its initial choice of regime type; it just goes to restate once more that the likelihood of any regime transition is low, and that most of the times it will be the specific circumstances of a polity that determine the timing of a regime transition, not structural covariates. Only then will correlates of political regime choice, posited in my formalization in Chapter 2 and investigated through the empirical models in section 6.3, be able to come into play.

6.4.3. Further dissecting type-to-type transitions

This section embarks upon a final, but still provisional, attempt at refining the investigation of the correlates of regime transitions. I develop six models relating to the three different regime types that I conceptualized in my theoretical model: closed autocracy, open autocracy, and democracy. Each model uses the *regime2* variable in order to identify the cut point between closed and open autocracy, but results are consistent whether I rely either on *regime1* or *regime3* instead (the *regime4* variable is excluded because it posits a quadripartite division of the political regime space).

I partition the political space in the three regime categories: democracy (D), open autocracy (OA), and closed autocracy (CA). I then ask whether there are some specific correlates explaining transitions from a particular regime type to a particular other, obtaining 6 possible transitions. Under this empirical setting, transitions become rarer still and significant correlations should get rarer as well.

	Model 29 *DV: transition from D to OA*		*Model 30* *DV: transition from OA to CA*		*Model 31* *DV: transition from D to CA*	
Inequality (std)	1.78 (0.92)		1.18 (0.20)		1.14 (0.34)	
Hydroc. export rents per capita (std)	1.08 (0.84)		1.05 (0.24)		1.90 (0.65)	
GDP pc PPP (logged) (std)	0.44 (0.23)		0.49 (0.12)	**	0.22 (0.08)	***
GDP growth (std)	0.74 (0.33)		0.65 (0.07)	***	0.47 (0.10)	**
Political violence (std)	1.16 (0.42)		1.31 (0.15)	*	1.49 (0.37)	.
Post-Cold war	1.21 (1.06)		0.40 (0.14)	**	0.24 (0.12)	**
(constant)	.002 (.003)		0.02 (0.01)	***	0.01 (0.01)	***
Year dummies	YES		YES		YES	
Average RVI	3.92%		3.65%		0.92%	
Largest FMI	20.1%		13.1%		3.9%	
Observations	2,115		1,759		2,115	

Notes. Results of logit models. All regressors lagged one year. The table reports odds ratios. Standard errors adjusted for 153 clusters (countries) in parentheses. The initial dataset has been imputed 20 times.
Significance levels: *** = .001 ** = .01 * = .05 . = .10

Table 6.10. Unpacking political retrenchments.

	Model 32 *DV: transition from CA to OA*		*Model 33* *DV: transition from OA to D*		*Model 34* *DV: transition from CA to D*	
Inequality (std)	1.13 (0.16)		0.70 (0.19)		1.04 (0.24)	
Hydroc. export rents per capita (std)	0.88 (0.17)		0.01 (0.01)	*	0.65 (0.22)	
GDP pc PPP (logged) (std)	0.65 (0.12)	*	1.90 (0.74)	.	1.15 (0.34)	
GDP growth (std)	1.16 (0.11)		0.89 (0.14)		0.83 (0.17)	
Political violence (std)	1.11 (0.12)		1.05 (0.24)		0.90 (0.19)	
Post-Cold war	4.01 (1.19)	***	3.45 (1.74)	*	2.49 (1.10)	*
(constant)	0.03 (0.01)	***	.002 (.002)	***	0.01 (0.01)	***
Year dummies	YES		YES		YES	
Average RVI	2.58%		1.43%		1.39%	
Largest FMI	8.2%		6.3%		5.0%	
Observations	1,227		1,759		1,227	

Notes. Results of logit models. All regressors lagged one year. The table reports odds ratios. Standard errors adjusted for 153 clusters (countries) in parentheses. The initial dataset has been imputed 20 times.
Significance levels: *** = .001 ** = .01 * = .05 **.** = .10

Table 6.11. Unpacking political liberalizations.

Models 29 to 31 (see Table 6.10) show the three transitions towards less liberal regimes: Model 29 studies transitions from democracy towards open autocracy, Model 30 accounts for transitions from open to closed autocracy, while Model 31 is concerned with transitions from democracy directly to closed autocracy.

As shown by the models' results, these three types of autocratic retrenchment appear to be correlated to different variables with different strength, although most of them appear to push in the same direction. Model 29 finds no significant relationship between transitions from democracy to open autocracy and my covariates. However, this may be due to the fact that in the period 1970-2007 such transitions happen just 7 times over 2,115 country-years, which is a mere 0.3% of the sample. Transitions from open to closed autocracy instead happen 48 times over 1,759 country-years (around 2.7% of the sample), while transitions from democracy to closed autocracy happen 27 times (which is about 1.3% of the sample).

These latter two types of transitions are found to be discouraged both by a growth in economic well-being and by GDP growth. On the opposite, political violence appears to increase their likelihood. The estimated average effect of the economic variables seems to be much higher for democracies, meaning that the same increase in economic well-being or GDP growth discourages autocratic retrenchment more than in an open autocratic case.

What is interesting here is that economic well-being goes back to showing a significant (and substantial) impact in discouraging transitions towards less liberal regimes. This appears to add even further complexity to my findings around the modernization hypothesis in 6.3.2, given that in this setting modernization is found to discourage all transitions, but mostly those occurring in the "wrong" way, from more liberal to less liberal regimes.

Finally, both here and in the next three models studying transitions towards more liberal political regimes, I continue to find evidence that the post-Cold War period was especially fertile for transitions towards more liberal regimes, and discouraged transitions towards less liberal ones.

Moving to Models 32-34 (see Table 6.11), pro-liberal transitions are more frequent in the 1970-2007 sample period; however, correlations almost disappear. I record 75 transitions from closed to open autocracy (6.1% of the sample), 44 transitions from open autocracy to democracy (2.5%), and 33 transitions from closed autocracy straight to democracy (2.7%).

Results show that fuel rents appears to stifle pro-liberal transitions only from open autocracies to democracy, but given the incredibly strong result this is likely an artefact that no transitions from open autocracy to democracy occurred in rentier countries over the sample period.

Meanwhile one finding in particular appears to shed light over how and when, more precisely, higher economic well-being may stifle transitions towards more liberal regimes. Specifically, Model 32 shows that GDP per capita PPP has a significant and negative effect on the likelihood of transitioning from closed to open autocracy, with the effect being substantively large (one standard deviation in economic well-being decreases the likelihood of transitioning by 35%), while it does not appear to stifle any other pro-liberal transition.

6.5. Conclusions and general considerations

To conclude and summarize: this Chapter shows the importance of discriminating between political liberalization and democratization trends. Some correlates of democracy appear to be even stronger in explaining levels of political regime liberalization. The effort to go beyond dichotomous partitions of the political regime space, while at the same time treating these new categories as ordinal and not interval-level "degrees" of liberalization, proved crucial in explaining what makes actors within a polity choose a more liberal political regime (including but now limiting to democracy) and what instead decreases the likelihood that this happens. I am also able to tell apart the correlates of liberalization from the correlates of anti-liberal transitions, and the two from the correlates of regime stability.

In general, I find support for all seven hypotheses that I brought forward in Chapter 2 (see below), which also allows me to conclude that my theoretical model appears to be able to consistently explain political regime choice over the last four decades.

Going into further detail, the most important findings of this chapter are:

- I find support for the hypothesis that higher political liberalisation levels should be found at average levels of inequality, and that the probability of encountering more liberal regime types decreases both at low and high levels of inequality. This supports my theoretical model and agrees with Acemoglu and Robinson's (2006) model, while decreasing support for Boix's (2003) hypothesis as extended to liberalization, i.e. that only low levels of inequality make countries more likely to be liberal;

- I find that gains in economic well-being have a controversial effect over the likelihood of a country to be more liberal, complicating the modernization hypothesis. On the one hand, higher levels of GDP per capita PPP are found to have a liberalizing effect overall. On the other hand, the same higher levels of economic well-being significantly decrease the likelihood of regime transitions, strengthening even less liberal countries. The two forces appear to balance each other out, and it is hard to say in practice which will prevail and when;
- I find support for theories arguing that hydrocarbon resources are a "political curse", stifling movements towards higher liberalisation levels. However, they appear to still retain a controversial and contrary effect at the within-country level (with the caveats for fixed effects that have been discussed at length throughout this chapter);
- I find support only for one "pure" democratic wave in the late Eighties-early Nineties, while there does not appear to be any other temporal clustering in my sample, neither towards liberalization nor against it;
- I find support for the hypothesis that the post-Cold War era was a fertile period for liberalisation. I link this to increasing international incentives for autocratic elites to lead liberalisation movements (including incomplete democratic transitions);
- I find support for the hypothesis that political regime types, be they democracies, open or closed autocracies, appear to cluster in space even after controlling for a host of spatially-clustered correlates (such as economic well-being, inequality, economic growth, and others).

Appendix – Political Regimes: From Spatial Dependence to Regime Diffusion

Introduction

Since the late Seventies of the last century, when the “Third Wave” of democratization was just at its onset,[4] a small but growing portion of comparative politics studies has focused on the external causes of democratic transitions.[5] Similar studies had always been confined to a niche, both because of their difficult position between comparative politics and international relations and because of a lack of adequate statistical tools. Moreover, periodic spells of skepticism cast serious doubts on the fact that even the most advanced statistical tools could detect any correlation between international factors and the domestic process of democratization in the different countries of the international system. During the late Nineties, for example, an influential study on the causes of democratization by Przeworski and Limongi found that transitions towards democracy appeared to have been random events: only “democratic stability” could be significantly predicted by any set of indicators (Przeworski and Limongi 1997).

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4 For the hypothesis that transitions towards democracy tend to come in waves, clustering in time when not in space, see Huntington (1991).

5 Ross and Homer (1976) state that: “Because of interactions among units, especially among modern nation-states, it is difficult to think of any case where correlation between two traits or behaviours could be attributed only to processes internal to those societies, or 'pure' functions”. O’Donnell et al. (1986) find that out of 61 countries that had by then shifted towards democracy, 58 presented “varying degrees of external influence promoting the transition”. Something similar to exogenous influences was already proposed in Rustow (1970).
The historical interactions between the international environment and the internal institutional form of political regimes had already come under scrutiny in the late XIX century, and had been a central them of the *Machtstaat* school; see A. Colombo (2011).

With their reliance on domestic variables, Przeworski and Limongi seemed to endorse the preference of comparative researchers towards internal factors in order to explain the dynamics of regime change. Their work fitted neatly in a literature that implicitly "assum[ed] that external events and processes in other countries do not affect the political institutions of a country or the likelihood of transitions", bar, at times, foreign military interventions (Gleditsch and Ward 2006). Reversing this trend, in a 1998 seminal study on democratic diffusion a group of geographers, political scientists and statisticians aimed at systematically studying the international dimension of democratic transitions (O'Loughlin et al. 1998).

As of today, a large (although seemingly declining today) body of literature has been devoted to exploring the concept of "democratic diffusion" in the way O'Loughlin *et al.* first proposed it, introducing causal mechanisms that might be at work at the non-domestic level and developing models to test the hypotheses that can be derived from their theoretical framework. In the view of these authors, diffusion is "an encompassing term that in its general form simply indicates that there are enduring, cross-boundary dependencies in the evolution of policies and institutions". External factors "can change the relative balance of power between regimes and opposition forces as well as the preferences and relative evaluations that different groups hold over particular forms of governance" (Gleditsch and Ward 2006).

During the last decade and a half, researchers have constantly and consistently verified that, while domestic development indicators are robust predictors of democracy, "their predictive power fades with the inclusion of diffusion variables" (Wejnert 2005). At the same time, we must account for the fact that Galton's problem (see below) might be soaking up some variability that is in fact due to the spatial dependence of domestic variables themselves, without any "diffusion" process being actually ongoing.

Despite this important caveat, the success in the empirical testing of the diffusion hypothesis pushed Przeworski and Limongi to somewhat recanting their conclusions on the randomness of democratic transitions. In their 2000 book, the authors produced empirical evidence that democracy is more likely to survive in a country that is in a more democratic region than elsewhere (Przeworski et al. 2000). At the same time, endogenous explanations were put into question from a purely theoretical point of view. In 2004 Hans Schmitz proposed a fully-developed agency-based model that described some of the mechanisms that could link international pressures towards democratization to their domestic reception (Schmitz 2004).

Although the debate on the causes of democratization is still raging on, to set the stage for the following discussion we must take some steps back from it, considering the problem from a somewhat wider perspective. First of all, the subject of interest in the literature seems nowadays to be evolving in the direction of the study of *regime transitions* as a whole, both toward and away from democracy, and not just on the process of democratization *per se* (Goodliffe and Hawkins 2015). Secondly, regime diffusion stems from the verifiable recurrence of spatial dependence of regime types; that is to say that all regimes, not just democracies, tend to cluster in space.

Measuring and assessing regime spatial clustering

To say that all regimes tend to cluster in space is equivalent to say that countries at a certain democratic (or autocratic) level tend to be found geographically closer to, rather than farther from, countries at similar democratic levels. Exploratory Space Data Analysis (ESDA) allows for a measurement of the global spatial clustering of regimes and for a preliminary visualization of local clustering in space. In this section I also propose a preliminary statistical analysis of spatial clustering.

To measure democratic levels I relied on the *polity2* variable in the Polity IV dataset, which aggregates autocracy and democracy scores and ranges from -10 to +10. It seems preferable to treat the polity score as continuous rather than using any sort of threshold to dichotomize or trichotomize the variable, because the hypothesis I want to test is that positive spatial autocorrelation involves countries at similar levels of democratic development (or autocratic retrenchment), and not just countries above or below some democratic level. The dataset used here employs annual data for the period 1980-2009, and a range of 94-120 countries in the different years. For the following spatial analyses I use the CShapes dataset (Weidmann et al. 2010), calculating the centroids' coordinates of each country for each year (accounting for border modifications). I then generate a row-standardized spatial weights matrix and the eigenvalues for each year.[6]

Specifically, to generate the weights I rely upon inverse quadratic distances, not contiguity-based criteria. The distance matrix is fairly complex, being an *i* by *i* matrix (where *i* is any country in the dataset for a specific *t*, or year) that has no zero values. Albeit we suppose that the influence that each country can exert on each other decreases quadratically, the smallest amount of influence may be exerted by New Zealand upon Italy. Given that quadratic distances fall quite rapidly to zero, it would be possible to indicate a cutoff point and impose zero values after that point, but at this stage I prefer to let everything be led by data rather than choosing an arbitrary cutoff.

Conversely, a contiguity matrix would be an *i* by *i* matrix filled with a series of 1 and 0: generally, overwhelmingly zeros. A cell would take the value of 1 when the row and the column country are neighbors, and 0 otherwise. Specifically, among the different criteria to select which countries are actually neighbors, the most straightforward and evident is the queen contiguity matrix, meaning that a country is considered to be neighbor to another whenever the two any portion of the border.

I find choosing inverse distances instead of any contiguity criteria to be both methodologically sound and theoretically valid. First, a distance matrix increases the resolution of the spatial analysis. Second, it avoids a problem that arises for contiguity matrixes related to countries when analyzing the whole world, and which is created by the presence of oceans. In a contiguity matrix, Africa and Eurasia would be isolated from the Americas, or from Oceania (not to mention that Great Britain would be separated from Europe, Ireland from Great Britain, Japan from China and South Korea, et cetera!). By generating discontinuities, this creates "spatial islands" that cannot influence each other. This, in turn, poses huge issues both at a theoretical level (is a sea border sufficient to cut off from influence two countries that would be pretty near otherwise, such as Morocco and Spain?) and at a technical one ("islands" prevent any estimation procedure employing spatial dependence to converge towards a finite value for the spatial lag; Drukker et al. 2013).

6 To do so, I use Stata's spatwmat command included in Maurizio Pisati's sg162 package. I generate a row-standardized spatial weights matrix and the eigenvalues for each year.

Finally, using a distance matrix is theoretically valid because a diffusion hypothesis implies that it is not just first- or second-order neighbors' choice of political regime that influence a country's regime, but potentially any countries' positions relative to many other countries in the system, and specifically a "region" around the country should have the most influence irrespective of whether countries belonging to this region are effectively neighbors or just near to each other.

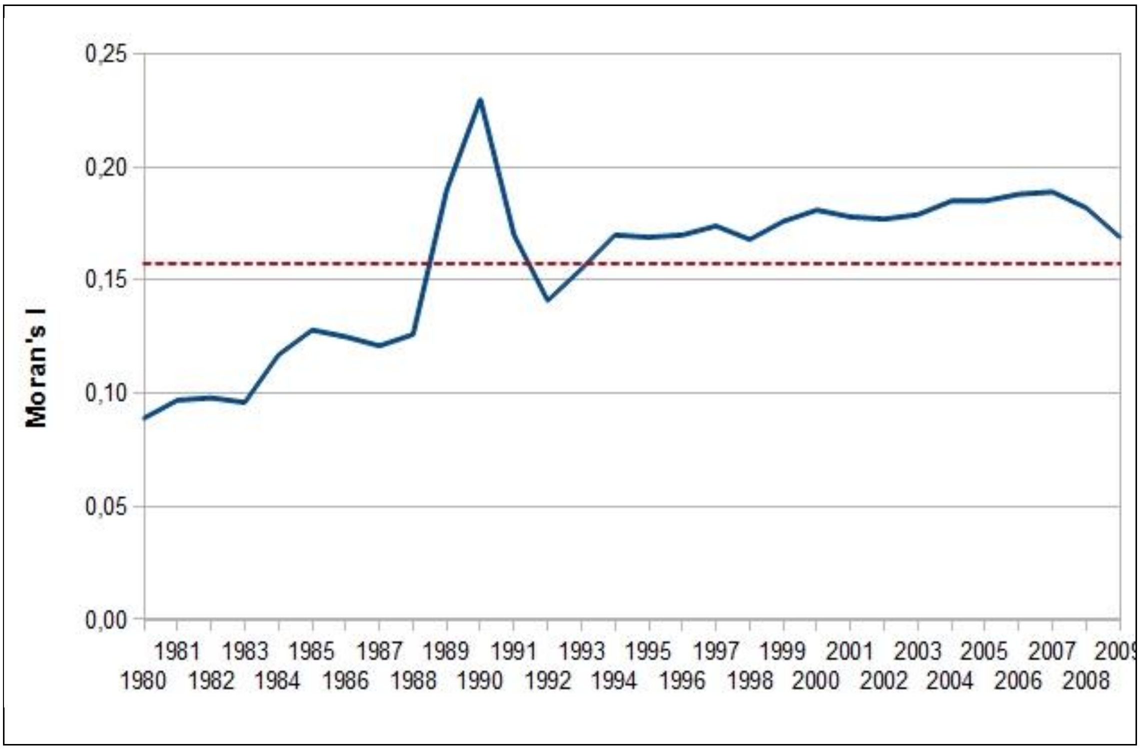
With the inverse distance matrix, I can calculate yearly values for the Moran's *I* score for cross-sections of all political regimes in the world, for each year in my sample. *I* is a classical index of global spatial autocorrelation: it compares the expected value for the null hypothesis of no spatial autocorrelation, along with its standard deviation, to the real autocorrelation that is estimated to be present in the data, thus allowing for z-tests for significance. The expected value for the null hypothesis of no spatial correlation is always negative but, for a sufficient number of cases, very close to zero. A non-significant *I* value as recovered within the dataset would mean that political regimes are randomly distributed in space. A positive and significant value of the index indicates positive spatial autocorrelation, this in turn meaning that countries tend to be geographically closer to others that display similar *polity2* scores (i.e., democracies tend to be nearer to other democracies; autocracies tend to be nearer to other autocracies).

By calculating the yearly value of *I*, I can track down the dynamics of global regime clustering, if any.

Figure 6.3 shows that in the 1980-2009 period countries have tended to significantly cluster around other countries with similar levels of political regime. Moreover, this positive autocorrelation has tended to increase with time, so that while in 1980 *I* was estimated at being below 0.09, in 2007 it reached 0.19. Figure A.1 also shows that the tendency of regimes to cluster in space has increased somewhat constantly spiking in the 1989-1991 period of rapid democratization of Eastern Europe, while abruptly decreasing right after that, falling back onto the previous path.

This slowly increasing trend may be interpreted as an indication that there is no real tendency towards convergence in different "regions" of the world. In fact, autocratic countries that are nearer to other autocratic countries tend to discourage the democratization of their neighbors. Finally, in the last decade the clustering seems to have somewhat plateaued, sensibly decreasing in the last two years.

Figure 6.3 – Moran's I value over time (1980-2009)



Note: Moran's I expected value (not reported) ranges from a minimum of -0.011 in 1980 to a maximum of -0.008 in 2009. Standard deviation for each year is around 0.02, so that every value is significant at the p=0.001 level. The red dashed line is the average value for the period.

Now that I have recovered an overall measure of spatial dependence between political regimes, and know how it trended over recent decades, I am interested in disentangling whether there are any specific regions of the world that "drive" the positive autocorrelation between political regimes. I therefore turn to local spacial indicators, and specifically to Anselin's LISA (Local Indicator of Spatial Association). LISA allows us to assign a "clustering value" to each country, and to tell us whether this value is significantly different from 0. The higher the number of political regimes with similar values are around a country, the higher the likelihood that this country is a "positive clustering" country. Vice versa, the higher the number of political regimes with very different values to a country's are around it, the higher the likelihood that this country can be considered a "negative clustering" country.

By mapping significant countries, we can check the geographic position of clusters of positive (or negative) spatial autocorrelation. To show how this works in practice, see the map in Figure 6.4, which refers to 2009 political regime types. In the map, the prevalence of clusters of democracies and autocracies as opposed to countries that display a negative autocorrelation (because they are democratic while having autocratic neighbors, or vice versa) clearly hints at the global positive autocorrelation. Democratic clusters are found where we would expect: in Europe, North and South America. Autocracies tend instead to cluster in the Middle East, in Central Asia, and in Central Africa.[7]

Although from this analysis it seems reasonable to conclude that spatial clustering might be the result of non-domestic forces, shaping each country's tendency towards or away from democracy, diffusion processes are actually very difficult to pin down, because "it is hard to distinguish true diffusion from illusions of diffusion created by global trends, correlated disturbances, or the regional clustering of domestic factors" (Brinks and Coppedge 2006). For example, if domestic factors such as GDP per capita tend to cluster in space, then regime clustering could be trivially correlated to the clustering of GDP per capita. This issue is common knowledge in the literature, and is classically referred to as Galton's problem.[8]

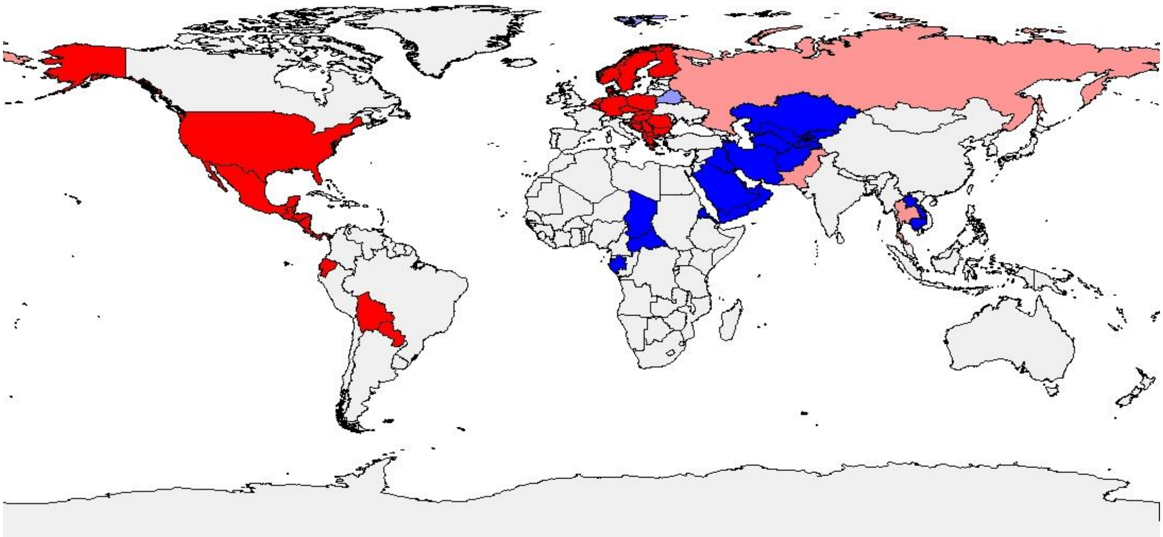
Examples of variables that may theoretically affect the levels of democracy and that tend to cluster in space range from the level of economic or social development to different types of institutional configurations, from dummies of a common colonial past to continuous variables measuring differences in size and population levels. The best a researcher can do to account for this problem is to control for many domestic variables in multivariate spatial models, and to check whether the regimes' positive spatial autocorrelation is robust to different specifications. Not doing so would cause a grave omitted variable problem, because there might exist a domestic variable that is not only significant but also clusters enough in space to render international spatial clustering dynamics irrelevant. At the same time, any domestic omitted variable that clusters in

7 The map also shows how difficult it is to solve between different spatial matrices. By choosing a matrix based on distances, that do not control for terrain formation or body of water separating countries, we can generate some weird inconsistencies. In this case, most of Western Europe does not belong to Europe's democratic cluster because Italy, France, Spain and Portugal are nearer to some Northern African countries with very low democratic levels than to democratic European neighbors. Also, the presence of some missing data in the northern part of South America (Polity IV does not rank Suriname and French Guyana, and GeoDa assigns a "0" value to missing data as a best guess between +10 and -10), is enough for Brazil to disappear from a democratic cluster.

8 In a recent reformulation, "[a] crucial challenge for empirical research (…) is the great difficulty distinguishing true interdependence of units' actions, on the one hand, from the impacts of spatially correlated unit-level factors, of common or spatially correlated exogenous-external factors, and of context-conditional factors involving interactions of unit-level and exogenous-external explanators on the other" (Franzese and Hays 2007).

space risks to show us regime spatial clustering as a force of its own, while there are background domestic forces at work driving this relationship.

Figure 6.4 – Local clusters of democracy scores (2009)



Note: image obtained with GeoDa using 5-nearest neighbors weights matrix. Dark red are democratic clusters (countries that have more democratic neighbors than the world average plus one standard deviation); dark blue are autocratic clusters. Light red (Russia, Pakistan and Thailand) are regimes that in 2009 were reported as being more than one standard deviation more democratic than their neighbors (given Russia's proximity to highly authoritarian regimes, this is not surprising), while light blue (Belarus) are regimes that were one standard deviation more autocratic than their neighbors.

Using 2009 data, as a preliminary attempt at controlling for internal factors, Table 6.12 reports the results of a spatial regression that includes some domestic predictors. I choose to model a spatial lag regression, which takes the general form:

$$Y = \boldsymbol{X}\beta + \rho\boldsymbol{W}Y + \epsilon$$

Where Y denotes a $N \times 1$ vector of observations on our outcome variable (political regime types), $\boldsymbol{X}$ denotes a $N \times j$ matrix of observations of the regressors, β denotes a $j \times 1$ vector of coefficients, $\boldsymbol{W}$ denotes the $N \times N$ spatial weights matrix, ρ is our estimated spatial lag (autoregressive) parameter, and ϵ is a $N \times 1$ vector of normally-distributed, homoscedastic, and uncorrelated errors.

	Model 1 *DV: polity2*	*Model 2* *DV: polity2*
GDP pc PPP (logged)	1.38 (0.15) ***	1.26 (0.36) ***
Oil rents	-0.85 (0.11) ***	-0.64 (0.13) ***
Oil rents (squared)	0.07 (0.00) ***	0.01 (0.00) **
Mineral rents	0.24 (0.18)	0.13 (0.12)
Land area (logged)	0.74 (0.30) **	0.46 (0.35)
Population (logged)	-0.18 (0.07) **	-0.29 (0.45)
Political violence	0.13 (0.08) **.**	0.18 (0.12)
(constant)	-3.26 (3.92)	-9.31 (6.87)
Spatial lag (rho)		0.78 (0.16) ***
R-squared	0.492	
Squared correlation		0.557
Sigma		4.20
N	120	120

Note: Results for linear (left) and spatial lag regressions.
Significance levels: *** = .001 ** = .01 * = .05 **.** = .10
Table 6.12. Levels of democracy and space.

The model assumes that there is an autocorrelation between the observed political regime of a country and other political regimes within this dataset, and that this autocorrelation depends on a spatial weights matrix that must be chosen by the investigator.[9] If we did not take the spatial lag into account, the autocorrelation would end up in the error term, and our model would be biased towards overestimating the influence of regressors. At the same time, we could not recover an estimate of the autocorrelation, which most of the time (as in this case) is of substantive interest.

The result of our spatial lag model shows that some economic domestic variables are highly significant: the level of economic wellbeing in a country (measured via the log of the GDP per capita at purchasing power parity) is positively correlated with democracy levels, as implied by the modernization theory. Oil rents over GDP tend to have a strong negative impact upon democracy levels, although the quadratic term implies that the effect moderates as a country is already dependent from oil rents.

On the contrary, and very importantly, other variables that are found to be significantly correlated with levels of democracy in a linear model, including a demographic variable

[9] In my case, not shown, the multivariate model proves robust to many different specifications of the distance matrix, with or without cut off points, and to the substitution of the distance matrix with a k-nearest neighbors matrix where only a number k of nearest countries is considered to determine the spatial relationship (in my robustness checks, I let k vary between 5 and 10).

(the log of the population), as well as the size of a country (the log of the land area), disappear from significance.

The spatial component, *rho*, is found to be not only highly significant but near the higher limit of its range (1.0).[10] Also, adding the spatial lag term appears to produce a somewhat better model in terms of its power to explain variation in the dependent variable. Indeed, a traditional linear regression without the spatial lag can explain around 49% of the variation in the data, while a spatial lag model explains 56%.

From regime clustering to regime diffusion: theory and causal mechanisms

Because of the complexity of spatial modeling, detailing huge matrixes of country-to-country dependences, the description and explanation of regime spatial clustering can only be achieved by using static, cross-sectional models.

Meanwhile, the literature on diffusion processes explicitly refers to dynamic process, so that time-series cross-section analyses should be preferred. Shifting from static to dynamic processes generates theoretical and methodological problems that studies on regime diffusion are forced to face. For example, the literature often uses regime transitions as a proxy measure of regime diffusion. "The global distribution of democracy as well as transitions to democracy cluster in time and space", say Gleditsch and Ward, adding that "taking a local or regional perspective can provide more insight into how external factors influence democratization and transition processes than an exclusive focus on global level influences" (Gleditsch and Ward 2006). The authors continue explaining that, although a large number of studies has found empirical evidence of "diffusion" processes at work, in the sense of spatial and temporal clustering in the distribution of democracy and transitions, "it is less clear what this stems from, and what it is about democracy in one state that influences the prospects for democracy in another" (Gleditsch and Ward 2006).

10 Note that a model with just the intercept yields a *rho* = .92. What is even more striking is that such a model explains alone 33.6% of the variance in the data. Keep in mind though that in this model *rho* includes by construction both the non-domestic, international pressures for political regimes to cluster in space, and the (almost certain) spatial clustering of many domestic factors.

In order to clarify the mechanisms that might be at work, from a purely theoretical and point of view, one can regard diffusion processes as international social processes composed of four elements: (1) innovation, (2) communicated through channels, (3) over time, among members of a (4) social system (Rogers 2003). Diffusion models are based on the non-independence of events, which "includes not only a temporal dimension but a spatial one as well". "The use of diffusion models makes a number of rather strong assumptions about the world in terms of behavior and behavioral processes and the interaction of agents and structures" (Starr and Lindborg 2003). Indeed, one element of diffusion processes is the emulation of some type of behavior within one unity of a system by other units (here, the propensity within countries in the international system to adopt some type of regime rather than another). This emulation can be driven by domestic as well as external factors, and diffusion analysis focuses on the external dimension: it links "behavior based on internal readiness [to] external cues" (Starr and Lindborg 2003).

Pinpointing the necessary elements of any definition of diffusion is obviously not sufficient to explain the links between international pressures and domestic outcomes, such as (rapid or gradual) regime change. To address this shortcoming, one of the most important studies on diffusion proposed four causal mechanisms, neither mutually exclusive nor exhaustive, through which national political elites might be conditioned into following the preferences of a set of other (neighbor) countries (Simmons et al. 2008).

The first mechanism is outright coercion, whereby the dominant actors in the system or sub-systems impose political institutions or cease to uphold others.[11] The second is competition: some institutions might end up being more effective than others for domestic political purposes. The third is learning: a progressive change in beliefs about the world might be happening at the national and international levels. For the study's authors, the latter mechanism is unidirectional, as influence *towards* democratization comes from some sort of Bullian 'anarchical society' and its contemporary international organizations, but one may argue that learning is a bidirectional process, and that democratic practices may be always supplanted by the "example" of a well-functioning autocratic polity (think of China or Singapore today, or South Korea before democratization; Bull 1977). Finally

11 See also the concept of "leverage" in Levitsky and Way (2006). In this case, 'leverage' is defined only in the direction of democratization, as the "authoritarian governments' vulnerability to external democratizing pressure", but it shouldn't be difficult to conceptualize an inverse relationship.

a fourth, residual category is called social emulation, and described by the authors as “the way in which policies are voluntarily instantiated”. At a closer analysis this does not appear as a very solid category, and apparently refers to Alexander Wendt's (1999) famous theorization of international politics.

Recently, scholars have drawn the attention upon the theoretical problems of such causal explanations, arguing that “[n]one of [those] processes clearly signals that geographically proximate actors should be the most important external influences on a state” (Goodliffe and Hawkins 2011). Another possibility, then, is to conjecture that states exist internationally “in dependence networks with each other” and that those networks “provide pathways for influence on a state's domestic political institutions”. In this conceptualization, a state's dependence network is “a set of partner states with whom [that state] regularly engages in exchanges of valued goods”, where these valued goods might be anything from commodities to ideas (Goodliffe and Hawkins 2011). Put in this way, the idea of a dependence network does away with any geographical determinants, so that any state in the world might potentially be part of any other state's dependence network, according to one or more chosen variables of interest, identified as driving the “influencing” relationship. Yet, few steps might be needed in order to acknowledge that the networks themselves tend to cluster in space, given that both the exchange of commodities and that of ideas are much more frequent (and, anyway, the possibility that they happen is generally greater) among nearer rather than farther countries, even in our globalized world (Gleditsch and Ward 2006).

Goodliffe and Hawkins even go as far as to explicate the possible mechanisms that might link these external dependence networks to domestic preferences on political regimes, conjecturing that the processes at work here might be at least three:

(1) domestic actors in government seek to gain rewards and avoid punishments offered by network partners for adjusting their domestic institutions appropriately;

(2) domestic actors in government and opposition gain resources useful in domestic power struggles through their interactions with network partners;

(3) *domestic actors in government learn how to adjust domestic institutions by observing network partners.*

Most studies fall short from offering precise and structured causal mechanisms such as these. Brinks and Coppedge, for example, propose a “neighbor emulation” mechanism, which they describe as “the tendency for neighboring countries to converge towards a shared level of democracy or nondemocracy” (Brinks and Coppedge 2006). It's not hard to notice that this is a tautological description of diffusion, not a causal mechanism *per se*. All in all, we seem to be still lacking a complete theory on regime diffusion dynamics, one that coherently and consistently links international pressures to domestic preferences, with Elkink's attitude diffusion model possibly coming closest to it (Elkink 2009).

Regime diffusion: empirical findings

As recently as 2011, a researcher in the field of democratic diffusion lamented that “[e]xplaining democracy with reference to factors external to societies (…) has a solid theoretical framework, but lacks clear empirical proof” (Lidén 2011).[12] Considering that most of the published articles on the topic in the last decade has actually produced sound and consistent empirical evidence, this hardly seems to be the case. Wejnert (2005), for example, found that at a global scale diffusion is a central predictor of democracy, even after controlling for economic development. As the democratic diffusion thesis by itself seems to be losing ground (because of an increasing risk that free countries fall back into “less free” categories, as opposed to a decreasing risk of non-free countries to land on higher ground[13]), support has continuously been found for regime diffusion as a general process that includes the influence of any political regime on any other. Brinks and Coppedge (2006), for example, find that the average regime score of neighbors affects in a significant and positive way the direction and magnitude of political regime change in any target country.[14] Even Goodliffe and Hawkins's (2011) study, which focuses on

12 Lidén’s (2011) paper provides a good review of the previous literature, but his proposed model specifications fail to provide any improvements whatsoever over earlier models.

13 “[p]ost-cold war growth in democracy might be more fragile than we suspect”; say Starr and Lindborg 2003.

14 The authors do not try to estimate whether the international environment also influences the probability of a target country of incurring in regime change. In fact, they posit it as unpredictable and impossible to identify in large-N studies.

dependence networks, consistently finds that “geographic proximity” facilitates “and thus proxy for some of these interactions”, and that these interactions in turn promote regime diffusion.

Also interesting is the impact that transitions themselves might have on neighbors’ propensity at regime change (domino/contagion effects). Starr and Lindborg (2003) find that “[o]f the 273 governmental transitions that occurred” in the period 1974-1996, “106 experienced some type of BGT [‘bordering governmental transition'] treatment”. As this proportion is significant at the .001 level, this should demonstrate a “positive spatial diffusion effect” of transitions.

Pending methodological issues

Given the variety of possible approaches to the issue of regime diffusion, some discussion around the different methods employed by different authors seems in order. Trying to measure diffusion processes poses the problem of which variable can best serve to measure the spread of political regimes. Simple measures of spatial autocorrelation are of little to no use, given that spatial regression only allows for time-static considerations in order to be feasible and its estimated being liable to use for prediction, while the study of diffusion clearly involves temporal dynamics.[15] The diffusion variable is typically a variable that tries to capture some sort “pressure” exerted by the international spatial distribution of political regimes over each different country. Once created, this “international pressure” measure can be included in regression analyses in order to control for its influence over (a) the propensity for regime change in the target country and/or (b) the direction of that change, given its occurrence.

This “international pressure” variable can be computed in several ways, with two of the most common in the literature being:

(1) taking the weighted or unweighted mean of the democracy score of neighboring countries. In this case, the authors usually construct a contiguity matrix, with cells

[15] Some models of space-time dependence can be found in recent literature (Franzese and Hays 2008, Baltagi and Pirotte 2012), but they still lack finite-sample properties amenable for prediction, they have a huge problem of efficiency, and their output is near-to-impossible to interpret.

that take value of 1 when two states are neighbors, and 0 otherwise (Brinks and Coppedge 2006; O'Loughlin et al. 1998). The use of weighted means is a very recent feature, and the authors that resort to it employ it "in order to capture [each partner's] differing potential for influence" (Goodliffe and Hawkins 2011);

(2) employing inverse distance weights, that imply that the influence of each country over the political regime of any target country decreases linearly with distance.

Much of the literature follows the first approach, only testing for "neighbor emulation" (O'Loughlin et al. 1998). However, choosing contiguity over distance poses important questions that are seldom answered, mainly because this would expose the fact that results are liable to huge sources of model dependence. This is so because the way in which one operationalizes who is neighbor to whom is not as straightforward as it might seem at first.

Take for example the UK, Australia, Japan, or any Pacific or Caribbean island: do we exclude these states from the analysis, or do we suppose they have neighbors at some threshold distance from their coast? And if we tend towards the latter solution, what will this threshold be, and shouldn't we adopt it also for continental countries? Thailand and Vietnam, for example, are separated only by a short strip of Laos; Ghana is even less far away from Benin.

What about, then, the number of neighbors: does the fact that Portugal will only be influenced by Spain (or by Morocco, at some minimum threshold distance), while China will be affected by all of her fifteen neighbors, compromise the statistical analysis in some serious way? One extreme example of this level of model dependence is offered by O'Loughlin et al.'s article, in which the US and the UK are coded as contiguous countries "based on cultural and political similarities" (O'Loughlin et al. 1998).

Contiguity and neighborhood seem thus to be highly insufficient and problematic conceptualizations, and preference should be given to studies employing some kind of (power-weighted) inverse spatial matrix.[16] Using a row-standardized matrix could also take into account the number of states at different distances from the country, meaning that "country density" in the whereabouts of each target country would also be considered

16 However, one should note that operationalizations of the concepts of power or, even more so, of threat, are usually highly ambiguous, there being no consensus in the literature as to which variables or indicator best describes the different dimensions of those terms.

to be relevant by the researcher. Namely, countries in high-density neighborhoods would undergo less pressure from each of their neighbors than states in low-density neighborhoods. On the contrary, leaving the matrix non-standardized would allow an equal influence by each neighboring country, irrespective of density.

How one operationalizes the diffusion variable also matters, as many authors until the first half of the 2000s have tended to focus on democratic diffusion only (from autocracy *towards* anocracy, or from autocracy and anocracy *towards* democracy) and not on processes of regime diffusion *per se*. For example, after taking note that "[p]ossible diffusion effects were found for the analysis of *all* governmental transitions", Starr and Lindborg focus only on transitions towards democracy (Starr and Lindborg 2003).

Another very serious issue is related to any contemporary study that has to do with the measurement of democracy. Leaving aside questions on how to "unpack" some of the most common democracy-score indexes (Pemstein et al. 2010), some studies on democratic diffusion employ the Freedom House scores and consider it as an interval-level variable that may be treated as continuous (Brinks and Coppedge 2006; Lidén 2011).

Finally, most studies do not hesitate to develop and include in the models new (and sometimes exotic) variables. For example, Brinks and Coppedge employ a "superpower influence" dummy and a "measure of global trends" variable. In another study, the authors introduce a "world-system position" variable (taking up the values: periphery; semiperiphery; core) that is straightforwardly derived from dependency theory but assigns core/periphery positions in a static and seemingly arbitrary manner. The same authors introduce a "spatial density" variable, described as "the sum of democratic countries in a world region divided by the total number of countries in the region" (Niemeyer et al. 2008). One is left to wonder whether there is a real necessity to introduce a subjectively-assigned regional position in spatial models that have historically struggled to develop objective measures such as country distances.

To conclude, we can have a look at the models as a whole. Many analyses do not use time-series cross-section methods but simple regressions, so that they might obtain an efficient estimate of their coefficients but their standard errors will certainly be biased towards overconfidence.

A second problem in these models is selection bias. The difficulty in doing a global analysis of changes in democracy for a series of years "lies in sample selection. Most countries simply do not change in most years. (…) Including only the observations with a change raises obvious sample selection issues" (Brinks and Coppedge 2006). To summarize the problem, including all cases risks underestimating the impact of the explanatory variables – mostly so if one hypothesizes a 'triggering domestic condition' that must come into effect in each target country before international spatial pressures can have any effect on the direction of change – but selecting only those cases in which change actually occurs introduces potential selection bias, censoring all international effects that never passed from potential to actual outcome.

To conclude: a number of stumbling blocks is still left in the way of anyone willing to include spatial dependence within regression models. Most of these problems still need to be tackled in any meaningful way, and the solutions at hand are not efficient enough, or we do not know the large-sample properties of some estimators. In general, people trying to include spatial dependence in their models are left with the need to use some "tricks", like generating variables of neighbor influence instead of relying on spatial dependence matrixes. While being already a second-best option in itself, this also generates a problem of endogeneity in the models: spatial regression models account for this endogeneity in the spatial lag or spatial error estimator, but linear or logistic/probit regression models with a "neighbor's dependent value" variable do not. The only way to avoid this endogeneity problem, but still rely upon traditional regression analysis that is unbiased and consistent, would be to resort to some set of regional dummy variables.

Conclusion

In this thesis, I proposed a novel way to look at and analyse political regime types. My analysis moved from the identification of a gap in current research on democratization and autocratic stability, namely that both these literatures tend to disregard intra-autocratic dynamics towards more or less liberal political regimes. This makes theoretical models and empirical tests blind to an important part of the political spectrum, banishing some open autocracies to a middle ground of “hybrid regimes” that fails to explain their resilience. According to my measures, open autocracies today account for almost a third of the global political regime spectrum.

Having acknowledged this gap, I proposed a formal theory that could go beyond democratization and analyse correlates of political liberalization across the board. I developed a game-theoretic model that assumes rational autocrats in search for a more precise signal from their citizens. This allowed me to derive a number of hypotheses over why some countries decide to veer towards a more open autocratic system, while some stick to repressive autocracy, and other still end up democratizing.

I proposed a new way to gauge levels of regime openness, and map this onto a tripartite political space of closed autocracies, open autocracies, and democracies. I improved robustness by setting a number of different thresholds to place autocratic regimes in each category. I also proposed an improvement in measuring political leverage from fuel rents. Finally, in setting up my empirical model, I relied on multiple imputation techniques to minimize bias (doing away with listwise deletion) and maximize robustness (by adding uncertainty, only stronger results remain significant).

My empirical findings showed the usefulness to think about political liberalization as movements both away from and within autocratic regimes. I managed to extend most findings on the correlates of democratization to the analysis of political liberalization, and found that the same correlates are much more robust in a liberalization context, while they tend to lose in significance in an analysis of the correlates of democratization.

In particular, studying political liberalization allowed me to retrieve support both for my and other models' prediction that more liberal regimes should be found at average levels of inequality. Competing theories suggest that they should be found at low levels, and this is unsupported both when looking at political liberalization and at outright democratization.

Another important finding is related to the modernization theory. Namely, I found that while increases in economic well-being tend to be associated with more liberal political regimes, they also tend to be associated with much more resilient political regimes across the board, decreasing the likelihood that a country experiences a political transition in the first place. This is crucial because it brings to the fore the role of agency and chance, and may be useful in explaining recent findings that appear to disprove the modernization hypothesis when employing fixed effects models that only account for within-country variation. A series of other important empirical findings is listed in section 6.5.

My work also faced some crucial challenges, that I leave to future research to attempt to solve. First, by adding a further layer of complexity to empirical models, multiple imputation methods reduce the range of models that one can rely upon at the estimation stage. This is an important weakness, because some models that have been recently proposed and which appear to go beyond fixed effects while partially accounting for the omitted variable bias (see section 6.3) cannot be used in a multiple imputation setting. Further studies are needed in order to show that the large sample properties of these models make them suitable for use in a multiple imputation setting, and how to calculate standard errors through a consistent estimator.

Second, while my measure of political liberalization accounts for some de jure and some de facto conditions of a polity, it only measures them after they occur. While surely the inauguration of a legislature can be traced back to a precise year, other changes may take place over multiple years, such as the discussion and adoption of a new constitution. My measure fails to capture multiple-year processes, and only "fires up" once their outcome is known.

Third, by advancing a proposal to create a tripartite typology of political liberalization levels, I am making the study of transitions from one level to another increasingly more complicated. Transitions become rarer, and the political space becomes more complex – especially in an ordinal variables setting which does not completely trust interval-level analyses. On the opposite, others may be convinced that my measure is not complex enough. It can be argued that political liberalization is an even harder process to describe as compared with democratization. Surely, further studies on different measures of this latent concept are warranted.

Fourth, and related to this, given that my political liberalization measure is novel, I cannot validate it against other indexes. For example, my measure of regime openness is only loosely correlated with the main indexes of democracy (Pearson's correlation values range between 0.55 to 0.67). However, this does not tell me anything about the validity or reliability of my measure.

Finally, and like in most other studies on political regime choice and change, I did not employ empirical methods that allow me to defend the link from correlation to causation, apart from the classical Granger causality method of lagging my independent variables. In the end, while I find a series of crucial findings, these risk to remain confined to the realm of correlates. I do develop a theoretical model in order to justify the inclusion of some covariates within my empirical models, while at the same time other covariates are drawn from the literature: this helps me in the quest for robust findings and linking correlation to causation, but cannot be definite evidence of that. In the end, until today the discipline on political regimes choice and change has lacked valid and useful instrumental variables to exclude endogeneity, while even natural experiments are rare and often teeter at a closer look. Going from correlation to causation is still an open research question that needs to be addressed by the comparative politics literature.

Despite such challenges and weaknesses, I believe my findings can be useful to scholars of international politics and policymakers alike. First, they suggest that national policymakers and the international community should think twice before supporting abrupt democratization attempts, which may have a higher likelihood of failing whenever structural conditions are not there. Sometimes, political liberalization within autocracies could be preferred.

Second, while GDP growth is correlated with changes in political liberalization levels, in fact economic recessions are only found to increase the likelihood of autocratic retrenchment. Policymakers arguing that economic sanctions might persuade the governments of target countries to change policy are advised to take into account that the most likely outcome is for the existing government – or for a different government succeeding it – to veer towards a less liberal political regime. Current sanctions against Russia and Syria (not to mention North Korea) may be a case in point.

Third, findings emphasize the role of agency. Structural relations within a polity do not perfectly determine political regime choice and change, and the same trend can have controversial effects over the likelihood of choosing a more liberal political regime and the likelihood of transitioning in the first place. Therefore, whenever policymakers and the international community are determined into "nudging" a country into a more liberal political regime, they are advised to concentrate over specific actors – e.g. governments in exile, oppositions within and outside the country, the military, etc. – and work with them towards a common objective. The bottom line is: while economic sanctions appear to be the easiest way to coordinate international action against an illiberal government, broader political action, while more costly, has a higher chance to succeed in the longer run.

Fourth, political regime transitions are associated to a higher degree of political violence. Whenever politicians, or the public opinion, side in favour of regime change in illiberal countries, they should be aware that this may not come at a small cost in terms of human lives or infrastructural damage. As conflicts in Syria and Libya that are dragging on since the 2011 Arab Spring show, attempted transitions may degenerate into longstanding violent confrontations.

Finally, as Egypt's autocratic retrenchment shows, any regime transition "resets the clock" of a regime's durability, making it more likely to change again within the next few years. At the same time, both Egypt on the one hand and Tunisia on the other are evidence that, apart from differing structural conditions, the main actors of a polity are the ultimate source of political regime choice and change.

To conclude, I believe that my work makes a series of important contributions to the current literature on political regime choice and change, filling some gaps while leaving other to future research. In particular, to look at political regimes by shifting from a democratization to a political liberalization setting is useful in order to answer questions as to whether and to what extent common causes drive both processes, and points at a potential way forward for a host of new studies.

Our understanding of the political reality through modelling and empirical testing will always be limited, especially in an environment of rare events in which unmodelled agency choices can drive the results. But we, as social scientists, are on a mission to explain political behaviour. Hopefully, this thesis has contributed with a few, small steps at improving the way in which we observe the political world.

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