

**Conflicts dynamics in democratic and autocratic regimes, 1946-2008**

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# 1

## Introduction

### 1.1. OUTLINE OF THE PROBLEM

A cursory look at the worldwide distribution of politically motivated violence suggests that democracy and peace tend to go hand in hand, while nondemocratic countries are those that experience the most severe forms of conflict. Consider, for example, Iraq. Between 1950 and 2008, Iraq experienced at least 11 coup attempts (Powell and Thyne 2011), and 9 intrastate armed conflict broke out in the same period according to the UCDP/PRIO Armed Conflict Dataset. Between 1946 and 2008, Iraq initiated 25 militarized interstate disputes reaching at least Level 4 on the Correlates of War coding scheme,<sup>1</sup> and was involved in such disputes during two thirds of its existence (41 years out of 63). During the same period, the country experienced 27 years of military dictatorship (Cheibub, Gandhi and Vreeland 2010). From 1981 to 2006, it repeatedly reached the lowest score on the CIRI physical integrity rights index<sup>2</sup> (20 years out of 26) and never received a score higher than 3. Needless to say, it did not experience one single year of democratic government since 1946.

By contrast, Switzerland only experienced democracy and peace from 1946 onwards. Since 1946, no coup was attempted and no civil war broke out in Switzerland. The country never initiated a militarized interstate dispute involving the actual use of force and never received a score below 7 on the CIRI physical integrity rights index.

Why, then, are some countries – like Switzerland – so peaceful while others – like Iraq –

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<sup>1</sup> Militarized interstate disputes are coded 1 if they involve no militarized action, 2 if one of the parties issues a threat to use force, 3 if the dispute results in a display of force, 4 if it involves the actual use of military force, and 5 if it results in full-scale war.

<sup>2</sup> The CIRI physical integrity rights index ranges from 0 to 8, higher scores indicating good practices and lower score indicating widespread violations of human rights.

are conflict-ridden? One way of making sense of the situation is to focus on the institutional roots of political violence: The view of institutions as conflict-resolution devices has been prevalent since at least Hobbes' *Leviathan*. While Hobbes called for political centralization, the Federalist No. 9 (Hamilton 1787) called for the creation of a large confederacy of states and a republican government with strong institutional checks and balances as means to deal with the risk of popular insurrection and civil war. Some 200 years later, Popper (1963) famously argued that democracy, by providing the possibility to vote governments out of office, was the only system allowing voters to get rid of bad rulers without bloodshed. This idea was further developed by Przeworski (1999): minimal – Schumpeterian – democracy fosters peace if only because there is a regular procedure for changing governments. More generally, the idea that democracy fosters domestic and international peace has long been widespread among scholars and policy makers. According to this view, it is unsurprising that Switzerland and Iraq differ so strikingly in their conflict behavior: while the former belongs since decades to the world's most democratic countries, the latter has so far only experienced an explosive cocktail of military dictatorship and personal rule. Contrary to the dictators who succeeded each other in Iraq, the Swiss Federal Council, being democratically elected, never needed to imprison or torture its citizens to maintain itself in power, and never had to rely on its military to maintain order at the risk of leaving itself exposed to military intervention in politics.

The “democratic peace” proposition is an influential theme and has inspired much of the American and European democracy promotion efforts abroad (see Mansfield and Snyder 2005). Likewise, the series of political changes that unfolded during the last quarter of the 20<sup>th</sup> century – the fall of the Southern European and Latin American dictatorships in the 1970s and 1980s, the trend towards democratization in Asia shortly thereafter, the collapse of communist regimes at the beginning of the 1990s, and the spread of democracy in sub-Saharan Africa during the same period – has given rise to hope and enthusiasm. Yet, the “third wave” of democracy (Huntington 1993) did not live up to expectations: instead of bringing about global peace, democratization has coincided with a resurgence of both domestic and international conflicts in many parts of the world (see, e.g., Mansfield and Snyder 2005; Collier 2009). Some forty years after the beginning of the third wave, the consensus among scholars is now that democracy generally did not fulfill its promise:

although the interstate democratic peace is still a well-established proposition in the international relations literature, it has become increasingly challenged in recent years (see, e.g., Thompson 1996; Rosato 2003; Gibler 2007). Furthermore, the empirical evidence in favor of the domestic variant of the democratic peace theory is meager at best: a recent review (Hegre 2014) indicates that very few studies so far have confirmed the existence of a monotonic relationship between democracy and the risk of intrastate armed conflict.

There are several possible reasons why cross-national research has failed so far to uncover any clear relationship between democracy and peace. One of these explanations is that dictatorship, war, and domestic conflict are only symptoms of a generalized disease whose roots can be located in political culture, economic backwardness, or country-specific historical legacies. If this view is correct, political institutions are only epiphenomena of deeper conflicts, and democracy – provided that it can emerge and survive in such unfavorable environments in the first place – is clearly not sufficient to bring about peace. A more nuanced view holds that democracy alone is not sufficient to prevent conflicts, because it can have detrimental effects on political stability in poor or severely divided societies (Horowitz 1993; Collier 2009; Hegre and Nome 2010). Finally, it could be the case that only some democratic institutions, or some forms of democracy, are conducive to social peace (Reynal-Querol 2002a, 2005; Schneider and Wiesehomeier 2008).

But maybe there is a simpler explanation: comparing democracies and nondemocracies is bound to fail because it is too simplistic and ignores the huge diversity of authoritarian regimes (traditionally treated as a residual category encompassing all systems that are not democratic). As recent research suggests, there are perhaps as many differences between two “types” of nondemocracies as there are between democracies and dictatorships: authoritarian regimes vary with regard to their degree of openness, the way rulers access, maintain, exercise and leave power, the type of organization on which they rely, and the institutions they create (see, e.g., Geddes 1999; Hadenius and Teorell 2007a; Gandhi 2008a; Goemans 2008; Cheibub et al. 2010; Svobik 2012). While there is still no consensus as to which of these aspects matters the most – research on authoritarianism is still a young field – there are good reasons to believe that at least some of these are relevant to predict states’ conflict behavior.

Authoritarian regimes have regained attention during the last two decades, and the

proliferation of cross-national datasets on authoritarian institutions has enabled researchers to examine their effect on a wide range of outcomes. A non-negligible part of these empirical investigations deals with political conflicts involving the use of physical violence: are violent conflicts unrelated to authoritarian regime type, or are they more prevalent in some dictatorships<sup>3</sup> than in others? After some fifteen years, it seems that comparative cross-national research has already succeeded in identifying the “culprits:” military regimes have been found to be more unstable (Geddes 1999; Hadenius and Teorell 2007a), to engage more often in physical integrity rights violations than other types of dictatorships (Poe, Tate and Keith 1999; Davenport 2007b), to initiate more international conflicts (Sechser 2004; Lai and Slater 2006; Weeks 2012), to experience more domestic and transnational terrorism (Wilson and Piazza 2013) and to be more prone to coups (Geddes 1999; Geddes et al. 2014) and civil wars (Fjelde 2010; Wilson 2014). Regimes labeled as “personalist” (Geddes 1999) do not fare much better, since they are also more likely to experience civil war (Gurses and Mason 2010) and to pursue aggressive foreign policies (Peceny, Beer and Sanchez-Terry 2002; Peceny and Beer 2003; Peceny and Butler 2004). By contrast, party regimes display a superior intra-elite cohesion and last longer (Geddes 1999), are less likely to experience internal violence (Gurses and Mason 2010; Fjelde 2010; Wilson and Piazza 2013) and also less likely to initiate external conflicts (Peceny et al. 2002; Peceny and Beer 2003; Peceny and Butler 2004). Finally, while monarchies have been comparatively neglected by cross-national research so far, they are clearly among the most stable (Hadenius and Teorell 2007a) and seem to be among the most internally peaceful (e.g., Gurses and Mason 2010).

At first glance, this accumulation of findings is impressive. Yet, researchers have often tended to investigate these outcomes individually, in isolation. Doing so, they have ignored two crucial aspects of the problem. First, regimes do not emerge in a vacuum. They are embedded in specific contexts and shaped by the very security issues they have to face. To give the most obvious example, researchers have repeatedly noted that military regimes most frequently emerge in periods of serious domestic crises (e.g. Finer 1962; Nordlinger 1977; and, more recently, Acemoglu, Ticchi and Vindigni 2008): it is thus likely that the

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<sup>3</sup> The terms “dictatorship”, “authoritarian regime” and “autocracy” will be used interchangeably throughout the dissertation and refer to all nondemocratic regimes (including monarchies and so-called “hybrid” or “anocratic” regimes).

context in which these regimes emerge significantly affects their subsequent behavior. Second, investigating civil wars, military coups, diversionary wars, or repression separately is tantamount to assuming that these forms of political violence are independent from each other, and ignoring that they actually often tend to go hand in hand. For example, as recent research has demonstrated, experiencing internal armed conflict makes regime more repressive (Davenport 2007a), more likely to get involved in an interstate war (Gleditsch, Salehyan and Schultz 2008), and more vulnerable to military coups (Piplani and Talmadge 2015). In turn, coup risk affects states' propensity to initiate conflicts abroad (Powell 2014b), the involvement in an interstate conflict affects repression (Davenport 2007b), and so on. To further complicate matters, these forms of violence might be theoretically distinct, but difficult to differentiate empirically: for example, most operational definitions of civil war rely on some threshold of violence, but this criterion might result in the inclusion of other types of phenomena (e.g., state-sponsored killings, or military coups) in civil war databases (see Sambanis 2004, 2008). Finally, some researchers have raised the question of whether manifestations of political violence that are usually considered in isolation really are inherently different phenomena – the most obvious example being the differentiation of civil wars and domestic terrorism (see, e.g., de la Calle and Sanchez-Cuenca 2012; Sambanis 2008). Thus, one is left to wonder if what appears to be an impressive sum of findings cannot be, in fact, traced back to a small number of ambiguous events, or does not result from the fact that researchers so far have drawn arbitrary boundaries between phenomena that are actually similar in their nature and consequences.

To phrase the problem in a more technical way, works devoted to the relationship between political institutions and political violence face a number of issues:

- (1) The problem of *measurement error*: do operational definitions of theoretical constructs and coding issues result in measurement bias?
- (2) The problem of *spuriousness*: are political regimes simply epiphenomena of deeper factors that might simultaneously affect political institutions and political conflicts?
- (3) The problem of *endogeneity*: does regime type affect violence, or is it the other way around?

(4) The problem of the *disentanglement of direct and indirect causal relationships*: does regime type cause violence directly or only through unobserved, intervening factors?

While the first problem – measurement error – is methodological in essence, the three others call for a unified approach to political regimes and violent conflict: because identifying how forms of political violence influence each other and how they shape political structures is a theory-driven process, it requires having a broad overview of the mutual relationships between political institutions and conflicts. Of course, such an approach is not without limitations: such an analysis cannot be exhaustive – some aspects of the problem will necessarily be left out of the picture – and at the same time, given its broad scope, the argumentation might become somewhat abstract (some would say oversimplistic). Yet, precisely because of its general, abstract character, such an approach is well-suited to isolate institutional causes of political violence, rule out alternative explanations, and avoid some of the potential pitfalls mentioned above: it can therefore allow for a more thorough understanding of the complex relationships between institutions and violence.

The aim of the dissertation will therefore be to provide a theoretical and empirical contribution to the growing literature that examines the impact of political regime type on the outbreak of political violence. It will lay down a theoretical framework that will be applicable to a wide range of cases and will thus facilitate cross-country comparisons and help anticipate risks in particular cases. It will then reexamine some of the causal relationships uncovered by previous research, and explore new ones. It will, finally, bridge some gaps between different research fields – particularly comparative politics and international relations – that often tend to evolve in relative isolation from each other.

Next sections specify the subject of investigation and provide some details on the approach adopted throughout the dissertation.

## **1.2. AIM OF THE DISSERTATION**

Broadly speaking, the explanandum of the dissertation is the actual or threatened use of armed violence for solving politically motivated conflicts. This rather broad definition

encompasses a wide range of outcomes: It includes forms of violence perpetrated by state actors (e.g., purges, genocides or politicides) or against state actors (e.g., assassinations or coups d'état) as well as violence initiated by, or targeting foreign actors (e.g., interstate wars). Theoretically speaking, most of these aspects of political violence are somehow related to extant political institutions. In practice, however, the scope of the empirical analysis will be restricted to following outcomes:<sup>4</sup>

- (1) The outbreak of intrastate armed conflict (Chapters 5, 6, and 7).
- (2) Military coups d'état (Chapters 4, 5 and 8)
- (3) State repression, conceived as physical integrity rights violations perpetrated by state actors (Chapter 8).
- (4) The initiation of interstate armed conflicts for diversion purposes (Chapter 9).

These outcomes correspond to several possible manifestations of conflicts taking place between three main groups of actors, namely the incumbent and his allies, the military, and opposition groups. State repression and intrastate armed conflict are, for example, two possible outcomes of conflicts between the government and opposition groups. Military coups are conceived either as direct conflicts between the government and the military (in which case the military is the initiator of the conflict) or as the outcome of an agency problem arising from conflicts between government and opposition (in which case the government is conceived as the principal and the military as an agent). Finally, diversionary wars are attempts by the incumbent to avoid open conflicts with the military. The central assumption of the dissertation is that the behavior of these actors and their mutual relationships (and, in turn, the way conflicts between them will manifest themselves) are shaped by the political environment in which they operate. This political environment, in turn, consists of two main features: 1) the identity of incumbents and of the political coalition supporting them; and 2) the formal rules that regulate access to, and exit from political office. These two features constitute the explanans of the thesis, that is, political regimes.

Political regimes are sorted into five broadly defined types. The first two types – democracies and monarchies – are part of a broader category I will refer to as the group of “institutionalized regimes.” Although democracies and monarchies differ in every respect,

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<sup>4</sup> Details on the definition of these variables will be provided in the corresponding chapters.

what they have in common is the existence of binding, agreed-upon rules that prescribe how the effective leader is selected, removed, and replaced. Regimes that lack such rules are referred to as “non-institutionalized regimes”: this category encompasses all regimes in which, for example, the leader has attained power by irregular means, or in which succession in office involves unregulated power struggles at the elite level. Theoretically speaking, there are institutional ways of filling political offices other than competitive elections and hereditary succession; yet, in practice – since the analysis is restricted to the post-World War II period – only democracies and monarchies are considered institutionalized regimes, and the category of non-institutionalized regimes encompasses all regimes that are neither monarchies nor democracies.

Non-institutionalized regimes, in turn, are sorted into three subtypes, namely party regimes, military dictatorships, and civilian autocracies. This approach speaks directly to the “new generation” of studies on authoritarianism (see, e.g., Geddes 1999; Magaloni 2008; Cheibub et al. 2010) which main focus lies on the autocrat and on the political alliances that support him – variously referred to as “launching organizations” (Haber 2006), “ruling elites” (Cheibub et al. 2010) or “ruling coalitions” (Magaloni 2008, Svulik 2012). Following this approach, authoritarian regimes are differentiated according to the identity of the individuals who are part of this ruling coalition (one of the most common distinctions being the civilian vs. military dichotomy). As we shall see in Chapter 2, there are alternative ways of conceptualizing authoritarian regimes. The main reason why I opt for this approach instead of another is that it was precisely designed to predict the stability of political regimes: most studies mentioned above implicitly or explicitly convey expectations about the form political conflicts are most likely to take in each regime. This conflict potential, in turn, helps predict how long autocrats will be able to stay in power and how long regimes will last. Thus, this approach fits perfectly to the general research question of the dissertation, since conflicts investigated here broadly correspond to the many ways in which autocrats can lose power (e.g., coups, rebellions or foreign interventions; see Goemans et al. 2009a, Svulik 2012). At the same time, the present work has the potential to contribute to extant research on the stability of authoritarian regimes in a meaningful way, to the extent that it will address a weakness of this research agenda: so far, most studies on the stability of authoritarian regimes have precisely focused on most

extreme forms of political instability – or ultimate consequences of conflicts – such as regime transitions or irregular leadership changes (for typical examples, see Hadenius and Teorell 2007a; Gandhi 2008a; Magaloni 2008; Svobik 2012). This narrow focus on political survival can potentially mask relevant empirical relationships (because, for example, countries that experience many civil wars the incumbent always wins are treated in the same way as countries that never experience civil war) and rely on a rather broad conception of stability: to take an extreme example, although Bashar al-Assad is (as of May 2015) still in power, nobody would claim that Syria is as stable as, say, Saudi Arabia. To put it otherwise, political violence can either be viewed as a cause or as a symptom of instability (or, alternatively, as a failure to solve conflicts in a peaceful way). Either way, restricting the analysis to “positive” cases (i.e., cases in which a violent conflict has taken place between the autocrat and someone else, and the autocrat has lost) can result in a distorted view of the effect of political institutions.

### **1.3. THE APPROACH**

#### **1.3.1. Theoretical approach**

The dissertation adopts a rational choice approach. Rational choice theories begin with three key assumptions (see Riker 1995):

- (1) Actors have well-defined preferences and are able to rank these in a transitive order;
- (2) They hold specific beliefs about the consequences of their actions;
- (3) They act purposively, depending on their ordering of preferences: given the choice between several actions, they will choose the one they believe will achieve their goals.

Several important caveats are in order. First, the third assumption does not require that individuals be able to take perfectly informed decisions, never make mistakes, or never choose ineffective strategies: it merely states that they are able to act according to their own goals and that means are related to ends in a non-random way (Riker 1995; see also Quackenbush 2004 for a distinction between procedural and instrumental rationality).

Second, the term of rationality only applies to the process through which individuals choose an action: that is, rationality only implies that individuals will choose the strategy they believe will lead to their preferred outcome, and says absolutely nothing about what this outcome precisely is. To put it differently, rational choice approaches usually do not specify any particular goal as universal, and no preference can be described as inherently rational or irrational (see Green 2002 for a discussion about Humean – instrumental – and Kantian rationality; see also Riker 1995, Quackenbush 2004). Third, the term “rational choice” only pertains to decisions about specific actions: neither desires nor beliefs are freely chosen.

To illustrate these points, the rationality assumption – narrowly defined as it is – is perfectly compatible with all possible journalistic accounts of dictators’ eccentricities. Examples of the latter abound: Turkmen President Niyazov was famous for renaming days and months after himself and his relatives, replacing the Turkmen word for bread after his mother’s name,<sup>5</sup> and outlawing beards and opera.<sup>6</sup> The Nationality Law issued by Mobutu in Zaire required citizens with a foreign-sounding name to change it to a Zairian one (Nordlinger 1977, 132). During his official visit to France, Libyan ruler Qaddafi received his guests in a Bedouin tent he erected in the garden of the official residence for visiting dignitaries.<sup>7</sup> General Ne Win of Burma, following a recommendation of his astronomer, ordered the withdrawal of several Burmese banknotes and issued two new denominations—forty-five and ninety—that are divisible by nine and whose numerals add up to nine (which resulted in people’s life savings being wiped out overnight).<sup>8</sup> The list goes on. But, as long as we do not have any information about the ultimate goals underlying Niyazov’s or Qaddafi’s actions, none of these examples constitute a rebuttal of the rationality assumption.

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<sup>5</sup> Osborne, Andrew. 2006. “Saparmurat Niyazov: President of Turkmenistan.” *Independent*, December 22, 2006, available at <http://www.independent.co.uk/news/obituaries/saparmurat-niyazov-429556.html> (accessed March 25, 2015).

<sup>6</sup> Whitlock, Monica. 2004. “Young Turkmen face beard ban.” BBC, February 25, 2004, available at <http://news.bbc.co.uk/2/hi/asia-pacific/3486776.stm> (accessed March 25, 2015).

<sup>7</sup> Sciolino, Elaine. 2007. “Divided, France welcomes and condemns Qaddafi.” *New York Times*, December 11, 2007, available at [http://www.nytimes.com/2007/12/11/world/europe/11france.html?\\_r=0](http://www.nytimes.com/2007/12/11/world/europe/11france.html?_r=0) (accessed March 25, 2015).

<sup>8</sup> Packer, George. 2008. “Drowning: Can the Burmese people rescue themselves?” *New Yorker*, August 25, 2008, available at <http://www.newyorker.com/magazine/2008/08/25/drowning?currentPage=all> (accessed March 30, 2015).

Thus, the rationality assumption is taken here as given: actors want to achieve specific goals and will choose the course of action that they believe is the most likely to lead to the preferred outcome. But then again, the main problem is that this assumption says nothing about preferences themselves. A necessary first step therefore consists in identifying these preferences.

A first way of gathering information about what motivates actors is to ask them. But there are reasons to think that, given the sensitiveness of the subjects investigated here, this approach is not the most appropriate. In the context of political struggles that involve the use of violence, the public discourse tends to be plagued by self-serving explanations that have little to do with actors' real motivations: dictators want to lead their country to democracy and development, rebel leaders want to fight injustice, and coup plotters want to save their countries from social turmoil or from the influence of incompetent and corrupt politicians. While these self-justifications can be really informative about the political or cultural context in which they are uttered,<sup>9</sup> they say little about the primary motivations of actors. For example, here is how the then warlord Charles Taylor justified the bloody armed struggle he initiated against Liberian dictator Samuel Doe:<sup>10</sup>

Did [Doe] ever do anything for the people? Did he ever respect human rights? He destroyed everything, the economy, private property, social and political life. (...) I do not want to establish a military government in Liberia, but a civilian, democratic regime, with a social and economic program to develop the country and bring back children who fled to neighboring countries and elsewhere. I want to guarantee free talk and press freedom, an independent judiciary, and lead the Liberian people to progress while respecting human rights. (...) Furthermore, I will not ban any opposition party. The Liberian people can trust me.

Whether or not Taylor believed in his own propaganda is beside the point (even though the way he behaved once in power makes this statement hardly credible in retrospect):<sup>11</sup> the

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<sup>9</sup> See, for example, Luttwak's (1979) comments on post-coups announcements, pp. 159-162.

<sup>10</sup> For the complete interview (in French), see Diallo, Miriam C. 2013 [1990]. "Liberia: Charles Taylor, itinéraire d'un tueur (#3): 'Seul Houphouët-Boigny m'a aidé'." *Jeune Afrique* 1542, July 18, 1990, available at <http://www.jeuneafrique.com/Articles/Dossier/ARTJAWEB20120422125832/libye-liberia-charles-taylor-samuel-doecharles-taylor-seul-houphouet-boigny-m-a-aide-3.html> (accessed August 31, 2014).

<sup>11</sup> In 2012, Taylor was charged for war crimes and crimes against humanity, and sentenced to 50 years in prison by the Special Court for Sierra Leone, thus becoming the only former head of state to be convicted

real problem is that it is hard – to say the least – to derive any clear predictions from such appeals to vague ideological principles. But how, then, can preferences be identified? An alternative solution to the problem is proposed by Riker (1995):

The simple identification of goals is the crux of the problem. The theorist can neither believe what actors say are their goals nor trust his or her own attributions of them. Many people hide aggrandizement under the cloak of self-proclaimed altruism. The theorist may be gullible or cynical or both. (...) So it is necessary to work back and forth among outcomes, actions, and intentions to discover goals people actually have. The procedure runs as follows: The scientist assumes goals for relevant subjects and then predicts choices and outcomes on basis of assumed goals. If the scientist then observes that predicted actions and outcomes occur, he or she has in fact explained the outcomes and the research is complete. If, however, the scientist fails to observe the predicted outcomes and actions, then he or she must reconsider the attribution of goals. With a new attribution, a new prediction, if observed, completes the research (Riker 1995, 25-6).

Therefore, the first step of the analysis will consist in specifying some underlying assumptions about who the relevant actors are, what their preferences are, and how the environment in which they operate shapes and constrains their behavior. This will be the objective of Chapter 3. As noted above, relevant actors are the incumbents and their ruling coalition, opposition leaders, and military commanders: all violent political struggles analyzed here result from conflicts between these groups of actors. Political regimes constitute the immediate environment in which these actors operate, and are expected to shape the interactions between them and thus, ultimately, the nature of political conflicts. In addition, Chapter 3 will specify the preferences of each of these groups. The assumptions I will make are not inherently provocative: in short, incumbents want to stay in office; higher regime officials would like to replace the current incumbent but what they want above all is to avoid being removed from their current position; military commanders likewise want to keep their jobs; the only ones who have a strong preference against the status quo are opposition leaders, who want the incumbent and his ruling coalition out of office and want to maximize their own political influence. Yet, although these assumptions

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for war crimes by an international court since World War II. See Bowcott, Owen. 2013. "War criminal Charles Taylor to serve 50-year sentence in British prison." *Guardian*, October 10, 2013, available at <http://www.theguardian.com/world/2013/oct/10/former-liberian-president-charles-taylor-british-prison> (accessed May 15, 2015).

seem self-evident, they entail some crucial implications about the role of political institutions in mitigating conflicts.

While Chapter 3 is essentially theoretical, the remainder of the thesis will be devoted to the testing of hypotheses which are derived from this theoretical framework. The results of the empirical analysis, in turn, will provide indications about the validity of the assumptions made in Chapter 3: assumptions about actors' preferences will allow for the deduction of empirically testable expectations about the relationships between certain causes and outcomes. If outcomes follow some regular empirical pattern, this implies that preferences are not randomly distributed among actors and that their behavior is at least partly predictable.<sup>12</sup>

Next section gives an overview of the methodological approach and the empirical strategy.

### **1.3.2. Methodological approach**

Given that the aim of the dissertation is to provide a general explanation for the outcomes investigated here, the testing will rely on statistical analysis. Differences between qualitative and quantitative research are increasingly viewed as purely technical, as recent calls for the integration of statistical and qualitative methods (see, e.g., Lieberman 2005; Brady et al. 2006; Fearon and Laitin 2008) and the development of mixed methods designs attest. Yet, qualitative and quantitative methods are not merely interchangeable "tools," for the choice of a methodology is rooted in specific ontological and epistemological

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<sup>12</sup> Cross-national research on assassinations (i.e., politically motivated killings) provides a nice illustration of this logic of inquiry. Assassinations of political leaders are a particularly telling case because they have long been treated as highly idiosyncratic, random events, which have to be traced back to the psychology of perpetrators (for a review of prior works on assassinations, see Iqbal and Zorn 2006, 489-90). In their dataset of political leaders, Goemans, Gleditsch and Chiozza (2009b) treat assassinations as a distinct category instead of merging them with, e.g., coups, arguing that it is difficult to assert whether assassinations have a clear political motive. Yet, a recent wave of comparative studies on assassinations of political leaders have demonstrated that these events do not occur randomly and they cannot be analyzed in isolation from the political context: There is, for example, growing evidence that constraints on the chief executive's power and the extent to which succession in power is regulated impact both the frequency of assassinations and their consequences for the long-term stability of the regime (Iqbal and Zorn 2006, 2008; Jones and Olken 2009; Torgler and Frey 2013). The main lesson that can be drawn from these findings is that, although it may indeed be difficult to identify the ultimate cause of individual assassinations by simply observing the perpetrators, the overall pattern in the distribution of assassinations implies that these are indeed politically motivated. This conclusion, in turn, makes it possible to generate further predictions about which politicians run a high risk of being assassinated and what institutional designs are conducive to more stability.

positions.

The causal ontology that underlies the empirical analysis that will be undertaken in this dissertation is “Humean,” that is, empiricist and regularist: the basic idea is that scientific explanations (whether or not they explicitly involve a statement about causation) always somehow imply law-like relationships (see Hempel 1965). Even singular causal explanations (e.g., the 1991 civil war in Algeria broke out because the legislative elections were canceled) imply the existence of (or are a single instance of) a corresponding regularity linking cause to effect (e.g., canceling elections leads to rebellion). In turn, the existence of the law-like relationships that underlie singular causal explanations can only be assessed to the extent that these relationships occur with a certain degree of regularity: in other words, empirical covariations are not sufficient for scientific explanation, but they are necessary.<sup>13</sup> The choice of a statistical analysis instead of, for example, case study-based qualitative methods follows from these epistemological positions. Inferential statistics are well-suited to uncovering empirical regularities for two reasons: first, identifying regularities across cases makes it possible to distinguish between general empirical associations and accidental co-occurrences. Second and relatedly, multiple regression analysis can deal with the simultaneous occurrence of several empirical regularities and makes it possible to isolate individual causes and to identify the ones responsible for the occurrence of the outcome of interest (Aneshensel 2013, Chap. 1).

The empirical analysis will be based on a cross-national time-series dataset, with the country-year as the unit of analysis. Data on the independent variables – that is, political regime types – are available for 199 countries for the period from 1946 to 2008. Yet, the sample size as well as the coverage will vary substantially depending on the availability of the dependent and control variables (details will be provided in each chapter). In particular, data on variables such as GDP per capita or population is especially sparse in the poorest and most closed dictatorships – particularly in earlier years – and generally unavailable for countries with fewer than one million inhabitants.

The empirical analysis will predominantly rely on logistic regression, which is used to predict the odds of a case being a positive case based on the value of the predictors. Odds

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<sup>13</sup> It is obviously a very crude and incomplete way of summarizing this approach. For an overview of current discussions about the logic of scientific explanation, see Woodward 2011.

give the probability that an observation is a positive case divided by the probability that it is a negative case, and range from 0 to positive infinity. Odds ratios, in turn, are defined as the ratio of the odds that an outcome will occur given an exposure to a certain predictor variable, compared to the odds of the outcome occurring in the absence of that exposure (that is, how the odds that the event occurs change with a one-unit change in the predictor when all other predictors are held constant). Odds ratios also range from 0 to positive infinity, values comprised between 0 and 1 indicating that the predictor variable has a negative effect on the probability of the event occurring, while values higher than 1 indicate a positive effect of the predictor variable.

Thus, the aim of logistic regression is to estimate the probability of an event occurring given the values of the independent variables: the whole empirical analysis is aimed at observing how political institutions affect the probabilities of a violent conflict breaking out. For example, Chapter 4 will investigate whether the risk of interstate conflict increases or decreases the probability of a military coup; likewise, Chapter 8 will investigate whether military governments are more or less likely to violate physical integrity rights than civilian governments. In spite of this reliance on probabilities, it should be noted that I do not make any assumption as to whether the uncovered empirical relationships are deterministic or probabilistic in nature: a deterministic “law” can appear to be probabilistic simply because there is a large number of conditioning or intervening variables (which can be related to the outcome of interest in a deterministic way as well) that can alter the observed relationship between cause and effects but cannot be modeled properly (either because they are unknown or because there is a limit to the number of variables a statistical model can include); alternatively, the “real world” can be stochastic for a large part. I am agnostic as to which of these interpretations is correct.<sup>14</sup>

Some important caveats are in order. Many of the outcomes analyzed here – such as civil wars, coups, or interstate wars – are comparatively rare events. Yet, rare events are notoriously difficult to explain and predict: logistic regression tends to underestimate the

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<sup>14</sup> To take the example of the Algerian civil war, let us assume that cancelled elections lead to civil war (with a 100% probability) if these elections are the first competitive elections the country has yet experienced and if per capita income does not exceed a certain threshold. If only 50% of the cases of cancelled elections satisfy these two conditions, testing the relationship between cancelled elections and civil war without taking both conditioning variables into account will yield the result that cancelled elections lead to civil war with a 50% probability. This does not mean, however, that the relationship is not deterministic.

probability of positive outcomes when these occur with a low (absolute) frequency (King and Zeng 2001). Relatedly, the rate of correctly classified cases tends to decrease as the events to non-events imbalance ratio gets larger, since logistic regression tends to classify observations into the larger outcome group (O’Connell and Amico 2010).<sup>15</sup> Conversely, when rare-event data are analyzed over a short period of time, models can easily become overfit, i.e., fit too closely to the idiosyncrasies of the sample at hand and generalize poorly to new data (for a detailed and nontechnical overview on overfitting, see Silver 2012, Chap. 3). Related to these two problems – that is, the risk of both Type I and Type II errors – are several model specification issues – e.g., the fact that, in logistic regression, the maximal number of control variables that can be included in the model without distorting the results depends on the number of observations in the smaller response group (O’Connell and Amico 2010).<sup>16</sup> Added to the difficulties related to the sample size are more general issues related to the analysis of time-series cross-sectional data (e.g., possible measurement error and nonrandomly missing data). Given these potential sources of error, all empirical findings presented throughout the dissertation should be taken as preliminary. This does not mean that they are worthless: taken as a whole, the results point to some clear tendencies. Yet, individual estimates have to be considered with a reasonable amount of caution.

Finally, it should be kept in mind that the empirical analysis is not aimed at predicting conflicts with accuracy;<sup>17</sup> neither do I seek to offer a comprehensive explanation of the outcomes investigated.<sup>18</sup> The analysis will be restricted to the goals that have been specified above, namely the examination of empirical relationships between political regimes and violent conflicts. Political institutions and regimes – which are the main focus of this dissertation – are only one among possibly hundreds of determinants of political

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<sup>15</sup> These are different problems since the first one is related to the count, and the second one to the percentage of positive cases

<sup>16</sup> Unfortunately, there is no consensus about what exactly this maximal number of control variables is: a rule of thumb is that at least ten positive outcomes per predictor variable are necessary, but this rule is by no means consensual (see, e.g., Achen 2002; Spicer 2004; Vittinghoff and McCulloch 2007).

<sup>17</sup> It is not aimed at prediction whatsoever.

<sup>18</sup> There is a practical implication for the empirical analysis. If the purpose of the statistical test is to assess the validity of a hypothesis by ruling out alternative explanations and verifying that the relationship is not spurious, it makes little sense to include as control any variable possibly related to the outcome of interest. Control variables should only be included if there is a (theoretical) reason to believe that their omission could bias the analysis of the relationship between main predictors and outcome variable (see Ray 2003).

violence. They can even plausibly be expected to exert only a marginal influence on the dependent variables. Yet, to the extent that institutions – contrary to, say, culture or historical legacies – can be modified by human agency, they are still worth being analyzed.

#### **1.4. ORGANIZATION OF THE DISSERTATION**

The remainder of the dissertation begins with Chapter 2, in which I will clarify what I mean by “political institution” and “authoritarian regime type.” In addition, Chapter 2 operates a synthesis of prior research on authoritarianism and proposes a classification of democratic and nondemocratic regimes, from which two main arguments are derived. First, only democracies – which rely on competitive elections – and monarchies – which rely on hereditary succession – can be viewed as institutionalized regimes, i.e., polities that rely on predictable, binding rules regarding the access to, and succession in power; in the remainder of political regimes (which I label non-institutionalized), political conflicts surrounding these two issues are typically solved with violence. The second argument is that democracies and monarchies are the result of a gradual evolution of domestic political structures, which did not necessarily involve the violent removal of previous elites from power; by contrast, non-institutionalized regimes – and particularly military dictatorships and party regimes – mostly emerged in a political vacuum or were imposed by force and involved the irregular removal of prior rulers from power.

Chapter 3 deals with the nature of political conflicts in the two resulting broad categories of political regimes (that is, institutionalized and non-institutionalized regimes). First, some crucial assumptions about relevant actors, their preferences, and their behavior will be specified. It will then be discussed how political institutions have the potential to mitigate conflicts of interest and allow for their peaceful resolution. Finally, some expectations that are relevant for the remainder of the dissertation will be discussed: the main implications of this discussion are, first, that the nature of political conflicts is fundamentally different in institutionalized and non-institutionalized regimes; and, second, that specific institutions have distinct effects depending on the type of conflict.

The remainder of the dissertation is organized in a somewhat unconventional fashion:

Rather than outlining the hypotheses at the end of Chapter 3 and then moving on to describe the data, methods, and results in consecutive chapters, I decided instead to treat each aspect of the topic in a separate chapter. Although all working hypotheses are derived from the same conceptual and theoretical framework, all empirical chapters can be read independently from each other, and each of them will include – in addition to the empirical analysis – a brief review of the relevant literature and a section devoted to the discussion of theoretical expectations.

The empirical chapters of the dissertation are organized into three main parts: the first part analyzes regimes as the *product* of violent conflicts; the second part treats regimes as *targets* of violent conflicts; finally, the third part investigates regimes' propensity to *initiate* violent conflicts.

With regard to the first part, Chapter 4 investigates the distinctive conditions in which political regimes emerge. It analyzes how internal and external security threats (broadly understood as the latent risk of intrastate and interstate armed conflict, respectively) impact the military's propensity to get involved in domestic politics and how military interventions, in turn, shape political structures. I find that military coups become more likely as the intensity of internal security threats increases: the higher the risk of civil war, the higher the risk of military coups. By contrast, the relationship between external security threats and military intervention in politics is curvilinear: the probability of a military coup first increases and then decreases as the risk of interstate conflict increases. On the long run, these patterns of military activity profoundly affect political systems: democracies are most likely to endure when the military is weak or virtually inexistent; single-party dictatorships are most likely to emerge when the military is strong but less prone to intervention in domestic politics; finally, countries with a politically active military are characterized by chronic political instability and intermittent periods of military rule.

The second part of the dissertation is devoted to the two most common threats each regime must face, namely rebellions and military coups. Chapter 5 investigates whether some political regimes are more likely to experience civil wars on the one hand and military coups on the other hand. It also re-examines one recent finding, that authoritarian regime types differ systematically with regard to their risk of civil war. I do not find any support

for this argument, however: non-institutionalized regimes all face a similar risk of civil war. By contrast, democracy – because it is, as Popper (1963) and Przeworski (1999) stated, the only regime that allows for regular, peaceful alternations in power – is associated with a consistently lower risk of domestic conflict, and I also find some tentative evidence that monarchies are internally more peaceful than other nondemocracies. Finally, while authoritarian regime type does not affect the risk of civil war, it is a strong and robust predictor of military coups. To the extent that prior research has uncovered a relationship between authoritarian regime type and civil war, I argue that this relationship is due to measurement error on the side of the dependent variable: it merely results from the inclusion of military coups in commonly used datasets on intrastate armed conflict.

Chapters 6 and 7 examine two further consequences of Przeworski's (1999) argument, that the possibility of changing governments is sufficient to prevent civil war. Chapter 6 investigates whether, in systems in which there is no regular procedure to dismiss the leader, gerontocracy produces social peace. The underlying argument is that, in the absence of formal limits to incumbency, alternation in power occurs either through the leader's death or a revolution: since old leaders can be expected to leave office reasonably soon, dissatisfied opposition groups then prefer to wait rather than to rebel. However, this can only happen when the dictator's launching organization is not able to replace him after his death. I find that, in civilian dictatorships, the risk of intrastate armed conflict decreases as the dictator gets older. Chapter 7 is devoted to the impact of electoral competition on the risk of rebellion, and analyzes the effects of parliamentary elections outcomes in all regimes that hold such elections. The main argument is that allowing opposition parties to compete in elections and win a large share of the votes can be profoundly destabilizing, unless these parties are granted the possibility to influence political decisions and/or remove the leader from power: in short, rebellion occurs when opposition parties' electoral support does not correspond to their political power under current institutions.

Finally, the third part of the dissertation investigates whether some regimes are more likely to make a systematic use of violence as a means of solving political conflicts. This question echoes the recent discussions on the power-maintenance strategies authoritarian leaders frequently use in order to ensure their political survival. Some of these strategies,

such as cooptation (see, e.g., Gandhi 2008a; Gandhi and Przeworski 2006, 2007; Arriola 2009), or, to a certain extent, coup-proofing (see, e.g., Quinlivan 1999; Belkin and Schofer 2003; Roessler 2011; Pilster and Böhmelt 2011, 2012) are nonviolent, but some others involve the use of physical violence. Chapters 8 and 9 will examine two particular forms of violent power-maintenance strategies, namely repression and the initiation of diversionary wars, respectively. State repression – understood here as the repression of rights to physical integrity perpetrated by state actors – is targeted at opposition groups. The initiation of diversionary wars is commonly thought of as a reaction of incumbents to the loss of public support, but one of the central arguments made in Chapter 9 is that diversion, in fact, can be best conceived as authoritarian leaders' reaction to threats posed by the military. Thus, we should expect these two forms of state violence to depend primarily on the intensity of threats originating from regime opponents or the armed forces, and to be unrelated to regime type. Yet, many scholars see it otherwise. A common argument in these two fields of research is that military officers – because of parochial interests, or because they hold specific beliefs about the desirability and usefulness of military force – generally tend to favor the use of violence over more peaceful means of solving conflict (on repression, see Poe et al. 1999; Davenport 2007b; on the initiation of interstate conflicts, see Dassel 1998; Dassel and Reinhardt 1999; Sechser 2004; Weeks 2012). If this view is correct, military dictatorships can be expected to make a more extensive use of repression and diversion. Yet, findings in Chapters 8 and 9 indicate that these arguments are not borne out. First, military dictatorships are not more likely to repress physical integrity rights (Chapter 8). Moreover, widespread physical integrity rights violations leave regimes exposed to a higher risk of military defections and increase the risk of coups. Second, while military dictatorships are indeed more likely to initiate militarized interstate disputes, this relationship is entirely driven by coup risk (Chapter 9): everything else being equal, the military's influence on domestic politics decreases the risk that a country initiates an interstate conflict. Finally, Chapters 8 and 9 further highlight the pacifying effects of institutionalization – democracies and monarchies being, on the whole, less likely to use violence against either the opposition or foreign actors. The final chapter (Chapter 10) summarizes these findings, draws some conclusions from the empirical analysis, and discusses some implications for future research.

## 2

### **Political regimes in the world, 1946-2008**

Plato had defined the human being as an animal, biped and featherless, and was applauded. Diogenes plucked a chicken and brought it into the lecture-room with the words, 'Here is Plato's human being.' In consequence of which there was added to the definition, 'having broad nails' (Diogenes Laertius, *Lives of Eminent Philosophers*, Book 6, Chapter 40).<sup>19</sup>

There are two lessons that can be learned from this anecdote. First, most definitions are implicitly comparisons (that is, objects are defined in contrast to what they are not). Second, purely descriptive definitions are not only difficult to achieve, but also pointless, because there is an infinite number of criteria on which we can rely to distinguish between classes of objects. In the example reported above, if we assume that the function of a definition is to accurately describe reality, there are no logical reasons to contest Plato's definition. The reason why we still believe that plucked chickens and human beings should be sorted into different conceptual categories is that such a distinction would be causally relevant.

Conceptualization is thus the process of drawing causally relevant distinctions between classes of objects. The same applies to political regimes. There are numerous ways of distinguishing between political regimes: the main criterion could be, for example, the height of the chief executive, the number of windows in the presidential palace, or the procedure through which political offices are filled. None of these criteria is inherently

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<sup>19</sup> As quoted in Piering, Julie (n.d.), "Diogenes of Sinope (c. 404-323 B.C.E.)". Internet Encyclopedia of Philosophy, <http://www.iep.utm.edu/diogsino/> (accessed January 31, 2015).

invalid, but we still expect the latter to be more important in its consequences.

Identifying causally relevant distinctions between political regimes is the aim of this chapter. It will thus define the frame of the dissertation and attempt to organize the diversity of political regimes in the contemporary era, by first drawing boundaries between democratic and non-democratic regimes, and then discussing how the latter should be differentiated. It will review several ways of classifying political regimes, and discuss some conceptualization and measurement issues. The main classification criterion is whether there is a regular procedure for selecting the chief executive: there are various, equally plausible alternative classification criteria, but this choice is guided by the expectation that the manner in which political offices are filled has several important consequences for the stability of political regimes. It should also be kept in mind that every attempt at classifying political regimes must confront a trade-off between exhaustiveness and accuracy: no classification can possibly capture the huge diversity of contemporary political regimes, unless it comprises more than hundred categories (but this, of course, would make the classification absolutely useless). The goal of this dissertation is to reach some general conclusions about relatively common institutional traits, which are shared by a large number of political systems. As a consequence, the regime classification used here will rely on a small number of (sometimes rather crude) criteria. To put it more bluntly, the resulting regime categories will be highly heterogeneous and many of them will necessarily include some “plucked chickens.” Yet, this approach represents a first way of making sense of the diversity of political regimes, and broad categories are necessary in order to empirically distinguishing between country-specific effects and institutional ones.

## **2.1. DEMOCRACY AND DICTATORSHIP**

How to conceptualize and measure democracy is a controversial question: in their comparative assessment of alternative democracy indices, Munck and Verkuilen (2002) surveyed no less than nine large-N datasets, and since then, a plethora of new measures have been made available (see, e.g., Beck et al. 2001; Bühlmann et al. 2011; Alexander and Welzel 2011; Pemstein et al. 2010; Coppedge et al. 2011; Boix, Miller and Rosato 2013).

Although most of these approaches rely on Schumpeter's (2003 [1942]) or Dahl's (1989)<sup>20</sup> procedural definition of democracy, all differ in their theoretical conception of democracy, their operational definition of the concept – i.e., the disaggregation of the concept into observable subcomponents – and their way of aggregating these subcomponents into an overall measure. Most of the debates revolve around questions such as which components should be taken into account in the measurement of democracy, or whether democracy measures should be categorical or continuous (see, e.g., Alvarez et al. 1996; Collier and Adcock 1999; Elkins 2000; Munck and Verkuilen 2002; Ulfelder 2006; Gleditsch and Ward 2007; Bogaards 2010; Cheibub et al. 2010; Coppedge et al. 2011).

This is hardly the place to discuss how democracy should be conceptualized and measured in general, or to engage in normative debates about how democracy ought to be like and what ideal conception of democracy should be used as a benchmark to evaluate real-world regimes. My justification for choosing one approach instead of another is methodological in essence, and this choice is ultimately guided by expectations about causal relationships between democracy and political violence.<sup>21</sup> The most important of these expectations can be summed up in one sentence: according to Popper (1963) and Przeworski (1999), elections are substitutes to violence in the sense that they allow voters to get rid of unwanted governments in a peaceful way.

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<sup>20</sup> Schumpeter's (2003 [1942]) theory of democracy is often called minimalist while Dahl's (1989) is not. Yet, both approaches are actually very similar, to the extent that Schumpeter does not restrict his definition of democracy to the mere existence of competition between political parties. After offering his famous definition of the "democratic method" ("that institutional arrangement for arriving at political decisions in which individuals acquire the power to decide by means of a competitive struggle for the people's vote," p. 269), he adds several qualifications to his initial statement. For example, to the extent that his definition does not preclude the possibility of unfair or severely restricted competition, he states that democracy requires a considerable amount of freedom of discussion and freedom of the press to be effective; he also adds that the role of voters is not limited to selecting governments, but also includes removing them from office (p. 272). Contrary to Dahl (1989), he does not explicitly name the right to vote and the right to run for public office as democratic criteria, but his theory still relies on the assumption that every citizen is in principle free to run in elections, "in the same sense in which everyone is free to start another textile mill" (Schumpeter 2003 [1942], 272).

<sup>21</sup> Of course, this is not as simple as it seems, since normative and empirical questions cannot be considered fully independently from each other. For example, one of Dahl's (1989) justifications for why democracy is desirable is that it supposedly promotes individuals' moral autonomy, their sense of responsibility, and their ability to take care of their own interests (pp. 92-93): to which extent political regimes affect these outcomes is an inherently empirical question. Conversely, opting for a procedural definition of democracy implies having at least some expectations about the outcomes institutions are supposed to produce (because we know at least since Arrow's theorem that elections do not reflect the "will of the people," elections are not a valuable good per se; they are only desirable to the extent that they lead to certain outcomes). Which outcomes are desirable is, in turn, a normative question.

Popper's (1963) and Przeworski's (1999) basic argument can be summed up as follows. Keeping in mind that any society is necessarily conflict-ridden and that not all of these conflicts can be resolved by means of negotiation or deliberation, the possibility of changing governments (however good or bad these are) is desirable per se, because the very prospect of future alternation induces the losing side to wait for the next elections rather than to engage in violent conflict. Przeworski (1999) further elaborates on this point and argues that alternation through elections is preferable to other mechanisms for changing governments (e.g., lottery), because majority voting conveys information about the distribution of preferences among voters: under majority voting, the side that just won the elections would also be the most likely to prevail in a civil war. Thus, elections induce compliance from the party that would otherwise be the most likely to resort to violence.

I thus adopt the minimal approach to democracy proposed by Alvarez et al. (1996, 4-5) and Cheibub et al. (2010, 69), and define democracy as a regime in which the chief executive office and the seats in the legislature are filled through competitive elections. I also adopt their operational definition of democracy: for a regime to be classified as a democracy, it must meet all of following requirements: 1) the chief executive must be chosen by (direct or indirect) popular elections; 2) the legislature must be chosen by direct popular elections; 3) there must be at least two parties competing in both executive and legislative elections; 4) at least one actual alternation in power must have taken place (Cheibub et al. 2010, 69).

The two most striking features of this operational definition are, first, the absence of any participation requirement, and second, the alternation rule. With regard to participation, Alvarez et al. (1996, 5) argue that in virtually all countries, universal suffrage can be taken for granted in the post-World War II era. Participation is also notoriously absent from the list of attributes of most current democracy indices, including Polity (Munck and Verkuilen 2002, 11). The alternation requirement is somewhat more counter-intuitive, and is probably one of the most controversial features of this approach (see especially Ulfelder 2006; Bogaards 2007).<sup>22</sup> Broadly speaking, the alternation rule is used as a proxy for the

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<sup>22</sup> Bogaards (2007) argues that "election outcomes and regime type can vary independently in principle" (p. 1212) – without specifying on which criteria definitions of "regime type" should rely instead – and demonstrates that elections outcomes in Africa are poor predictors of countries' scores on the Polity and Freedom House indices. This is not entirely convincing, yet: there is actually no clear theoretical reason why these two measures should be used as benchmarks, and the low correlation between electoral outcomes and countries' scores on both scales can indicate error on both sides. Ulfelder's (2006)

competitiveness of elections:<sup>23</sup> given the increasing number of dictatorships that hold multiparty executive elections, the first three rules are not sufficient to unambiguously distinguish regimes in which the opposition has a real chance to win from those in which elections are systematically biased in favor of the incumbent. As Alvarez et al. (1996, 5) state, “alternation in office constitutes prima facie evidence of contestation.”

More generally, this approach presents several features that make it suitable to testing Popper’s (1963) and Przeworski’s (1999) hypothesis.<sup>24</sup> First, it relies on a procedural definition of democracy: since the theory sketched above is based on voters’ expectations about future leadership changes, the procedure for selecting and removing government officials from office must be regular, predictable. An alternation in office that would occur randomly or by irregular means would not generate any expectations about the opposition’s future chances of accessing power.

Second, the resulting democracy measure is dichotomous. This logically results from the aggregation rule that underlies the classification: since all components are treated as necessary conditions, there are only two possible cases, i.e. either all requirements are met

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argumentation is more sophisticated: although he does not contest the face validity of electoral outcomes as an indicator of competition, he argues that the retroactive coding of systems as democracies or dictatorships according to whether the incumbent eventually lost the elections involves a bias against new or instable democracies. To be more specific, systems in which the ruling party never lost an election are coded as dictatorships, even if there are no exceptional allegations of fraud or repression (e.g., Botswana); whenever a party eventually loses elections and yields office, the regime is classified as a democracy for the entire period the same party was in power under the same rules. Retroactive coding also works the other way around: if a democratically elected incumbent once decides to close the legislature, cancel elections, or ban opposition parties, the regime is coded as dictatorship for the entire period his party was in power. As Ulfelder (2006) argues, this is problematic because 1) most recent democracies are automatically classified as dictatorships because they did not have enough time to produce electoral losers; 2) democracies that break down after a short time are treated as if they never were democracies in the first place (i.e., instable countries are *ceteris paribus* more likely to end up in the category of dictatorships), which makes the measure inappropriate to analyze the conditions under which democracy endures. While I still believe that there are no acceptable alternatives to assess the competitiveness of elections, these problems should be taken seriously.

<sup>23</sup> Alvarez et al.’s (1996) initial argumentation is slightly different. If the incumbent never loses, the question arises whether elections are not held only because he is certain to win, and whether he would not cancel results if he ever lost. When the opposition eventually wins and is allowed to assume office, they take it as retrospective evidence that the incumbent was in fact willing to relinquish power and did not hold elections only because he was certain to win. By contrast, if the opposition wins but the incumbent cancels results and refuses to yield office, there is evidence that the system never was a democracy. If the incumbent repeatedly wins elections, there is no way to unambiguously identify the regime as either democracy or dictatorship (electoral fraud being per definition difficult to observe in a reliable way): the question thus arises whether to err by coding as democracies regimes that are actually dictatorships (false positives), or by coding as dictatorships regimes that are actually democracy (false negatives). “Err we must,” they argue. “The question is which way” (Alvarez et al. 1996, 11; see also Cheibub et al. 2010).

<sup>24</sup> To my knowledge, this hypothesis has so far never been directly tested.

(in which case the regime is democratic) or one rule is broken (in which case the regime is not democratic). This also implies that there is no gradation or further differentiation within the subgroup of nondemocracies: undertaking a further differentiation within the category of autocracies would thus require identifying other dimensions than each country's "democraticness." The dichotomous nature of Alvarez et al.'s (1996) democracy measure as well as its underlying aggregation level is one of its most contested features (see especially Munck and Verkuilen 2002). However, one argument in favor of this strategy is that attributes of elections can hardly be considered independently from each other: if democracies are countries in which the chief executive's office is filled as a consequence of voters' choices, breaking one of the rules instantaneously makes the other rules ineffective and meaningless. In other words, arguing that Kazakhstan (where elections are held and repeatedly won by the incumbent with an overwhelming majority of the votes) is more or less "democratic" than South Africa in the Apartheid era (during which elections were relatively competitive but not inclusive) or China (in which no national elections are organized at all) does not make any sense: in each of these cases, voters do not choose office holders, and these are selected through other processes than popular elections.

Third, this definition of democracy is minimal, that is, exclusively based on the electoral component of democracy. Several aspects possibly related to democracy (e.g., rule of law, horizontal accountability, or protection of civil liberties) are deliberately left out of the list of requirements systems must meet to qualify as democracies. Obviously, this feature is not completely unrelated to the aggregation rule, to the extent that specifying too many necessary conditions would result in a concept without empirical referent.<sup>25</sup> The decision to reduce democracy to its electoral dimension is admittedly debatable, not only from a purely normative point of view, but also to the extent that institutional constraints on the chief executive's power, rule of law or the absence of restrictions on civil liberties very

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<sup>25</sup> Alvarez et al. (1996, 4) as well as Munck and Verkuilen (2002, 9) seem to argue that this holds true of every democracy measure, regardless of the aggregation rule. However, while additive scales also encounter this problem, they are not limited to the same extent. Specifying a too large number of criteria for an additive scale has the consequence that no country can reach the maximal score, but the scale is still able to discriminate between countries that reach a fairly high score from those at the lowest end of the scale: thus, the measure still contains some empirical information. By contrast, multiplicative indices that include too many necessary conditions result in all countries reaching the same score of 0. While such a scale would still convey the information that no real-world country approximates some ideal conception of democracy, it would be of no use for an empirical analysis.

probably affect the relationship between elections and political violence: for example, it is possible that systems in which one party constantly wins elections but that also encompass powerful checks on the executive and grant the opposition the possibility to influence policies are less prone to political violence than centralized political systems with no possibility of alternation. Likewise, the extent to which alternation generates compliance probably depends on the ability of elected officials to effectively govern, which in turn depends on issues such as civilian control of the military or state capacities. In short, I do not argue that such issues are completely unimportant. I do argue, however, that minimal definitions of democracy are preferable to more extensive ones from an analytical point of view. First, as discussed above, there is a clear, direct causal argument linking elections to peace. Second, while characteristics of elections – e.g., competitiveness and inclusion – are theoretically interdependent, the effects of elections and the effects of other attributes of democracy (such as executive constraints, rule of law, or civil liberties) can be analyzed independently from each other. Third, although more demanding definitions of democracy can be preferable from a normative point of view, minimal democracy measures make it possible to isolate specific causal relationships and investigate them separately, while existing classifications that rely on non-minimalist definitions tend to collapse different institutional features into a single measure and make it impossible to properly test specific hypotheses.

This latter issue has been extensively discussed with regard to the widely used Polity index (Marshall et al. 2011). The index is made up of six components that are added and converted into a 21-points scale ranging from -10 to 10: regimes receiving a score higher than 5 on the index are then classified as democracies, regimes receiving a score lower than -5 are classified as authoritarian regimes, and the remainder are labeled “anocracies”. These coding rules generate two related problems: first, there are hundreds of possible institutional combinations through which a regime can achieve a specific score (Gleditsch and Ward 1997). This problem is especially acute in the case of anocracies, which account for more than half of the range of the scale: an anocracy can be an anocracy for various reasons, and there is no institutional combination through which one can perfectly predict whether a regime belongs to the group of anocracies or not.<sup>26</sup> Second and relatedly, the

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<sup>26</sup> Yet, curiously, the most heterogeneous of these categories seems to be the one of authoritarian regimes: a

aggregation rule that underlies the index makes it hard to interpret empirical relationships: if, for example, a positive correlation is found between anocracies and civil conflict (see, e.g., Hegre et al. 2001; Fearon and Laitin 2003), it is nearly impossible to identify which dimension of democracy (or institutional combination) is driving the relationship (Cheibub et al. 2010; Regan and Bell 2010). These issues do not only concern the Polity index, but also other additive democracy measures such as the Freedom House index.<sup>27</sup> These analytical issues thus call for a more parsimonious operational definition of democracy.

## 2.2. CONCEPTUALIZING AUTHORITARIAN REGIME TYPE

The inherent difficulty in classifying authoritarian regimes partly stems from the fact that, unlike democracy definitions,<sup>28</sup> operational definitions of dictatorships cannot be derived from an “ideal” model of authoritarianism (monarchies being the only exception in this respect). While the distinction between democracy and dictatorship is deductive, classifications of authoritarian regimes are inductive, i.e., they are taxonomies rather than typologies. In the absence of a theoretical (normative) definition of dictatorship, the only possible approach consists in observing real-world dictatorships and trying to make sense of their diversity.

The resulting regime classifications are easy to criticize: they tend to place too much emphasis on a few visible and often arbitrarily defined traits, the criteria used to distinguish regimes are not always consistent, and they often result in categories that are neither exhaustive nor mutually exclusive (see Svobik 2012, Chapter 2). Yet, some of these critiques are somewhat overstated, and they are not always well-founded: provided that regime classifications are one-dimensional – which is often the case – using, say, Geddes’ (1999) or Cheibub et al.’s (2010) tripartite classifications is not fundamentally different from using a simple trichotomous variable which can, in turn, be included in a statistical

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quick comparison between Polity scores and Cheibub et al.’s (2010) disaggregated data reveals that (for the period from 1946 to 2008) 24% of the regimes scoring between -6 and -10 have an elected executive, an elected legislature and multiple parties; in 21% of these regimes, the executive and the legislature are both unelected and opposition parties are banned. Interestingly, a substantial proportion of autocracies could be in fact qualified as hybrid regimes.

<sup>27</sup> Note that the Freedom House index is not a measure of democracy per se, but it is commonly used as one (see, e.g., Muller and Weede 1990; Weber 2010).

<sup>28</sup> Of course, the fact that the definition of democracy is first normative and then empirical is the source of many of the difficulties of measuring democracy.

model together with variables measuring other aspects of political regimes. For example, nothing speaks against using Geddes' (1999) classification (as a measure of the identity of leaders) simultaneously with measures of political competition, ideology, regime repressiveness, or whichever dimensions researchers deem relevant for answering their research question.

Viewing regime classifications as simple variables also solves the issue of arbitrariness. For example, comparing economic growth in, say, military and party regimes is neither "better" nor "worse" than examining whether rule of law, civil liberties, or the number of veto players affect economic development: regime definitions are not set in stone and they can be used flexibly, depending on the research question. In short, the stance I adopt here is that regime classifications can be useful shortcuts as long as it can be proven that they have some predictive power. In turn, which institutional traits are causally relevant is first a theoretical and then an empirical question: examining the predictive power of "regime type" is the overarching goal of this dissertation, but it requires, first, identifying the institutional traits that can be expected to be causally related to the outcomes of interest – that is, choosing among the numerous classifications of authoritarian regimes that have been proposed in recent years. This will be the aim of the following sections.

### **2.2.1. Contemporary approaches to authoritarian regimes**

Researchers' growing interest in dictatorships has given rise to a proliferation of classifications of authoritarian regimes, most of which are coupled with publicly available datasets that cover a large number of countries over a long period of time.<sup>29</sup> At first glance, many of these classifications seem interchangeable to the extent that they tend to correlate with each other and identify the same regime types: for example, the overwhelming majority of discrete classifications of autocracies includes a category for military dictatorships (Geddes 1999; Hadenius and Teorell 2007a; Cheibub et al. 2010; Magaloni et al. 2013), monarchies (the only exception in this regard being Geddes' initial typology, which was subsequently updated so as to include monarchies) and some measure of party

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<sup>29</sup> Given the objectives of the present work (which is mostly empirical), I will limit the discussion to these approaches and deliberately ignore purely theoretical contributions.

regimes (Geddes 1999; Hadenius and Teorell 2007a; Magaloni et al. 2013). As Wilson (2014) notes, this apparent similarity has resulted in the widespread practice of substituting datasets for each other in robustness checks of empirical findings.<sup>30</sup> Yet, these measures differ in both their theoretical underpinning and their operationalization of regime type.

Broadly speaking, discrete regime type measures either emphasize the institutional setup of the regime (Hadenius and Teorell 2007a; Wahman et al. 2013) or the launching organization on which the dictator relies to access and maintain power (Geddes 1999; Cheibub et al. 2010). The latter approach rests on the argument that identifying who rules and who supports the ruler is crucial for understanding the risks and constraints dictators face and how they are likely to exercise power once in office. Geddes (1999) distinguishes between military dictatorships, party regimes, and personalist autocracies: her main point is that relying either on the armed forces, a mass party or a narrow clique affects both the dictator's longevity in office and elites' behavior in face of external crises. Understanding how dictators behave, she argues, requires identifying on whom they depend to stay in power and what the fundamental interests of their constituents are. For example, regimes that rely on the military are the most brittle because, contrary to civilian elites, most military officers do not want to hold onto power at any cost.

While many scholars have criticized the notion of “personalism” as a distinct regime category – arguing instead that personalism is an attribute that can be more or less present in every type of dictatorship (e.g., Hadenius and Teorell 2007a; Svobik 2012, 31) – Geddes' approach with regard to party regimes and military dictatorships has clearly set the agenda for subsequent research: it would be indeed difficult to find any cross-country comparative study on authoritarianism published in the last fifteen years that does not at least refer to the military/party dichotomy, and even harder to find one that does not distinguish between military and civilian regimes.<sup>31</sup>

Cheibub et al.'s (2010) approach is relatively similar to the extent that they differentiate

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<sup>30</sup> Wilson convincingly argues that this practice, instead of being methodologically sound, rather indicates poor theory-building.

<sup>31</sup> Svobik (2012), who is overall very critical of the use of regime classifications, could be that exception. However, the two last chapters of *The Politics of Authoritarian Rule* – which deal with the problems of authoritarian control – mirror the military / party dichotomy to the extent that they deal with the effectiveness of these two types of launching organizations in ensuring the regime's stability. Even though his arguments differ widely from Geddes' (1999) theory, he reaches a similar conclusion: party-based regimes are more stable than military-based ones.

authoritarian regimes according to the characteristics of the ruling elite. Dictators, they argue, are often deposed by regime insiders: thus, identifying which group has effective decision-making power provides insight into the internal organization of the regime, the type of incentives and constraints the dictator faces, and how he is likely to exercise power or be removed from office (Cheibub et al. 2010, 84). They put forward a tripartite classification of authoritarian regimes that encompasses the categories of civilian dictatorships, military regimes and monarchies.

By contrast, Hadenius and Teorell's (2007a) initial typology of authoritarian regimes, as well as its updated version by Wahman et al. (2013), is explicitly based on institutions. They reject approaches based on the identity of the ruler, and differentiate instead three modes of accessing and maintaining political power, namely hereditary succession, popular elections, and the actual or threatened use of military force (Hadenius and Teorell 2007a, 146). From these rules, they derive three generic regime types, namely monarchies, electoral regimes, and military dictatorships. The category of electoral regimes is further split into several subtypes, according to the degree of competition allowed by the regime. Thus, the most important conceptual divergence between their classification and the two alternatives is the emphasis on rules rather than leadership traits.

That being said, the main reason why regime typologies so frequently overlap despite relying on different theoretical foundations is that in practice, most of them are based on a mix of criteria that are partially derived from both approaches. For example, Cheibub et al. (2010, 88) define monarchies as regimes in which the ruler bears the title of "king" or equivalent, and takes power and/or is replaced by rules of hereditary succession. Yet, these rules do not logically follow from their main criteria for distinguishing regimes, i.e. the aforementioned "inner sanctum." To the extent that hereditary succession is an institution in the narrowest sense of the term, the coding rules for identifying monarchies can hardly be deduced from the identity of the launching organization.<sup>32</sup> This lack of consistency

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<sup>32</sup> This makes sense to the extent that monarchies are not necessarily based on primogeniture, which gives the ruling dynasty some leverage in the succession process: thus, the royal family is not "constrained" by exogenous succession rules (Cheibub et al. 2010, 85). Yet, this is only valid for the subset of monarchies in which primogeniture is not a formal rule. Furthermore, it is not completely clear why the ruler has to bear the title of king: if the argument is that any ruling family, once it is in power, has an incentive to choose the leader within its own ranks (i.e., that hereditary succession is not an institution), it should also apply to some civilian autocracies which are not formally monarchies but are de facto based on hereditary succession. Either hereditary succession results from the preferences of the launching organization (in

between theoretical foundation and actual operationalization rules is even more obvious in Hadenius and Teorell's (2007a) and Wahman et al.'s (2013) classification: first, although they claim that they primarily focus on "institutions on which (...) elites rely to regulate the access to and maintenance of public authority" (Wahman et al. 2013, 22), they include the "actual or threatened use of military force" (which can hardly be viewed as an institution) in their categorization of modes of regime maintenance (Hadenius and Teorell 2007a, 146; Wahman et al. 2013, 21). Second, their regime classification is not logically derived from these institutions, since no-party and one-party regimes are neither based on military force nor on hereditary succession, and most of them (if any at all) are not based on elections.<sup>33</sup> However, in spite of these internal inconsistencies and the fact that as a result, regime typologies frequently overlap, the main reason for opting for a classification instead of another is the authors' underlying arguments about the consequences following from these distinctions: as I shall argue later, distinguishing different sets of regimes on the basis of different and somehow inconsistent rules is a less than elegant solution, but it is also a necessary evil.

### **2.2.2 Rules that do not bind: Does the mode of accessing political power define regime type?**

As discussed above, there are two main ways of making sense of the diversity of nondemocratic regimes: the first way consists in identifying who rules and who supports the ruler; the second way consists in identifying the rules that regulate access to, and exit from political offices and the way political power is exercised. The argument I put forward is that this second approach is inherently problematic.

The "institutional" approach to nondemocratic regimes is at first glance compelling because it is parsimonious (differentiating subtypes of democratic and nondemocratic regimes requires only one criterion) and symmetric (the distinction between subtypes of

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which case the formal denomination of the regime is irrelevant), or it is an institution (in which case the strict "inner sanctum" criterion does not apply): either way, the classification is not entirely consistent with the underlying theoretical approach.

<sup>33</sup> To a certain extent, this applies to multiparty autocracies as well. The fact that they organize elections or not is not the point: the point is that the chief executive is not selected or removed as the result of popular elections, and whether elections constrain his behavior at all is still an open question.

nondemocratic regimes is undertaken on the basis of the same criteria that are used to distinguish between subtypes of democracies). The idea that the criteria for distinguishing democracies from dictatorships should be the same as the ones for distinguishing between subtypes of dictatorships is *prima facie* logical. The problem, however, is that not all regimes are institutionalized to the same extent: the fundamental difference between democracies and nondemocracies might not lie in the nature of institutions, but rather in the extent to which these are actually constraining. To put it otherwise, there are two possible views regarding the role of institutions in democracies and dictatorships: the first view holds that democracies and dictatorships are different because they rely on different institutions. The second view holds that, while democracies rely on institutions, dictatorships most often do not.

The role of formal, written institutions in dictatorships is still subject to debate, but current research points to two potential pitfalls in analyzing them: first, formal institutions often do not correspond to actual practices (see, e.g., Ginsburg and Simpser 2014). Second, authoritarian institutions that resemble democratic ones – e.g., elections or parliaments – are in fact instruments of control, that do not constrain the leader but instead help him consolidate his rule (e.g., Gandhi 2008a). These two facts suggest that the criteria we use for identifying democracies are not necessarily valid for distinguishing types of autocracies from one another.

Defining regimes based on their institutional traits first requires knowing what precisely an institution is. Definitions of (political) institutions can be broad or narrow, but most of them tend to emphasize three core components. First, institutions are constraints; second, they are, if not permanent, at least relatively durable; third, they are transparent. With regard to the first aspect, North (1991, 97) defines institutions as the “humanly devised constraints that structure political, social and economic interaction.” Riker views them as “rules about behavior, especially about making decisions” (Riker 1982, 20, as quoted in Ostrom 1986, 3). The second aspect – durability – is emphasized by Glaeser et al. (2004, 275), who state that transitory rules would not be as binding as permanent ones. The third component – transparency – is emphasized by Ostrom (1986), who offers a comprehensive definition of institutions as rules:

Rules (...) refer to prescriptions commonly known and used by a set of participants to order repetitive, interdependent relationships. Prescriptions refer to which actions (or states of the world) are *required, prohibited, or permitted*. Rules are the result of implicit or explicit efforts to achieve order and predictability within defined situations by: (1) creating positions (...); (2) stating how participants enter and leave positions; (3) stating which actions participants in these positions are required, permitted, or forbidden to take; and (4) stating which outcome participants are required, permitted, or forbidden to affect (Ostrom 1986, 5; italics in original).

With these basic points in mind, the inherent difficulty of studying institutions in authoritarian settings becomes apparent: it requires, first, assessing whether formal institutions really constrain the dictator's behavior (i.e., whether he would behave differently in their absence, or under different institutions); second, being able to distinguish between outcomes that result from the existence of institutions and those that result from the dictator's policy choices (see Glaeser et al. 2004); third, distinguishing between "real" institutions and power-maintenance strategies that autocrats implement to cope with specific problems, and that they can modify at will. In sum, the problem with dictatorships is that formal institutions typically do not constrain the dictator, and the real sources of constraints are often not institutions.

The second aspect of the problem is obvious in the case of military dictatorships. Although Wahman et al. (2013) insist that their focus is on the institutional setup of authoritarian regimes, they define military dictatorships as regimes that rely on the "actual or threatened use of military force" as their primary mode of regulating access to, and maintenance of, political power (p. 21; see also Hadenius and Teorell 2007a, 146). Yet, the use of military force as a means to access and maintaining political power can hardly be viewed as an institution. Leaving military dictatorships aside, it is not obvious whether the other institutions Wahman et al. (2013) identify really determine how leaders attain, maintain and exercise power. Many contemporary authoritarian regimes may display a whole set of seemingly democratic institutions, such as parties, legislatures, and contested elections.<sup>34</sup> But, to the extent that elections are not competitive enough to put the chief executive's seat at stake and that legislatures either are not popularly elected or are controlled by the

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<sup>34</sup> The problem with classifying closed party dictatorships such as the USSR as "electoral regimes" is self-evident enough that there is no need to elaborate on it.

regime-sponsored party or do not have the power to remove the government from office – in short: that the chief executive is neither selected nor removed through these mechanisms – it is highly questionable whether authoritarian elections qualify as a constraint. Furthermore, most of these “institutions” fall short of the permanence requirement: in most authoritarian regimes – with the exception of some “hybrid” regimes that do allow fairly competitive executive and legislative elections but for some reason fail to be completely democratic<sup>35</sup> – elections can be canceled, legislatures can be closed, opposition parties can be banned, without fundamentally altering the nature of the regime.<sup>36</sup> This is not to say that these arrangements are generally inconsequential: such an assertion would be flatly contradicted by the most recent findings on contemporary dictatorships. For example, elections have been shown to contribute to the stabilization of authoritarian regimes (Gandhi and Lust-Okar 2009), as well as parties and legislatures (Gandhi 2008a, 2008b; Gandhi and Przeworski 2006, 2007). Yet, most researchers agree that even if these arrangements resemble democratic institutions, they serve distinctively authoritarian purposes. For example, while surveying the various functions elections can perform under dictatorship, Gandhi and Lust-Okar (2009) argue – amongst other – that relatively competitive elections can be used to divide the opposition or provide the incumbent with valuable information on the level of public support for the regime, while uncontested elections signal the regime’s strength and deter potential challengers from defecting. While analyzing the role of parties and legislatures, Gandhi (2008a) is very careful to state that these are only “nominally democratic institutions,” and not diminished versions of their democratic counterparts. While these organs may help regimes regulate the interactions between ruling elite and key segments of the civil society, the dictator’s propensity to cooperate does not result from their existence: rather, the existence of parties and legislatures results from the need to gain support from specific segments of the population (see also Gandhi and Przeworski 2006, 2007). In this sense, parties and legislatures are not “institutions” in the sense of durable constraints, but rather instruments of control. All in all, this implies that many authoritarian institutional arrangements are rather tools

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<sup>35</sup> Some would say the reason is simply measurement error: see, for example, the “Botswana controversy” in Alvarez et al. 1996, Ulfelder 2006, Bogaards 2007, Cheibub et al. 2010.

<sup>36</sup> Of course, this does not mean that it can be done without consequences, as the Algerian military’s decision to cancel the 1991 legislative elections exemplifies.

than constraints, and rather problem-oriented than permanent. Contrary to democracies, dictatorships are institutionally flexible, in the sense that one of these arrangements could disappear without the regime's basic structures and way of functioning being fundamentally altered. In other words, the set of necessary conditions included in the definition of democracy implies that, once one of these rules is broken, democracy ceases to exist. It is by contrast harder to identify formal institutions that autocrats cannot modify without changing the nature of the regime: to take an extreme example, it is doubtful whether the highly personalized regime of contemporary Kazakhstan would be drastically different if the current president had decided not to hold the 2015 presidential elections (that is, the elections he won with more than 97% of the votes).<sup>37</sup> In most cases, we know that an authoritarian regime has ended when both the leader and his ruling coalition have changed.

Given the difficulties in identifying effective institutions in dictatorships, I will thus rely on the "ruling coalition" approach (Geddes 1999; Cheibub et al. 2010) to distinguish non-democratic regimes. I only depart from these approaches in one way: I argue that, unlike other authoritarian regimes, monarchies rely on binding rules that regulate access to, and succession in power. Details will be provided in the next section.

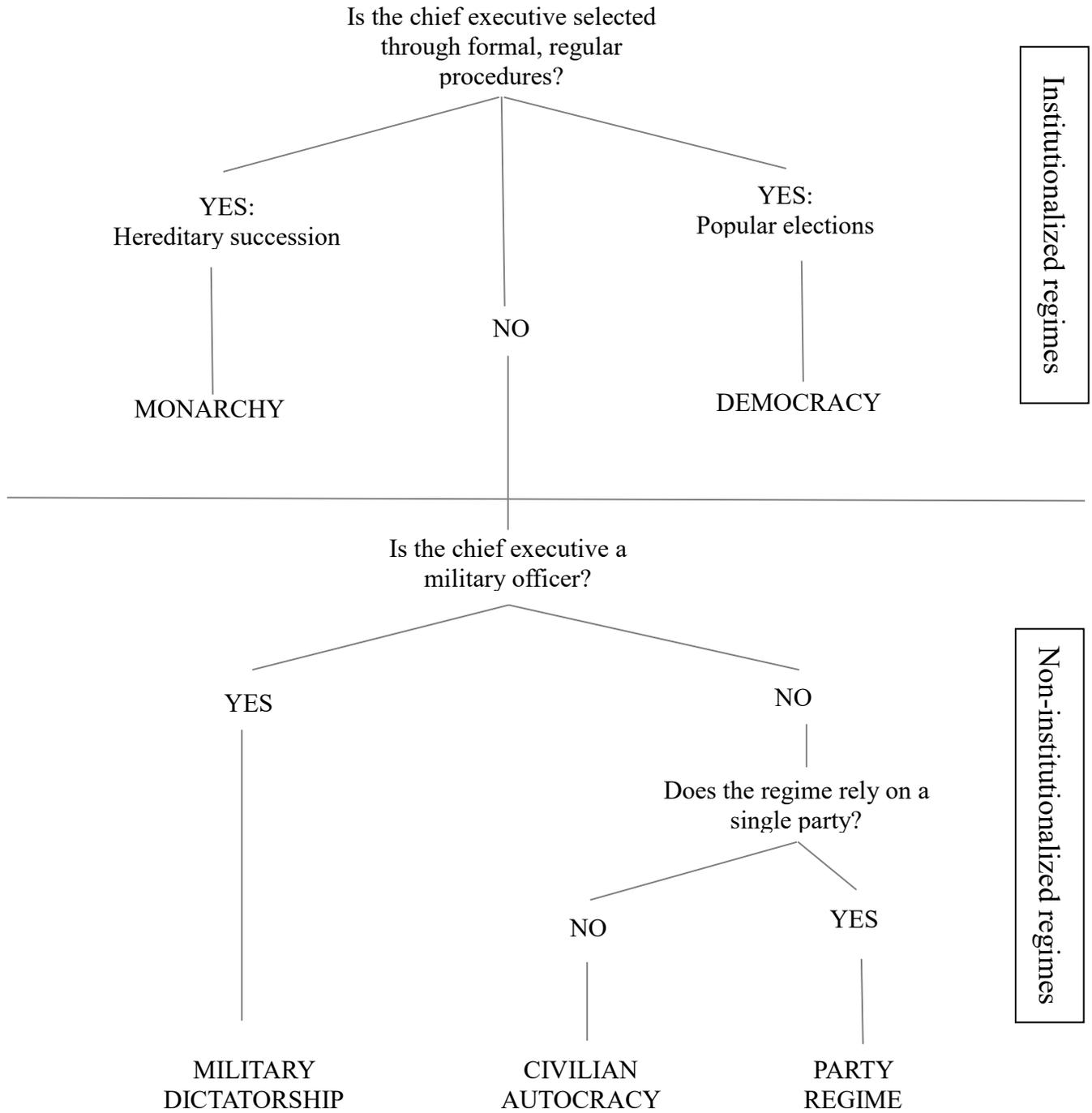
### **2.3. A CLASSIFICATION OF POLITICAL REGIMES**

The regime classification I will rely on is a slightly modified version of Cheibub et al.'s (2010) regime classification. The original classification includes – besides democracies – three regime categories, namely monarchies, which rely on kinship networks, military dictatorships, which are ruled by a military officer or a junta of military commanders, and civilian dictatorships, which rely on a small body within a ruling party (Cheibub et al. 2010, 84). While I adopt the overall classification, my approach to political regimes differs from Cheibub et al.'s (2010) in two ways: first, I add one regime category (see below). Second, my theoretical approach to political regimes – that is, the rationale behind the definition of regime categories, and expectations about the consequences of these distinctions – is slightly different from theirs.

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<sup>37</sup> "Kazakh leader gains crushing election victory." BBC news, April 27, 2015, available at <http://www.bbc.com/news/world-asia-32471428> (accessed May 2, 2015).

Figure 2.1. Political regimes in the world, 1946-2008



Note: adapted from Cheibub et al. 2010, 87

Figure 2.1. summarizes the classification criteria. The first step consists in identifying the rules that specify how the effective leader is selected, dismissed, and replaced: regimes that provide such formal rules are considered institutionalized, while those that rely on looser or more informal rules are labeled non-institutionalized regimes. Consistently with the definition of institutions I rely on (see Section 2.2.2.), these rules have to be effectively binding, durable, and transparent. In other words, these institutions have to be formal – which means that I do not take temporary power-sharing arrangements into account – and they have to be implemented effectively – which means that the selection of the effective leader has to take place in accordance with these rules on a regular basis. There are, on the whole, two ways of selecting leaders that satisfy these criteria: on the one hand, in democracies, the leader is selected through regular, competitive elections, for a fixed term in office; on the other hand, in monarchies, access to power is determined by hereditary succession.

### **2.3.1. Institutionalized regimes**

The category of institutionalized regimes is comprised of monarchies – that rely on hereditary succession – and democracies – that rely on periodic, competitive elections. Theoretically speaking, there are other conceivable ways of selecting chief executives: elections and hereditary elections do not exhaust all possibilities.<sup>38</sup> Yet, in contemporary political regimes, these two rules are the only ones that are applied consistently and across a wide range of cases.

Since these two rules pertain to the selection of the effective leader, they are mutually exclusive: the chief executive cannot be elected and inherit power at the same time. Ceremonial monarchies are thus classified as democracies if the effective head of

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<sup>38</sup> An alternative could be leadership selection by seniority in party-based regimes or military juntas. The rule would be self-enforcing on the long run, since it would incite members of the launching organization to invest in the regime (see Svobik 2012, Chapter 6 for a discussion of this mechanism in party regimes) and give the most powerful elites an incentive to wait until the current leader dies or retires instead of attempting to overthrow him (more on this in Chapter 6). Since the extent to which the ruling organization effectively controls political appointments varies from one dictatorship to another, this could partly explain the variations in the durability of these regimes: military and party regimes that explicitly rely on the seniority rule can be expected to be more stable than their counterparts that do not. Unfortunately, I am not aware of any available dataset that could provide such an information.

government is democratically elected. Another notable feature of Cheibub et al.'s (2010) classification is that the coding rules for both monarchies and democracies specify that a leadership change under rules identical to the ones that brought the previous leader to office must have taken place at least once. With regard to democracies, this means that leaders who attained power through elections must be replaced through elections, i.e., that the opposition must access power under rules identical to the ones that brought the previous leader in office (for a discussion of the alternation rule, see Alvarez et al. 1996, 10-13; Cheibub et al. 2010, 70-71). With regard to monarchies, the chief executive must not only bear the title of king or equivalent but also either inherit the throne from a relative or be succeeded by a family member, which excludes self-proclaimed monarchs such as Jean-Bedel Bokassa of the Central African Republic. Because elections and hereditary succession have to exist *de jure* and be practiced *de facto*, these procedures for selecting leaders are both characterized by a certain degree of stability and predictability.

At first glance, including monarchies in the subgroup of institutionalized regimes might seem at odds with the definition of institutions discussed in Section 2.2.2 (see Ostrom 1986; North 1991). In some respects, hereditary succession is an institution: it is a binding, commonly known rule that specifies which “players” can legally enter what types of positions and which decisions they are allowed to take. Concretely, hereditary succession specifies that only certain individuals can legally become monarchs; if primogeniture is not automatic, it specifies who (that is, the ruling dynasty) is allowed to choose the ruler among the group of potential contenders; it finally specifies the duration of the chief executive’s term in office<sup>39</sup> and the conditions under which the ruling coalition is allowed to replace him (that is, after his death), and who is or is not a potential successor. Yet, these rules clearly benefit the monarch and it is not obvious to which extent they constitute a constraint on his behavior: to the extent that he is the only legitimate ruler and cannot be removed from office (while he still can legally abdicate on his own initiative), hereditary succession frees him from any constraint from either citizens or his ruling coalition and protects him against any attempt to hold him accountable. From this perspective, monarchy could be viewed as the most extreme form of power personalization.

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<sup>39</sup> Hereditary succession does not necessarily imply that monarchs are selected for life; however, primogeniture makes lifelong tenure a requirement for practical reasons (since it takes the successor quite a long time to come of age).

However, by legally restricting the set of potential contenders for the throne, hereditary succession still entails a fundamental constraint on the monarch's discretionary power: contrary to most dictators, monarchs cannot select their successors. Although this restriction might seem secondary, its crucial implication is that the current leader cannot use his power to designate successors as a bargaining chip, and hence cannot manipulate his ruling coalition by distributing future positions as rewards for loyalty. Paradoxically, the very fact that they have no hope of attaining power under current institutions thus makes members of the ruling elite more independent from the monarch than they would be from any other dictator. As will be discussed in Chapter 3, this also reduces some of the uncertainty that often characterizes the relationships between the leader and the members of his ruling coalition (see Tullock 1987; Svobik 2012). Besides the fact that monarchies and democracies rely on strict procedures for selecting the chief executive, what these two systems have in common is that the current ruler does not play the decisive role in the succession process.

### **2.3.2. Non-institutionalized regimes**

Thus, democracies and monarchies display formal rules that ensure orderly access to, and succession in power. By contrast, regimes I classify as non-institutionalized lack such rules: they are rather anarchic systems in which conflicts regarding access to power are typically solved by force. In those systems, politics tend to be personalized, unregulated and opaque: in general, the leader stays in office for life unless he is forced from power by rivals, and succession in office is the result of either cooptation (that is, the new leader is chosen by his predecessor, provided that the latter is powerful enough to impose his will on his allies) or of a violent struggle between elites jockeying for power. This most obviously applies to what Geddes (1999) labels personalist dictatorships, in which power is concentrated in the hands of a single ruler and which are consequently highly vulnerable to succession crises upon the leader's death. But even in more collegial forms of dictatorship, there are usually no agreed-upon procedures to remove a leader from power without violence or to select a new one. Even if the regime relies on a formal organization – such as a party or the military – and real power lies in a formal body such as the

Politburo in communist one-party regimes or the junta in corporate military dictatorships, the processes of leadership selection and succession are typically informal and often entail large-scale violence (see Sandschneider 1985; Tullock 1987; Acemoglu, Egorov and Sonin 2008; Svulik 2012). To take the example of the Soviet Union, Acemoglu, Egorov and Sonin (2008), 990) report that, of the 40 Politburo members appointed between 1919 and 1952, only one could leave the body and survive. The fate of several of Mao's potential successors after his death is also telling, as are the violent succession crises that characterized leadership transitions in China until recently (Sandschneider 1985). To state the matter in a more abstract way, a growing body of research, beginning with Tullock (1987), has begun to conceptualize authoritarian politics in terms of dynamic coalition formation (e.g., Acemoglu, Robinson and Verdier 2004, Acemoglu, Egorov and Sonin 2008, 2009; Svulik 2012): in dictatorships, there are no rules that prescribe how political positions should be allocated, and there is always the possibility of a subversive coalition forming against the leader and his closest allies.

But are all non-institutionalized regimes alike? Intuitively, we might at least expect that there are more differences between the categories of institutionalized and non-institutionalized regimes than within the latter category. Yet, as already discussed, various attempts at differentiating authoritarian regimes and the resulting findings regarding dictatorships' behavior suggest that non-institutionalized regimes are not necessarily a homogeneous group. Precisely because we now know that most dictators are removed from power by members of their own ruling coalition (Tullock 1987; Haber 2006; Gandhi 2008a; Cheibub et al. 2010; Svulik 2012), identifying the group who has the power to effectively constraint the leader's decision by threatening to remove him from office is one straightforward way to predict how regimes will behave and with what outcomes. If the incumbent and his coalition rule without formal constraints, the identity of the current leader – and, by extension, of his supporters – becomes relevant to predict variations among non-institutionalized regimes.

Consequently, the next step consists in distinguishing regimes that are led by a member of the military from those led by a civilian. This criterion for differentiating non-institutionalized regimes has two advantages. First, it is directly observable<sup>40</sup> and does not

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40 This does not, of course, preclude difficult and sometimes contestable coding decisions. For example,

require making subjective judgments about, for example, the degree of concentration of power or the nature of decision-making processes. Second, the civilian/military dichotomy is widely believed to be causally related to a wide range of outcomes. It is believed, for example, to affect the stability of political regimes, because military officers tend to value political power less highly than civilian politicians and often still have the option of continuing their previous career following a transition back to civilian rule (Nordlinger 1977; Geddes 1999). Military officers' specific professional socialization is also widely thought to affect their preferences and beliefs about the appropriateness and effectiveness of the use of violence in crisis situations: this argument is regularly used to explain why military dictatorships seem to be both internally (Nordlinger 1977; Poe et al. 1999; Davenport 2007b; Fjelde 2010) and externally (Sechser 2004; Lai and Slater 2006; Weeks 2012; Horowitz and Stam 2014) more violent than autocracies ruled by civilians.

Dictators who are not members of the armed forces often rely on a political party to maintain their rule. However, this is not always the case: as Hadenius and Teorell (2007a) report, some countries (such as the Maldives) prohibit all parties. Furthermore, ruling parties differ strikingly with regard to their internal organization, their degree of autonomy, their extent of control on the population, and the extent to which they can exercise some constraints on the autocrat's rule (Smith 2005; Magaloni 2008; Gehlbach and Keefer 2012). While the role played by some ruling parties – e.g., the Chinese Communist Party or the Mexican Partido Revolucionario Institucional – in ensuring cohesion at the elite level and establishing inter-temporal power-sharing agreements is now well-documented in the literature (Geddes 1999; Brownlee 2005; Magaloni 2006, 2008; Svobik 2012; Köllner 2013), not all parties perform these functions. Some ruling parties are loose organizations that assemble the dictator's following, some others are created by fiat to mobilize popular

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Cheibub et al. (2010) code as military all leaders who are or ever were members of the military, even if they retired from service (with the exception, however, of those who fought in World War II and left the military afterwards). While the underlying argument (that these leaders still have connections with, and access to the armed forces, which sets them apart from civilian leaders) is convincing, this decision still has the drawback of lumping together active members of the military and leaders who were once in the armed forces but retired long before taking office. But then again, undertaking a more fine-grained distinction between former members of the military who still have significant connections with the armed forces from those who have not would require making arbitrary decisions about, for example, the minimal duration of military service before past membership in the armed forces is deemed significant enough, or the time elapsed between the moment the leader left the armed forces and the moment he took office.

support behind the new regime,<sup>41</sup> and many of them are “parties of power” (see Herron 2009; Isaacs 2011) that operate in the shadow of the regime’s strongman. Cheibub et al. (2010) do not take these distinctions into account: the group of civilian regimes is rather a residual category that encompasses all nondemocratic regimes that are neither governed by a hereditary monarch nor by a military officer. Yet, given the prominence of party-based autocracies in the literature, I undertake a further differentiation within the subgroup of civilian regimes: I classify these regimes as single-party regimes if they effectively allow only one party and prohibit all the others, and as (other) civilian autocracies otherwise.<sup>42</sup> Arguably, the mere existence of opposition parties does not perfectly reflect what I intend to measure (that is, the strength of the ruling party and its ability to exert some control on both the population and the dictator), given that some powerful ruling parties (e.g., the PRI in Mexico) allowed an organized opposition to run in elections and sometimes gain a fair share of the votes. However, given that the strength and internal cohesion of ruling parties are difficult to gauge empirically, this proxy has two advantages. First, it is directly observable and does not rely on subjective judgments on the part of the coder. Second, although crude, it is still informative: it indicates, on the one hand, whether the ruling party was able to eliminate organized opposition and become the “only game in town.” On the other hand, the existence of opposition organizations outside the ruling party is likely to crucially affect the relationships between the dictator and his allies, because it might strengthen the autocrat’s power by generating divergent incentives for dissatisfied elites – since some members might try to exit the ruling party and join another organization while some other might prefer to plot a coup (see Haber 2006; relatedly, Cox 2008; for an alternative point of view, see Magaloni 2008).

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<sup>41</sup> Apparently, this practice is also common among military regimes (see Geddes et al. 2014). A well-known example is the South Korean Democratic Republican Party, which was created by the junta shortly after the 1961 military coup but was later discarded by Park and was banned upon the latter’s death (Nordlinger 1977, 136-7; Savada et al. 1992).

<sup>42</sup> Empirically, this distinction is made using the variable “de facto,” which is available in the dataset (see Cheibub et al. 2009). The dataset provides three variables that measure the number of political parties: “De jure” indicates whether there are legal opposition parties, and both “de facto” and “de facto 2” indicate whether opposition parties actually exist (the latter information being arguably more useful, since the legal status of opposition parties does not always reflect actual practices). The difference between “de facto” and “de facto 2” is that, while the former simply indicates whether parties exist beside the incumbent’s party, the latter measures the existence of parties “outside the regime front” (Cheibub et al. 2009, 5). “De facto 2” provides valuable information, but I still prefer to rely on “de facto,” because I am not entirely clear about what exactly “regime front” means and how the authors were able to observe this empirically.

The remaining category of civilian autocracies is arguably the most heterogeneous one, since it encompasses, among others: a) relatively institutionalized, party-based dictatorships such as Mexico under the PRI; b) pure personalist autocracies such as the Philippines under Marcos; c) ambiguous cases such as Botswana (which many researchers would classify as democracy); and d) regimes *sui generis* such as the Islamic Republic of Iran. Thus, this group rather constitutes a residual category than a distinctive regime type. Yet, a distinctive trait of these regimes is that the leader relies on multiple sources of power – or, alternatively stated, that neither the dictator’s party nor the military is powerful enough to exercise independent decision-making power or decide over political appointments. This intra-elite competition, on the one hand, fosters inter-organization rivalries and thereby makes the leader relatively independent from his allies (see Haber 2006), since most of them are not able to coalesce to remove him from office. On the other hand, the lack of cohesion of the ruling elite should make these regimes more fragile, and more vulnerable to succession crises.

### **2.3.3. What this classification is not.**

Because authoritarian regimes can be differentiated by other (different but potentially overlapping) criteria, I will briefly discuss these in order to avoid unnecessary confusion.

First, the distinction between single-party regimes, military regimes and civilian regimes does not reflect the relative openness of these regimes; it is, for example, unrelated to current discussions on “competitive” or “electoral” authoritarianism (e.g., Levitsky and Way 2002; Schedler 2006). Likewise, civilian autocracies are not necessarily hybrid regimes (see Diamond 2002) and single-party regimes are not necessarily totalitarian regimes, even though the latter mostly rely on a party (see Linz 2000).

By the same token, the repressiveness of authoritarian regimes is not part of the classification I use. The extent to which authoritarian regimes restrict civil liberties is better reflected by indicators such as the Freedom House index or the Cingranelli and Richards Human Rights Data Project. With regard to coercion (that is, the extent to which regimes repress physical integrity rights), I treat it as a potential consequence of regime type (see Chapter 8) rather as one of its defining features.

Finally, the distinction between single-party regimes, military dictatorships and civilian autocracies does not reflect differences in institutional strength; nor does it indicate to which extent power is personalized. The recurring discussions on the institutionalization of party regimes such as the Communist regime in contemporary China could mislead one into thinking that these regimes are less prone to power personalization; but, as stated above, not all ruling parties are able to exert control on the chief executive to the same extent. It is not quite implausible to assume that party regimes are more likely to develop strong institutions on the long run than are, say, military juntas. But, in the absence of any comprehensive comparative analysis of institutional strength across dictatorships, I prefer to refrain from making bold assumptions. This, of course, does not preclude that these two dimensions – the identity of the ruling coalition and the degree of institutional strength of the regime – are partly overlapping; but this is an empirical question, not a working assumption.

Personalism – and its variants, such as sultanism (Chehabi and Linz 1998) or neopatrimonialism (Bratton and van de Walle 1994) – is a recurrent topic in the literature on dictatorships, and, following Geddes (1999), many authors (especially in the international relations literature) have investigated its effects, thereby treating personalist regimes as a distinct type (e.g., Peceny et al. 2002; Peceny and Butler 2004; Reiter and Stam 2003; Pickering and Kisangani 2010; for other applications, see, e.g., Davenport 2007b; Wright 2008; Gurses and Mason 2010). Clearly, the personalization dimension seems to matter. There are yet several reasons why I do not undertake any further distinction between regimes that are personalized and those that are not. The first one is that power personalization is a somewhat ambiguous concept (Svolik 2012, 30-31) that is hard to gauge empirically. Second, as several scholars have noted (Hadenius and Teorell 2007a; Svolik 2012), power personalization is best conceived as a continuum rather than as a dichotomy, and as an attribute that can vary within and across regimes rather than as a distinct type. While, in some regimes, political power is without doubt concentrated in the hands of a single individual (the Dominican Republic under Trujillo or Turkmenistan under Niyazov are paradigmatic cases), many other cases – such as Chile under Pinochet – are more ambiguous (see Geddes 1999; Barros 2002). Furthermore, many regimes may switch from collective leadership to power personalization and back again: the USSR and

the People's Republic of China are typical examples (see Svobik 2012).

The working assumption made here is that all non-institutionalized regimes, regardless of whether they are ruled by a party, the military, or a coalition of both, are to some extent likely to degenerate into personal rule. Again, this does not preclude the possibility that, for example, military leaders are more likely to consolidate their personal power at the expense of their allies than party leaders are:<sup>43</sup> the point is simply that, with regard to the degree of power personalization, there are more differences within each regime category than between these categories.

#### **2.3.4. The origins of institutionalized and non-institutionalized regimes**

To sum up, I identify five regime types that can be sorted into two broader categories. The category of institutionalized regimes is comprised of monarchies and democracies, while the category of non-institutionalized autocracies consists of military dictatorships, single-party regimes, and civilian autocracies. What distinguishes institutionalized regimes from non-institutionalized ones is foremost the existence or absence of a regular procedure for selecting the chief executive. But there is yet another common trait shared by most non-institutionalized regimes: they were often established in a manner that was not only irregular, but also involved the forceful – and often violent – displacement of the previous ruling elite. By contrast, the establishment of institutionalized regimes either occurred gradually or was the result of a negotiation between several groups competing for power. Consider, first, transitions to democracy. Democratization is often the result of a negotiation between authoritarian elites and disenfranchised citizens groups, as the literature on pacted transitions highlights: for example, O'Donnell and Schmitter (1986) work on the third wave of transitions to democracy highlight the role of moderate elite groups (“soft-liners”) in democratization processes. The fundamental lessons that can be drawn from their analysis are, first, that even if mass mobilization sometimes plays a crucial role in the initiation of the transition, democratization cannot occur unless a part of the ruling elite is willing to negotiate with moderate opposition forces; second, that soft-

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<sup>43</sup> Tullock (1987) claimed that on the long run, all juntas tend to shrink to one single individual, but the idea is contested (see Acemoglu et al. 2009).

liners enjoy a pivotal position in the process and are often able to impose a transition on their own terms; and third, that they are mostly able to retain a substantial power in the subsequent regime. More recent works on democratization – although they place a greater focus on the role of mass mobilization – also emphasize the crucial role played by authoritarian elites, and view democratization as the result of negotiations made under the shadow of revolution (e.g., Rosendorff 2001; Boix 2003; Acemoglu and Robinson 2000, 2001, 2006).

Interestingly enough, some studies on democratization depart from the widespread assumption that democratization results from a confrontation between the elites and the disenfranchised, and argue instead that elites sometimes willingly expand political rights for reasons that have more to do with intra-elite competition than with the threat of a revolution. For example, according to Llavador and Oxoby (2004), the expansions of the franchise that occurred in several European countries during the 19<sup>th</sup> century resulted from the need to manipulate politicians' constituencies in order to alter their incentives to implement specific policies. Lizzeri and Persico (2004) adopt a similar approach: in the example they discuss, England gradually expanded the franchise to create incentives for politicians to increase health and infrastructure spending – that is, to provide public goods – in order to deal more effectively with epidemics and other public health emergencies that resulted from urbanization in the 19<sup>th</sup> century. In the same vein, Green (1993) views the parliamentary government of Medieval England as a way to facilitate information transmission between the king and the barons, and as a mechanism that enabled the king to issue costly signals in order to generate compliance (for a related argument, see North and Weingast 1989).

The point is neither that mass mobilization plays no role in transitions to democracy, nor that democratization ever involves threatened or actual violence. The argument is rather that transitions to democracy are often initiated with the consent – if not the active participation – of at least a part of the ruling elite, and often do not involve the forceful removal of the latter. This point is less documented with regard to monarchies, but historical evidence indicates that European monarchies were the result of a gradual evolution of formal rules regulating access to political leadership and the relationships between the king and his allies: for example, hereditary succession gradually replaced

elections in the course of the Middle Age (see Kurrild-Klitgaard 2000; Kokkonen and Sundell 2014). Likewise, contemporary monarchies in North Africa and the Middle East emerged as decentralized systems based on family networks and evolved toward more centralized systems as a result of the state-building process that unfolded during the 20<sup>th</sup> century (Anderson 1991):<sup>44</sup> before then, hereditary succession was seldom practiced, and although it has now become a common practice, the modes of leadership succession are still being negotiated in some contemporary Middle Eastern monarchies (Anderson 1991; Peterson 2001).

By contrast, the establishment of most non-institutionalized regimes is almost by definition the result of a violent power struggle that ultimately resulted in the forceful replacement of previous elites. While there are some exceptions to this general pattern – for example, although Peru under Alberto Fujimori and Russia under Vladimir Putin can be counted as weakly institutionalized dictatorships, both leaders accessed power in a way that did not involve substantial violence – the association between non-institutionalized regimes and violent establishment is practically tautological in the case of military dictatorships (the illegal and forceful replacement of a civilian government being a definitional feature of military coups) and is also clear in the case of party regimes: for example, most communist party regimes were established in the aftermath of a revolution (as in the cases of the Democratic Republic of Vietnam and the Soviet Union), a foreign intervention (as in the cases of North Korea and Eastern European communist regimes), or a combination of both (as in the case of Mongolia). Likewise, the origins of many African party states – often headed by independence leaders – can be traced to wars of decolonization (see Bratton and van de Walle 1994).

This pattern is not merely incidental, and it involves two crucial implications for the dynamics of violent conflicts in non-institutionalized polities. First, the nature of the conflict that resulted in the downfall of the prior regime helps predict how the subsequent regime will operate: this aspect of the problem will be discussed in further detail in

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<sup>44</sup> The fact that most monarchies in North Africa and the Middle East were established recently and had no historical precedents constitutes, according to Anderson (1991), a definitive rebuttal of the culturalist arguments according to which the persistence of monarchies in the MENA countries can be attributed to a form of legitimacy rooted in tradition. Interestingly enough, of the four monarchies that existed in the 19<sup>th</sup> century in the region – Morocco, Oman, Egypt, and North Yemen – two were overthrown shortly after independence (Anderson 1991, 6).

Chapter 3. Second and most importantly, it is precisely the irregularities and violence that characterize the emergence of non-institutionalized regimes that bring about what Svobik (2012) identifies as the two fundamental problems of authoritarian politics, namely the problem of “authoritarian control” and the problem of “authoritarian power-sharing:” in order to ensure their political survival, authoritarian incumbents must deal with two kinds of threats, the first ones coming from regime opponents and the second one from regime insiders. The first problem arises from the forceful replacement of prior elites, which are likely to pose serious challenges to the new regime unless they can be completely neutralized. The second problem arises from the characteristics of the new ruling coalition, because regime elites who have participated in a violent seizure of power are likely to exhibit aggressive behavior. These issues will be dealt with in Chapter 3. Additionally, Chapter 3 will discuss how well-designed institutions can contribute to solving these problems.

**PART I:**  
**THE ROOTS OF POLITICAL CONFLICT**

### 3

## **Institutions and conflicts in institutionalized and non-institutionalized regimes**

As discussed above, the central difference between institutionalized regimes on the one hand and non-institutionalized regimes on the other hand is in the existence of regular procedures for selecting and replacing the effective leader: in monarchies and democracies, leaders enter power in a regular way and have no discretionary power in selecting their successor. But why does it matter? The central argument that will be made in this chapter is that institutionalization has a decisive impact on the preferences and behavior of the ruler and his ruling coalition. Specifically, I argue that institutionalization has the potential to mitigate the two central problems that plague intra-elite relationships in the typical dictatorship, namely the problem of adverse selection and the problem of uncertainty. By affecting the stability of the regime at the elite level, institutionalization has broader consequences, to the extent that it will also partly determine the behavior of the military.

The chapter is organized as followed: first, some crucial assumptions about relevant actors, their preferences, and their behavior will be specified. It will then be shown how political institutions have the potential to mitigate conflicts and allow for their peaceful resolution. Finally, some expectations that are relevant for the remainder of the dissertation will be discussed: the main implications of this discussion are, first, that the nature of political conflicts is fundamentally different in institutionalized and non-institutionalized regimes; and, second, that specific institutions have distinct effects depending on the type of conflict.

### 3.1 SOURCES OF POLITICAL CONFLICT IN INSTITUTIONALIZED AND NON-INSTITUTIONALIZED REGIMES

#### 3.1.1. Sympathy for the devil? The retirement problem in non-institutionalized regimes

Many works on dictatorship begin with (or implicitly rely on) the assumption that dictators want to stay in power (see, e.g., Tullock 1987; Geddes 1999; Gandhi 2008a; Svobik 2012). This assumption might seem trivial – after all, even elected leaders in democracies probably prefer winning elections over losing them – unless its hidden implication is made more explicit: contrary to democratically elected leaders, dictators want to stay in power *at all costs*. For example, Alvarez et al. (1996, 12) remark that “the dream of many political elites is to rule perpetually and to rule with consent.” Since there is an obvious trade-off between both objectives, what distinguishes dictators from democrats becomes clear: after a certain time in office, most leaders have to decide whether to stop ruling or to start ruling without consent. Dictators are those who make the second choice.

Empirically, it seems that while all rulers have to balance these two conflicting goals against each other, not all of them choose the same exchange rate: the relative payoffs of ruling perpetually and ruling without massive opposition appear to vary from one ruler to another. While most democrats and even some autocrats – like Ghanaian ex-military ruler and president Jerry Rawlings<sup>45</sup> – step down in a peaceful manner, some others are willing to risk jail, exile, or, in the most extreme cases, death. This, in turn, begs another question: What explains dictators’ unwillingness to yield office?

Recent scholarship offers a simple explanation for why, for most dictators, retirement is simply not an option: while democrats, on average, fare relatively well after they lose office, a substantial proportion of dictators face severe punishment such as exile, jail, or

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<sup>45</sup> Rawlings seized power in a coup in 1979 and ruled as a military leader for more than a decade. He then held multiparty elections – in which he ran as a civilian – and won twice (in 1992 and 1996). In 2000, having spent his two constitutionally allowed terms, he voluntarily stepped aside. See Polgreen, Lydia. 2009. “Ghana's unlikely democrat finds vindication in vote.” *New York Times*, January 9, 2009, available at [http://www.nytimes.com/2009/01/10/world/africa/10rawlings.html?pagewanted=all&\\_r=0](http://www.nytimes.com/2009/01/10/world/africa/10rawlings.html?pagewanted=all&_r=0) (accessed May 2, 2015); Comarin, Elio. 2000. “Captain Rawlings tire sa révérence.” RFI, December 7, 2000, available at [http://www1.rfi.fr/actu/fr/articles/012/article\\_4903.asp](http://www1.rfi.fr/actu/fr/articles/012/article_4903.asp) (accessed May 2, 2015).

death. Goemans et al. (2009a) report that nearly half of dictators who lost office during the post-World War II period suffered some kind of post-tenure punishment. This, in turn, predicts how far dictators are willing to go to avoid losing office: for example, a series of empirical studies (Goemans 2000, 2008; Debs and Goemans 2010) finds that their anticipated post-tenure fate affects leaders' propensity to initiate or continue external conflicts.

This literature is somewhat less specific about what exactly determines the severity of post-tenure punishment. Regime type is one of the commonly cited factors: for example, Debs (2010) finds that because of the threat they represent for subsequent rulers, military leaders are more likely to be imprisoned or killed upon exit, unless they manage to hand power to civilians and negotiate a transition to democracy. Using Cheibub et al.'s (2010) data, Debs and Goemans (2010) find that military dictators face a significantly higher risk of jail, exile or death than civilian dictators. Goemans (2000) argues that leaders of semi-democratic (or semi-authoritarian) regimes face roughly the same risk of post-tenure punishment as dictators, but are also more likely to be removed from office in the first place. The factor which seems to be the most tightly associated with post-tenure fate, however, is the way leaders lose office: Goemans (2008, 780-1) reports that almost all leaders who lost office in a regular way (i.e., in accordance with rules or established conventions) enjoy a safe retirement, while 80% of those who were removed from power in an irregular manner suffered some form of punishment during the year immediately following their exit.

In turn, the risk of being removed from office in an irregular fashion seems to depend foremost on the manner in which the dictator accessed power in the first place (Goemans 2008; Goemans et al. 2009a). Why dictators who entered office by unconstitutional ways are more likely to lose it in an irregular manner and to suffer subsequent punishment is not difficult to understand. First, accessing power in an irregular manner means overthrowing the previous ruling coalition, and thus requires being able to neutralize all remaining potential challengers: a failure to do so increases the dictator's risk of being himself deposed by the contenders he once removed from power.<sup>46</sup> Second, challengers who succeed in removing the dictator from office obviously face similar incentives to eliminate

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<sup>46</sup> Other reasons why those dictators are more insecure in office are discussed in the next section.

their former rival, especially if this rival accessed power by force: rulers who once overthrew a government are susceptible – that is, both willing and able – to do it again, and cannot credibly commit to comply with the new regime (see Debs 2010).

Curiously, the links between leaders' anticipated post-tenure fate and the use of force against domestic opponents have received less attention than the mutual relationship between the risk of punishment and the initiation of diversionary wars. There is, however, evidence that (1) democrats – who, by definition, have accessed power in a regular manner, and are less likely to suffer punishment after leaving office – are *ceteris paribus* less likely than autocrats to use state repression (Davenport and Armstrong 2004; Davenport 2007a); (2) state repression “works” in the sense that it lengthens autocrats' tenure (Escriba-Folch 2013a) and prevents regime transition (Albertus and Menaldo 2012); (3) military dictators are more likely to have accessed power in an irregular manner and more likely to leave office in an irregular manner, more likely to face a harsh post-tenure punishment (Debs 2010; Debs and Goemans 2010),<sup>47</sup> and more likely to engage in physical integrity rights violations (Poe et al. 1999; Davenport 2007b). Combined with the overwhelming consensus that the use of repression primarily depends on the magnitude of threats leaders face (Davenport 2007a), this accumulation of empirical evidence is on the whole consistent with a view of violence as dictators' desperate response to threats to their own physical integrity.

Yet, the causal arrow might well run the other way around: an alternative – and equally plausible – explanation for the correlation between irregular entry, violence, irregular exit,

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<sup>47</sup> Debs' (2010) study provides an interesting explanation for why military dictatorships are both less durable and more likely to democratize than other dictatorships. While standard explanations rely on the argument that military leaders are less interested in being in power and more likely to secure rents after leaving office (Geddes 1999), Debs (2010) argues that this is precisely their capacity to secure rents that makes military leaders most likely to suffer post-tenure punishment, since this also implies that they should pose the greatest threat to the new leader. This creates an incentive for military leaders to hand power to civilians and negotiate a transition to democracy. He consistently finds that former military dictators face the worst fate after leaving office, unless the new regime is a democracy. Yet, this finding begs further questions: while it is a well-established fact that a large proportion of military rulers have negotiated their exit with civilian elites and participated in the regime's peaceful transition to democracy (see, e.g., Geddes et al. 2014), it is not entirely clear (theoretically speaking) why democracy – rather than another dictatorship – should help former military leaders secure a safe exit from office. Contrary to dictatorships, democracies cannot insulate themselves from demands of their voters, and these are the most likely to want former dictators to be punished. Furthermore, pacted transitions are inherently undemocratic processes: there is no obvious reason why such arrangements cannot be negotiated with another dictator. In short, while empirical evidence suggests that democratic leaders do not feel the need to eliminate former dictators, and that retiring dictators anticipate this and tend to prefer democratization over transition to another authoritarian regime, why it is the case remains mysterious.

and post-tenure punishment, is that dictators who enter office illegally and use violence to stay in power are simply more likely to end up being overthrown and punished for their behavior.<sup>48</sup> These explanations do not differ strikingly in their consequences, but they imply different underlying assumptions about dictators' primary motivations. While causal path A suggests that dictators who access power through irregular or violent means end up "stuck" in office – knowing that they cannot relinquish power without risking jail, exile or death – and have no choice but to resort to extreme measures to protect themselves, causal path B assumes that dictators could choose to yield office peacefully but that they enjoy power so much that they are willing to use every possible strategy to stay in office, regardless of the risk of being subsequently punished.<sup>49</sup> In short, both explanations rely on different assumptions about dictators' average level of risk acceptance. But then again, they do not differ in their consequences: irregular seizures of power initiate a vicious circle in which dictators often have no choice but to use violence to maintain themselves in office; and the use of violence makes peaceful retirement virtually impossible. In short, in non-institutionalized regimes, violence breeds violence.

### **3.1.2. Leaders and their ruling coalitions**

Intra-elite conflicts – either in the form of “palace coups” or of subversive coalitions made up of factions of the military and the ruling elite – are one of the main sources of variation in the political survival of autocrats (Goemans et al. 2009a; Cheibub et al. 2010; Svobik 2012). While research shows that democracies are not immune to such phenomena (see, e.g., Maeda 2010), coups, purges and other forms of power struggles are the modus operandi of many authoritarian governments (Tullock 1987).

Predicting conflicts between rulers and their coalition requires making general

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<sup>48</sup> There is a similar debate ongoing in the diversionary war literature: while the risk of post-tenure punishment creates an incentive for dictators to initiate or continue interstate wars even if they cannot win (Goemans 2000; Goemans 2008; Debs and Goemans 2010), there is also evidence that the fate of dictators is more sensitive to war outcomes than the fate of democrats (Debs and Goemans 2010) and that losing a war increases the risk of post-tenure punishment (Goemans 2008; Escriba-Folch 2013b).

<sup>49</sup> Neither explanation is entirely satisfying. If causal path A is correct, it is unclear why dictators want to be in power to begin with. If causal path B is correct, it is unclear why dictators do not take the opportunity to leave office peacefully while they have the chance.

assumptions about the preferences of key regime elites. To begin with, it can either be assumed that members of the ruling coalition want to become rulers themselves, and that, given an opportunity to overthrow the incumbent and replace him, they will seize it. Alternatively, it can be assumed that they are not interested in taking power (or at least not enough to engage in a risky conspiracy). But both assumptions are equally unsatisfactory, because they do not explain why, for example, the current president of France runs a comparatively low risk of being overthrown by his own defense minister whereas such situations happen fairly often in the average dictatorship (Tullock 1987; Cheibub et al. 2010; Svobik 2012).

A way of making sense of these variations is to assume that the preferences of higher regime officials (and, in turn, their relationships with the current incumbent) are partly determined by the political environment in which they operate: this is the approach I will take. More specifically, I argue that intra-elite relationships in non-institutionalized autocracies are plagued with two interrelated problems: an uncertainty problem and an adverse selection problem. While institutionalized regimes have managed to mitigate these two issues, their prevalence in newly established autocracies explains why leaders in these regimes run a higher risk of being overthrown by their own allies. Each of these mechanisms will be discussed in turn below.

The first argument I put forward is that regime type has an impact on the personality of individuals who self-select into the ruling coalition. Adverse selection is a recurring topic in organizational economics and has received some attention in the conflict literature (e.g., Weinstein 2007), but is still a comparatively understudied issue in the cross-national, comparative literature on political regimes in the sense that we still lack thorough empirical investigations (for a notable exception, see Besley and Reynal-Querol 2011). The topic is nonetheless ubiquitous: for example, while discussing whether democracy is an appropriate method for selecting effective governments, Schumpeter (2003 [1942], 288-9) examines the argument that selecting individuals who are successful at the polls is not the most straightforward way to fill political offices with competent statesmen and administrators, and that good tacticians are also more likely – once in office – to avoid being punished for their incompetence. With regard to nondemocratic regimes, Tullock (1987, 18) bluntly states that self-selection processes in dictatorships result in dictators

being frequently both intelligent and aggressive, while “accidents of gene selection” in hereditary monarchies often bring inept kings in power. More recently, Egorov and Sonin (2005) propose an explanation for why (personalist) dictators tend to be surrounded by incompetent advisors: first, the dictator himself has an incentive to select only individuals who are (comparatively) unlikely to betray him; second, dictators – contrary to democrats, who are constrained by laws – are usually unable to commit not to punish potential contenders, which discourages most talented individuals from entering the government. In a rare cross-national examination of leadership selection in democracies and dictatorships, Horowitz and Stam (2014) show that, while democratic leaders with prior war experience are less likely to pursue aggressive foreign policies than those without military background, the reverse is true of dictatorships: severely autocratic regimes, they argue, are more likely to reward individuals who reacted to combat experience by developing aggressive and risk-acceptant attitudes.<sup>50</sup>

Selection effects can partly explain the well-established fact that dictators are most likely to be overthrown and replaced by one of their allies (see, e.g., Tullock 1987; Gandhi 2008a; Cheibub et al. 2010; Svobik 2012). In newly established dictatorships, the ruling coalition is typically made up of a dictator and his supporters, most likely military members and former supporters of the previous incumbent, who coalesced to overthrow the previous incumbent and took over power by force. In most extreme cases, the new coalition can be made up of complete outsiders (for example, in the case of “rebel regimes” such as Fidel Castro’s regime in Cuba), but either way, the very process through which it attained power requires a fair amount of risk acceptance: elites of the prior regime freely made the choice of joining a plot, thereby risking failure and subsequent punishment. With regard to open rebellion, failure can easily result in the death of its leaders. If coup plotters are willing to run such high risks, they presumably expect rewards that outweigh these risks: knowing that the most frequent path to becoming a dictator is to rise in the governmental hierarchy (Tullock 1987, 131; Cheibub et al. 2010), one obvious reward elites can expect is either succeeding to the first dictator or having the opportunity to overthrow and replace him. Thus, in newly established dictatorships, ruling coalitions are likely to be populated by individuals who are both motivated by the prospect of

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<sup>50</sup> Colgan (2013) puts forward a similar argument with regard to revolutionary leaders.

becoming dictators and willing to take the risk.

This, in turns, leads to the second aspect of the problem, namely structural uncertainty. Tullock (1987) pointed out that in any authoritarian elite group, there is always the possibility of a subversive coalition forming against the dictator: if a coup is attempted, individuals who supported whoever wins at the end are rewarded, and those who backed the loser are punished. Furthermore, for members of the ruling coalition, remaining neutral is rarely an option: first, the winner is likely to interpret hesitation as treason; second, neutral members can lose office simply because their position is needed for rewarding more active supporters; third, insecure dictators tend to distribute high rewards for loyalty and harsh punishment for treason, which in turn generates an incentive for regime officials to denounce false conspiracies in order to improve their own position at the expense of potential rivals. Thus, members of the ruling elite live under constant threat coming from the sitting autocrat, his allies and his potential contenders. Knowing that their current position is fundamentally of temporary nature, they hence face a high incentive to grasp at any opportunity to seize power: each member who succeeds in accumulating enough power to overthrow the incumbent might be tempted into doing just that, not only because he has the opportunity to do so, but also because he faces a greater risk that the dictator or one of his rivals turn against him.

The uncertainty problem is somewhat alleviated under what Svobik (2012) calls “established autocracy” (i.e., regimes in which there is a sharp power imbalance between the leader and his allies). Svobik (2012) shows that these established autocrats become less likely to be overthrown by a member of their inner circle as their tenure increases (see also Bienen and van de Walle 1989, 1992). The explanation is that, when the autocrat appears to be more secure in office, potential challengers might find it more difficult to overcome their coordination problems: absent a clear signal that a substantial number of higher officials would back an attempt to overthrow the autocrat (or at least refrain from betraying the plot), they find themselves unable to recruit allies because other members of the ruling elite fear that the coup may fail (see also Tullock 1987). But another implication is that, when the incumbent seems to be unlikely to lose office in the near future, his allies have less to worry about being overthrown together with him and are therefore less incited to take preemptive measures: on the long run, the mechanism reinforces itself.

Some authors argue that formal institutions can facilitate power-sharing agreements between the dictator and his allies when political power is more evenly shared between them, and thus prevent instability at the elite level (see, e.g., Magaloni 2008; Köllner 2013; Svulik 2012). Most of these studies have focused on the role of parties in facilitating bargaining between the dictator and his ruling coalition, and consistently found these to increase authoritarian regimes' duration (Geddes 1999; Magaloni 2008; Svulik 2012). For example, Magaloni (2008) argues that parties enable dictators to credibly commit to reward their allies if they remain loyal to the regime: by delegating their control on political appointments to the ruling party, dictators can establish some credible limits to their own ability to renege on their promises, and thereby generate incentives for elites to invest in the survival of the current regime instead of plotting against it (for a variant of this argument, see Svulik 2012). The obvious problem is that it remains so far unclear whether the stabilizing effect of parties can be generalized to the whole universe of party-based regimes: the analyses mentioned above tend to draw general conclusions from a small subset of cases, typically China, the Soviet Union or the PRI regime in Mexico. In the absence of comprehensive, global analyses of power allocation mechanisms in ruling parties, it is not entirely clear whether the longevity of party regimes can be attributed to the role of institutions: for example, Magaloni's (2008) argument about credible power-sharing agreements rests on the assumption that parties have the power to appoint members into government positions, but it is unclear how many ruling parties really have this power. Yet, Magaloni's main point can still be generalized to other regimes: if the argument is correct, it implies that rulers who do not have the power to select their successors – typically, monarchs or elected leaders – should be less likely to be overthrown by their allies.

All in all, this suggests that “established autocracy” (Svulik 2012) is the first step toward stability at the elite level, to the extent that it mitigates the uncertainty problem. The second step consists in mitigating adverse selection problems: as discussed above, leaders are less likely to be overthrown by their allies if they manage to avoid being surrounded by higher officials whose primary goal is to become dictators themselves. This can be achieved through delegation, that is, if leaders give up their power to appoint their own successor. The reason is that, in weakly institutionalized or “anarchic” dictatorships, the

most effective way of becoming a dictator is to be part of the inner circle of the current incumbent (Tullock 1987). If, by contrast, the incumbent has given up his power to appoint his successor, being part of the incumbent's inner circle does not guarantee access to power, since succession in office depends on other criteria (e.g., succession orders in hereditary monarchies, or elections in democracies). Thus, if succession in office is regulated by formal rules, key regime positions are more likely to be filled by individuals whose primary goal is not to become rulers themselves but rather to pursue their own career path: potential challengers will not necessarily be part of the ruling coalition, since challenging the regime from the inside does not make success more likely.

To sum up:

- (1) Rulers who attained power by illegal or violent means are the most likely to be overthrown by their allies, because they will be surrounded by ambitious and risk-seeking individuals who are, furthermore, uncertain about their own future.
- (2) As rulers succeed in consolidating their own power, uncertainty problems are somewhat mitigated and coup risk decreases.
- (3) Formal rules regulating access to, and succession in power mitigate both uncertainty and adverse selection problems.

Three final points need to be discussed. First, power concentration and institutionalization seem to be inherently contradictory. However, as will be shown in Section 3.2., establishing formal institutions (e.g., hereditary succession) typically involves a risk for the ruler, since it deprives his allies from the possibility to attain power in the future: such measures are thus most likely to be taken by rulers who are already relatively secure in office.

Second, as has been pointed out above, the combination of uncertainty and adverse selection creates a vicious circle: overt instability at the elite level is likely to attract ambitious and opportunistic individuals, and the overrepresentation of such individuals in key regime positions in turn aggravates instability issues. Both effects are mutually reinforcing. By the same token, regime stability creates a virtuous circle: knowing that the incumbent is unlikely to be overthrown not only reassures elites on their own fate (thereby decreasing the risk of "preemptive" coups) but also reinforces their belief that even if they wanted to attempt a coup, they would not be able to find allies to do so.

Third and finally, the impact of formal institutions might seem overstated in light of what has been discussed about the relationship between rulers and their allies: why should they be expected to abide by the rules? This will be the topic of Section 3.2.

### **3.1.3. Regime elites and the opposition**

A second common form of violent political conflict is armed struggle taking place between the incumbent and one or several opposition groups. This formulation already conveys an assumption that is often implicit but nonetheless pervasive in prior cross-country investigations of the relationship between political regimes and intrastate armed conflicts (e.g., Muller and Weede 1990; Hegre et al. 2001; Fjelde 2010; Gurses and Mason 2010): that civil wars can be conceptualized as political conflicts that have become violent. To state the matter differently, expecting political regimes to have an influence on intrastate armed conflict requires assuming that such conflicts are (at least partly) motivated by political issues. This might seem self-evident, but, as will be discussed below, one persistent debate in the comparative literature on civil wars revolves around the question of whether these conflicts are political in essence, or are simply economic conflicts, analogous to organized crime (Collier 2000).

In spite of the well-known shortcomings of “grievances-based” approaches (see Collier and Hoeffler 1998, 2004; Fearon and Laitin 2003), I still assume that intrastate armed conflicts can break out over issues related to the distribution of political power. The assumption I make here is that the preferences of opposition groups mirror those of the incumbent and his allies: what is at stake in intrastate conflicts is political power, that is, control over the state’s government or a portion of its territory. There is a “soft” version and a “hard” version of this assumption: the soft version states that opposition groups tend to rebel when they have been submitted to an unwanted leader for too long. The hard version states that, regardless of leadership changes that occur at the government level, opposition groups will still resort to violence unless they have a fair chance of attaining power themselves. Importantly, these assumptions are completely agnostic about what fundamentally motivates rebel leaders. They can be motivated by personal ambition or greed, or by genuine political, religious or otherwise ideological beliefs; like incumbents, they can be office-seeking or policy-seeking. But either way, in order to be able to

implement their preferred policies or capture the state’s income, they first have to be in power, which requires seizing control of the central government or of a sizable portion of the state’s territory.

Treating rebels’ preferences as relatively uniform across cases has some implications for the conceptualization of armed rebellion, since it implies that all intrastate armed conflicts, in spite of their differences, have some common causes. One consequence is that, although there is a relative<sup>51</sup> consensus among scholars that not all civil wars are the same, most of the hypotheses I will test throughout the dissertation pertain to all types of conflicts: I do not distinguish between, for example, conflicts over government from conflicts over territory, or ethnic from non-ethnic civil wars.

Table 3.1. gives some examples of distinctions that are commonly undertaken in the civil conflict literature. The first striking point is that civil conflicts can be distinguished along numerous dimensions that are sometimes overlapping but nonetheless conceptually and empirically different: for example, while separatist conflicts are mostly fought by ethnic minorities, almost half of ethnic conflicts concern government control (Buhaug 2006, 694); territorial conflicts are not necessarily fought over separatist purposes (de la Calle and Sanchez-Cuenca 2012, 582); nor are they necessarily rural (Staniland 2010).

**Table 3.1. Types of civil war in the comparative conflict literature**

<i>Type of civil war</i>	<i>Example</i>
Ethnic vs. non-ethnic civil war	Sambanis 2001b
Urban vs. rural insurgency	Staniland 2010
Territorial vs. non-territorial conflict	De la Calle and Sanchez-Cuenca 2012
Governmental vs. separatist conflict	Buhaug 2006

Given the many dimensions along which intrastate conflicts can be differentiated and the lack of a clear theory that would link political regimes to each of these “conflict types,”

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<sup>51</sup> The consensus is not perfect since proponents of the “greed” approach typically rely on aggregated data and treat all civil wars as fundamentally similar (Collier and Hoeffler 1998, 2004; Fearon and Laitin 2003). More curiously yet, many empirical studies that investigate the impact of political regimes on civil wars do not undertake any distinctions between different types of conflicts (Hegre et al. 2001; Reynal-Querol 2002a, 2005; Regan and Bell 2010; Gleditsch and Ruggeri 2010; Fjelde 2010; for an exception, see Reynal-Querol 2002b).

one is left with a choice between two equally unsatisfactory options: the first option consists in working with aggregated conflict data under the assumption that political regime has roughly the same influence on all types of intrastate armed conflict; the second option would be to adopt a purely inductive approach and investigate whether, say, monarchies are more prone to non-ethnic, territorial conflicts over government while military dictatorships are more vulnerable to ethnic, urban insurgencies without rebel territorial control. But there is yet a reason not to take these distinctions into account: there is growing evidence that these differences between types of civil war are not due to fundamental differences in rebels' objectives and preferences, but rather to the balance of power between government and rebel groups. In short, power asymmetry determines which form the conflict will take: relative capabilities, for example, determine whether conflicts are likely to be fought in urban or rural areas (Staniland 2010) or whether the rebels will resort to insurgency or terrorism (de la Calle and Sanchez-Cuenca 2012). With regard to the commonly made differentiation between governmental and territorial conflict, Buhaug (2006) finds that whether rebel groups seek state control or self-determination depends on their strength relative to the government: while weak states tend to attract revolutions, rebel groups that challenge stronger states anticipate that they will never be able to seize control of the central government and are thus more likely to claim independence for a limited portion of the state territory. This does not only explain the fact that some states are more prone to governmental conflicts than others, but also the evolution of claims made by individual rebel groups: demands might shift from independence to regime change and back again, depending on rebel groups' changing expectations about the outcome of the conflict (see pp. 694-5).<sup>52</sup>

A fundamental lesson that can be drawn from these studies is that the causes of civil conflict outbreak are not the same as the causes of conflict escalation, conflict intensity, and conflict continuation or termination. This distinction is not trivial: First, it has crucial implications for the research design, and particularly for the choice of the operational definition of intrastate armed conflict (see Chapter 5); second, it is a highly relevant aspect of the debate about the relative impact of "greed" and "grievances" on civil conflict.

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<sup>52</sup> A more radical conclusion that can be drawn from Buhaug's (2006) findings is that the stated goals of the rebellion should never be taken at face value (i.e., that claims over government control or independence are fundamentally a question of discourse and have a very limited explanatory value).

A series of studies by Collier and Hoeffler (Collier 1999, 2000; Collier and Hoeffler 1998, 2004; Collier et al. 2008) have highlighted the role of economic and material benefits that rebel leaders gain from civil war and hence severely challenged the “traditional” explanations of rebellion: in short, armed conflicts are not motivated by grievances (because these are so ubiquitous that they cannot account for the relative rarity of rebellion) but by greed, and provided that rebellion is feasible, the opportunity will never be passed up. One version of the “greed” hypothesis is based on the assumption that expected post-conflict payoffs are the primary cause of civil conflict: Collier and Hoeffler’s (1998) main argument is that the motivation of rebel groups is to capture the state’s revenues. Similarly, Collier (2000) puts forward that criminal violence perpetrated by gangs and mafias and large-scale conflicts between insurgents and the state are located on the same continuum: what makes rebellion a distinctive form of crime is, first, the size of the group engaged in such activities, and second, the type of goods (that is, natural resource exports) the group seeks to loot. A later, and more radical version of the greed hypothesis (Collier and Hoeffler 2004) is that sustained conflict is not due to rebels’ expected payoffs in case of victory, but rather to the fact that civil war provides some groups with opportunities to enrich themselves in the course of the conflict. As Collier puts it:

Various identifiable groups will “do well out of the war.” They are opportunistic businessmen, criminals, traders and the rebel organizations themselves. The rebels will do well through predation on primary commodity exports, traders will do well through widened margins on the goods they sell to consumers, criminals will do well through theft, and opportunistic businessmen will do well at the expense of those businesses which are constrained to honest conduct. If some people do well out of civil war they may not be particularly concerned to restore peace (Collier 1999, 9–10).<sup>53</sup>

At first glance, this latter approach seems to be inherently incompatible with the idea that political regime type has an impact on civil conflict: if what motivates rebel groups is the prospect of enriching themselves during the war, there is no reason why we should expect

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<sup>53</sup> Reasons why these particular groups have better opportunities to enrich themselves are discussed throughout the paper. Civil war benefits criminals because government expenditures on the police decrease as military expenditures increase; it benefits some businessmen because it disrupts economic competition; and it obviously benefits rebel groups that are able to seize control over lootable resources.

them to be more or less opportunistic under democracy than under monarchy or military dictatorship. If civil war has political causes, these causes have to be located in factors such as weak statehood (see, e.g., Fearon and Laitin 2003) rather than in the institutional setup of the regime.

There are, yet, some reasons to believe that both approaches are not mutually exclusive. Most of the greed-based arguments actually pertains to the sustainability of civil conflict rather than to its outbreak: violent conflicts, so the argument goes, only reach the level of full-scale civil war when rebels are able to recruit combatants, provide them with weapons, and finance the insurgency. In other words, the greed approach to civil war does not really seek to explain what sparks violence but rather what makes violence escalate and last. Several studies, furthermore, suggest that the motivations of the combatants and the strategies they pursue might evolve in the course of the conflicts, depending on the intensity of the conflict: for example, Regan and Norton (2005) argue that, since initial mobilization entails few costs for the participants, grievances may be sufficient to spark low-level unrest; yet, as the state becomes increasingly repressive in response to the threat, rebel leaders must resort to providing combatants with selective benefits (see also Weinstein 2007). Relatedly, while reviewing the available empirical evidence on natural resources and civil war, Ross (2004a) notes that “lootable” resources such as drugs and gemstones seem to be related to the duration of armed conflicts (the explanation being that in resource-rich states, war becomes so profitable that combatants gradually lose their incentive to reach a peace settlement), but not with the onset of armed conflict. In a detailed analysis of thirteen cases of civil war in resource-rich countries, he further observes that in all of these cases, rebel groups only started to loot resources after the conflict began (Ross 2004b): this finding suggests that, while looting is necessary for combatants to finance the insurgency, it does not necessarily constitute their primary goal at the outbreak of the conflict.

To sum up, understanding how conflicts escalate and last requires taking collective action problems and the self-interests of rebel leaders into account. What makes violence break out, by contrast, is not necessarily greed. For the purposes of the empirical investigation, I assume that the risk of intrastate armed conflict increases when opposition groups are durably excluded from power and believe they would have a fair chance to prevail in a

civil war (more on this latter point in Chapters 5 and 7). A crucial implication is that not all institutional features of political regimes should be related to the occurrence of intrastate conflict. More specifically, since democracies are the only systems in which opposition groups strong enough to pose a serious threat to the central government are unlikely to be durably excluded from power, I expect democracies to experience fewer intrastate armed conflicts than any other regime type. By contrast, while I expect regime institutionalization to reduce the risk of intra-elite conflicts and military coups, I do not expect it to have any direct effect on rebellion: if this expectation is correct, monarchies should display the same vulnerability to intrastate conflicts as any other nondemocratic regime.<sup>54</sup>

### **3.1.4. Incumbents and the military**

If there is one lesson to be learned from the Arab Spring, it is the following: virtually no regime can maintain itself without the active support of the armed forces. The military's behavior is perhaps the ultimate factor that determines the success or failure of popular uprisings: revolutions tend to fail whenever the armed forces remain loyal to the regime, and to succeed whenever they defect and switch to the opposition's side (D'Anieri 2006; Barany 2011; Croissant 2013a; Nepstad 2013). For many authoritarian regimes, the military constitutes the main "pillar" of stability (Gerschewski 2013); yet, it tends to defect quite often. Coups account for the overwhelming majority of irregular changes of leadership and regime transitions in nondemocratic countries (Svolik 2012); and even when alternations through elections are taken into account, coups account for almost one tenth of all leadership changes (Goemans et al. 2009a).

But what, then, explains why military coups are pervasive in some countries while they are virtually absent from some others? General explanations are never entirely satisfying because of the huge diversity of armed forces, and because the military, even when it is highly cohesive and professionalized, cannot be viewed as a monolithic entity. Assigning preferences to the military as if it were a homogeneous organization is thus inherently problematic. Yet, for the reasons mentioned above, leaving the military out of the picture is not a conceivable alternative.

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<sup>54</sup> As we shall see, however, this expectation is not borne out.

For the remainder of this subsection, I will proceed in two steps. I will first provide a very brief and non-exhaustive review of theoretical approaches that attempt to provide a generalizable explanation of coups. These theoretical approaches fall into three general categories: a corporate-interests approach, a class-conflict approach, and an opportunity approach. As will be shown, however, while these explanations may contribute to a more thorough understanding of individual events, none of these can perfectly account for the cross-country variations in the frequency of coups. Next, I will discuss some recurring findings of previous studies on coups, and draw some conclusions about how the military is likely to behave in specific political environments.

#### 3.1.4.1. General approaches to the explanation of military coups: Corporate interests, class conflicts, and opportunities

Corporate grievances have been repeatedly emphasized by early studies on military coups (Finer 1962, Decalo 1973, Needler 1975, Nordlinger 1977): the military intervenes either to pursue specific demands (e.g., better pay, easier promotion, less political interference in appointments or other military affairs) or to hinder what it perceives as threat to its integrity. The most radical proponent of this approach, Nordlinger (1977), goes as far as to say that “almost all coups are at least partly, and usually primarily, inspired by the military’s own interests” (p. 192). He reports some striking examples: for instance, in the period from 1912 to 1964, every Peruvian government that attempted to reduce military expenditures ended up overthrown; one coup was even justified by the statement that the government “had not built even one military base” (p. 67).

The corporate-interests approach has been criticized for crediting coup plotters with too much concern for the whole organization’s utility and overlooking their own personal ambitions: as Bienen notes, “Military factions can be little more than gangs of thugs (...) willing to risk the stability of the state and the military’s own cohesion and identity to stay in power” (Bienen 1981, 368). A related issue is that numerous cross-national studies have failed to uncover any clear relationship between military government and defense spending patterns (see, e.g., McKinlay and Cohan 1975, 1976; Zuk and Thompson 1982): if coup plotters are motivated by a genuine desire to improve the military’s status and capacities, it

is then hard to explain why they do not act accordingly once they have taken power.

The most serious problem, however, is that corporate grievances fail to explain the cross-national variations in patterns of civil-military conflicts: in short, conflicts between the military and civilian governments are so pervasive that one may wonder why coups do not occur all the time. For example, Clinton and the US military quarreled endlessly about the status of homosexuals and the inclusion of women in fighting units, among others (see Desch 2001, Chapter 3); yet, it seems reasonable in hindsight to assume that the Clinton administration did not face any serious risk of being overthrown. By contrast, in some other contexts, coups can occur for reasons that seem entirely futile, as in this account of the overthrow of the Bolivian interim government in 1980:

Relations between the military and the Gueiler government were always tense (...). At one point, in the Spring of 1980, the rivalry became almost comic. Garcia Meza complained bitterly to President Gueiler during an official ceremony in Cochabamba that the NCO academy there did not have a swimming pool and that this was an affront to the honor of the armed forces. In front of the press, the president placed her hand on the general's arm and said soothingly, "Alright, General, we'll build you a pool, and then you won't have to overthrow the government after all."... On 17 July, in the early hours of the morning, the garrison at Trinidad rose against the government." (Farcau 1994, quoted in Goemans et al. 2009b, 272).

Thus, explaining coups would require identifying the reason why the military is sometimes willing to use force to protect its interests and sometimes not.

Class-conflict approaches to military intervention face similar issues. They are based on the assumption that military interventions result from redistributive conflicts between the pro-democratic poor and the anti-democratic elites (see, e.g., O'Donnell 1973, Acemoglu and Robinson 2006; for a review and critique of early class-conflict approaches to coups, see Thompson 1975). The ideology of the military is either assumed away (Acemoglu and Robinson 2006) or viewed as generally conservative (O'Donnell 1973; Nordlinger 1977).<sup>55</sup>

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<sup>55</sup> Although Nordlinger (1977) generally portrays military officers as ideologically conservative, he offers an interesting alternative explanation of one paradigmatic case of coup motivated by class conflict, namely the Chilean coup of 1973. According to him, what sparked the coup was not the military's opposition to socialist economic objectives, but rather the fact that the government encouraged the formation of leftist organizations who not only began to arm themselves and factory workers, but also allegedly attempted to infiltrate the military (Nordlinger 1977, 76-77). Thus, the defense of corporate interests, rather than

Class-conflict explanations of military coups are still discussed in the contemporary literature (see, e.g., Tusalem 2010, Hiroi and Omori 2014; for a critique, see Slater et al. 2014). Still, they are clearly at odds with one emerging finding, that most coups since the end of the Cold War have resulted in a transition to democracy (Marinov and Goemans 2014; Croissant 2013b; Thyne and Powell 2015; Powell 2014a). Furthermore, they seem only applicable to a small subset of cases: in his account of coups in West Africa, Bienen (1985) note that those were led by junior officers or NCOs, most of which were allegedly motivated by a general discontent about current elites, policy failures and socio-economic and ethnic inequalities. Many of the subsequent regimes had a clear anti-elite, populist tone. On the whole, the range of political motives underlying military intervention is too broad for ideological explanations to have any predictive power: while reviewing prior research on coups, Zuk and Thompson (1982) note that coup plotters have been alternatively described as “opportunists, revolutionary reformers, modernization vanguards, middle-class political surrogates, constitutional guardians, reactionary oligarchs, or corporate military welfare defenders” (Zuk and Thompson 1982, 60; see also Needler 1975). Even though most post-coup announcements include a policy program, the emphasis on political motives, rather than reflecting a genuine desire to reform the political system, may in fact result from the necessity to either form alliances with civilians or to provide an ex-post justification of military interventions (Tullock 2005 [1974]; Thompson 1975; see also Nordlinger 1977). As Thompson (1975) puts it, “One could hardly expect a coup leader to proclaim to his troops, his countrymen, and the world that a president has been abruptly removed in order to save the coup leader’s political skin” (Thompson 1975, 467).

Finally, opportunity approaches to coups are prominent in the early literature (see, e.g., Finer 1962) as well as in more recent scholarship, as demonstrated by the renewed interest in coup-proofing strategies (Quinlivan 1999; Belkin and Schofer 2005; Roessler 2011; Pilster and Böhmelt 2011, 2012). Opportunity-centered explanations are silent on what motivates military intervention and rather focus on which factors determine its feasibility. So far, however, the empirical record on the side of opportunity approaches is mixed at best. For example, while theories of “military centrality” (Jenkins and Kposowa 1990,

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political motives, was the decisive cause of the coup.

1992; Kposowa and Jenkins 1993; Maniruzzaman 1992; Wang 1998) assume that coups occur when the military's capabilities in terms of resources and organization are superior to those of civilian institutions, there is still no consensus as to which factors facilitate or impede coups: For example, conflicts within the military – e.g., ethnic divisions, social and intergenerational conflicts, or inter-branch rivalries – have alternatively been said to increase or reduce coup risk (Finer 1962; Janowitz 1964; Decalo 1973; Bienen 1974, 1985; Jenkins and Kposowa 1990, 1992; Kposowa and Jenkins 1993; Belkin and Schofer 2005). To give an even more striking example, military strength (in terms of both manpower and resource endowment) is typically assumed to facilitate military takeovers; yet, Powell (2012) finds that military size aggravates coordination issues and that increasing military expenditures actually makes coup attempts less likely to succeed.

Moreover, these approaches are not entirely convincing, to the extent that they suffer from the same kinds of problems as the corporate-interests approaches: opportunity is simply too ubiquitous to explain the considerable variation in patterns of military involvement in domestic politics. Weak states – in which the overwhelming majority of coups occur (see, e.g., Londregan and Poole 1990) – succumb quite easily to quickly executed coups, even when those are organized by a small faction of the military without the support of the military's high command. Tullock's (2005 [1974]) account of one of the numerous coups that occurred in Iraq provides a nice example:

The government of Iraq was once overthrown because a division of infantry, moving from one part of the country to the other, passed through the capital, and its general took advantage of the opportunity. The fact that he had relatively weak military support was not obvious until it was much too late (Tullock 2005 [1974], 283-4).

But even in established democracies, full civilian control of the military is not self-evident. The overwhelming organizational advantages of the military vis-à-vis any other civilian group, and the fact that, contrary to parties or churches, it possesses weapons, led Finer to state that, instead of asking why officers sometimes intervene, “we ought surely ask why they ever do otherwise” (Finer 2009 [1962]: 5). Similarly, Przeworski calls it a “miracle” that “people who have guns obey those without them” (Przeworski 1999, 16). In sum, what makes coups seem so random and unpredictable is that even small and underequipped

militaries are hardly constrained by structural factors.

#### 3.1.4.2. Some assumptions about the military's preferences

As discussed above, assigning preferences to the military as a whole organization is inherently difficult – if not bound to failure – and global explanations of coups generally do not perform very well (Croissant 2013b). Yet, prior research has pointed to several stylized facts about coups, which provide some clues about the military's willingness to interfere in domestic politics.

The first stylized fact is that, while the corporate-interests approach generally fails to explain why coups are attempted in the first place, it can explain why coup plotters receive support from the military as a whole. Since most officers value the unity and internal cohesion of the military more than anything else, they often go along with coup plotters regardless of whether they are themselves in favor of an intervention or not (Needler 1975; Geddes 1999). Thus, even if the motives of the first movers are idiosyncratic, the success or failure of interventions can be explained by a desire to preserve the organization's integrity. Powell (2012) finds that militaries that are given larger financial endowments are not only less likely to attempt coups, but also less likely to succeed: according to him, this is because these resource endowments give the military strong incentives to oppose any conspiracy that would emerge within its own ranks.

This softer version of the corporate-interests approach does not even require making strong assumptions about commonly shared values or beliefs, because if a coup is staged, officers are always worse off if they remain passive, regardless of the outcome of the intervention. If the coup fails, the government is likely to respond with coup-proofing measures that would weaken the military; if the coup succeeds, officers who remained loyal to the regime run the risk of being punished for their defection (Geddes 1999, 126-8; see also Tullock 1987). The worst possible outcomes are either a complete disintegration of the military hierarchy – with the consequence that military commanders lose not only control over their subordinates but also their pivotal position vis-à-vis the civilian government – or a civil war in which factions of the military fight against each other (Geddes 1999).

A second stylized fact is that the military rarely intervenes against “legitimate”

governments (see, e.g., Finer 2009 [1962]; Luttwak 1979; Needler 1975; Nordlinger 1977; Belkin and Schofer 2003). More specifically:

(1) Officers rarely attempt coups they believe will be met by mass protests. This is why military interventions are less frequent in countries where civilian organizations are highly developed and where there is a certain level of public attachment to civilian institutions (Finer 2009 [1962]; Putnam 1967; Luttwak 1979; Belkin and Schofer 2003). Finer's (2009 [1962], 93–98) discussion of the failed coups in Germany in 1920 and France in 1961 clearly shows that otherwise successful coups can fail because plotters either encounter mass popular opposition (Germany) or lack civilian support (France). This effect is not restricted to democracies: according to Geddes (2006), the ability to mobilize popular opposition to coups is one of the factors that contribute to the stability of party-based autocracies.

(2) In countries where political participation is broad and the public is organized in parties, unions, etc., coups are usually caused by performance failures on the part of the government. Such performance failures typically include economic crises with large-scale unemployment and massive inflation, chronic cabinet instability, or the government's failure to maintain public order (see Luttwak 1979; Nordlinger 1977; O'Kane 1981; Powell 2012); conversely, high growth rates have been found to inhibit coups (Londregan and Poole 1990). Interestingly, military governments' withdrawal from politics is often motivated by these very same reasons (Nordlinger 1977; Needler 1980).

A final stylized fact – which reflects a near consensus in the literature – is that coups rarely overthrow stable regimes (e.g., Decalo 1973; Bienen 1985), and, conversely, that coups tend to breed further coups (Luttwak 1979; O'Kane 1981, 1983; McGowan and Johnson 1984; Londregan and Poole 1990; Belkin and Schofer 2003). In spite of the regularity of this finding, however, the causal mechanisms underlying this empirical pattern are rarely discussed at length.<sup>56</sup>

Finer (2009 [1962], 118-9) provides such an explanation: after having successfully removed a government from office, military officers have to reckon with the possibility

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<sup>56</sup> There are some discussions about factionalism and the dynamics of coups and countercoups in military governments (see Finer 1962; Nordlinger 1977), but the “coup trap” (Londregan and Poole 1990) is a somewhat different phenomenon.

that this government or its supporters might become strong enough to return to power and retaliate. Thus, they can never fully disengage and have to continue to regularly intervene in politics “if only for self-defense,” Finer writes (p. 119).

The simplest explanation, however, is that on the whole, officers tend to behave in the same way as civil servants or higher-ranking officials. The analogy should not appear surprising given the numerous parallels that have been drawn between professional soldiers and civil servants (for example, O’Donnell’s [1973] “bureaucratic authoritarianism” or Nordlinger’s [1977] discussion of military officers’ governing style). But the point here does not concern the natural affinity between officers and bureaucrats, but rather their similar way of reacting to political instability. When political regimes are stable – that is, when the country has not experienced an irregular change of leadership in recent years and when it seems unlikely that the government will be forcefully removed from office in the near future – the dominant strategy for military officers is to stay out of politics. Even those who strongly dislike the current government are deterred from attempting a coup because they are unlikely to be supported by their colleagues. In such regimes with no recent history of military intervention, the higher levels of the military hierarchy are likely to be populated by individuals whose primary motivation is to keep their current jobs and move up the career ladder. By contrast, when interventions become routine and membership in the military becomes a privileged path to political power, the organization begins to attract individuals who are interested in seizing power themselves, or at least less willing to prevent others from trying to do that. Furthermore, leaders who face a high coup risk – including military leaders themselves – tend to make an extensive use of the well-documented “coup-proofing” strategies (Quinlivan 1999; Belkin and Schofer 2005; Pilster and Böhmelt 2011, 2012; Powell 2012; see also Tullock 2005 [1974], 1987), which include, among others, developing paramilitary organizations to counterbalance traditional military forces, moving senior officers from one position to another – or keeping the most influential ones away from the country, if not removing them from office altogether – or exacerbating internal rivalries by creating multiple branches with overlapping jurisdiction that monitor one another. More radically, coup prevention can consist in purges and widespread assassinations. Officers operating in such high-risk environments thus have shorter time horizons and face higher incentives to

participate in anti-government plots: political instability may motivate preemptive military coups for the same reasons that explain why members of the ruling coalition of a dictator may attempt to overthrow him in anticipation of forthcoming power struggles.

Finally, potential threats do not only come from insecure leaders, but also from potential conspirators from within the military's ranks: officers who failed to join a successful coup run the risk of being punished for their defection or simply losing their position because it is needed for rewarding coups participants. Because, in highly unstable regimes, neutrality rarely pays off and participation in coups entails fewer opportunity costs, the dominant strategy for officers is to anticipate the next change of leadership and try to jump on the coup bandwagon on time (see Geddes 1999; Tullock 2005 [1974]; Tullock 1987).

To summarize the discussion, the officer corps – like civilian elites – is also affected by the adverse selection and the uncertainty problems discussed in Section 3.1.2. It follows that, first, instability at the elite level can breed coups even when the military has no specific grievances or high-ranking officers are not particularly interested in participating in politics. Second, civilian control of the military is an equilibrium, but it is a fragile one: once it is disrupted, military intervention in politics becomes a vicious circle, and coups breed further coups.

### **3.2. POLITICAL INSTITUTIONS AS CONFLICT-RESOLUTION DEVICES**

The previous section dealt with potential sources of political conflict in political regimes, especially in weakly institutionalized ones. This section will discuss the function of political institutions as conflict-resolution devices.

Specifically, section 3.1. described three types of conflict: those taking place between the incumbent and the regime's higher officials; those taking place between the incumbent and the military; and those taking place between the ruling elite and the political opposition. The central argument made here that political institutions have the potential to pacify at least two<sup>57</sup> of these types of conflict, namely intra-elite conflicts and conflicts between

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<sup>57</sup> The question of the impact of political institutions on conflicts between the government and the military is somewhat more complicated. As will be discussed throughout the dissertation, I assume that political institutions have an indirect effect on the military's propensity to interfere in political affairs: first, I argue

elites and opposition groups.

But here is the crux of the problem: any claim that institutions can somehow solve conflicts over the distribution of political power must rely on the rather strong assumption that every actor abides by the rules and does not try to subvert them. This is not self-evident: as Hardin (2013) notes, contracts are abided by because they are enforced by a third party (typically the state); yet, there is no third party to enforce political institutions and guarantee that every participant plays by the rules. Thus, any causal argument about the effect of political institutions requires identifying the reasons why actors would choose to comply with these rules even if they have sufficient power to subvert them.

Part of the problem can be solved if formal rules are conceived as coordination devices, similar to conventions on road traffic (Hardin 1987; 1989; 1997): there is in principle no reason why we should drive on the left side or on the right side of the road, but drivers are better off if everyone follows the same convention. A rule stipulating that everyone should drive on the right is thus likely to be observed by everyone – even if it is arbitrary in its content and was established by unilateral imposition – because deviating from it would bring about heavy costs. This echoes the discussion on ruling coalitions in the previous section: a member of one of these coalitions might be completely indifferent about whether the current ruler stays in power or is replaced by someone else, but he surely cares about whether the others will coordinate on the same option as himself. Formal institutions can solve part of the problem by providing all members with a focal point (Schelling 1960) to coordinate their actions.

Yet, this is only one part of the problem. While interactions between members of the ruling coalition often (not always) take the form of a coordination game (i.e., a situation in which actors are better off if everyone chooses the same strategy) and thus can be pacified by formal rules (see Hardin 1989), this is not true of every type of interaction. Thus, the mere existence of a set of formal rules does not always guarantee compliance: for political institutions to “work,” they must be self-enforcing in the sense that once rules are established, nobody has an incentive to deviate from them, and even actors who would

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that institutions indirectly affect coup risk to the extent that they contribute to the stabilization of political regimes (that is, I expect every stable regime to be less coup-prone, regardless of which specific institutions contributed to this stability). Second, since coup risk partly depends on the risk of intrastate armed conflict (more on this in Chapter 4), every institution that decreases the risk of intrastate armed conflict indirectly decreases coup risk.

have the power to subvert extant institutions choose instead to abide by them. To put it in more concrete terms, we can easily think of institutions that are not self-enforcing because at least someone is better off unilaterally deviating from the rule: for example, the reason why primogeniture gradually replaced agnatic seniority in European monarchies might lie in the fact that in the latter system, the age difference between the current monarch and the next in line for the throne was not large enough to guarantee that the monarch would die before his heir (Kokkonen and Sundell 2014). To take another example, Tullock (1987) argues that juntas – i.e., systems in which political power lies with a small group of individuals who take decisions by unanimity – are inherently unstable, because one member will always try to accumulate power at the expense of his allies: thus, juntas tend to gradually shrink to one man, either because one member has succeeded in consolidating his power or because those who have failed to do so have been gradually eliminated by the other members. In these two examples, rules are established but they do not provide sufficient incentives for the most powerful actors to abide by them instead of trying to subvert them.

In the following subsections, I will discuss the properties of two political systems – democracies and monarchies – and identify the institutional mechanisms that make these two systems self-enforcing. As already noted, monarchy and democracy are not the only conceivable self-enforcing institutions.<sup>58</sup> Yet, they are today the most common forms of institutionalized political systems, and they both display some properties that make them less prone to violent political struggles. The central argument I will make is that, while democracies are better at solving conflicts between ruling elite and political opposition, monarchies are better at preventing intra-elite conflicts.

### **3.2.1. Self-enforcing institutions (1): Electoral competition**

Why and under which conditions democracy endures is one of the oldest and the most controversial questions in comparative politics: Weingast (1997, 245) rightly characterizes

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<sup>58</sup> In a short article, Tullock (2002) discusses several historical forms of nondemocratic governments that achieved stability and were relatively successful. Most of these systems relied on some variant of elections or hereditary succession, but were nonetheless very different from contemporary democracies or monarchies.

the literature on this topic as “immense, multifaceted, and compartmentalized.” There are, on the one hand, studies devoted to the conditions under which democracy is more likely to endure – the most prominent of which are economic prerequisites of democracy (e.g., Lipset 1959; Przeworski and Limongi 1997; Boix 2003; Acemoglu and Robinson 2006). On the other hand, and more closely related to the question of self-enforcing institutions, numerous studies have been devoted to the question of which specific institutional arrangements make democracies more likely to endure (for an overview, see Ginsburg 2011): recurrent topics include presidentialism (Linz 1990; Stepan and Skach 1993; Mainwaring and Shugart 1997; Cheibub 2002, 2007) and counter-majoritarian institutions (Mittal and Weingast 2010; Alberts, Warshaw and Weingast 2012). The topic of electoral engineering in plural societies has likewise received a great deal of attention (Horowitz 1993, 2004, 2006, 2007; Fraenkel and Grofman 2004, 2006a, 2006b, 2007; Reilly 2002; Mozaffar et al. 2003). What all these works have in common is that they treat specific institutional arrangements as instrumental to the stability of democracy.

In this respect, what constitutes Przeworski’s (1999, 2003, 2006) fundamental innovation is his claim that democracy, even reduced to its barest minimum – that is, periodic electoral competition between at least two political parties – constitutes by itself an effective way of preventing conflicts.<sup>59</sup> According to him, democracy is self-enforcing to the extent that it reduces the opposition’s incentives to rebel against the incumbent and increases the incumbent’s willingness to relinquish office peacefully after his term in office.

The argument has two parts. First, from the perspective of the opposition, Przeworski (1999) argues that the mere possibility of changing governments – regardless of the procedure – can avoid violence because the losers can expect to become winners in the next round. This can be achieved through any regular procedure (e.g., a lottery), but elections have two further advantages. On the one hand, the incentive to get reelected leads governments to pursue relatively moderate policies, which further decreases the opposition’s incentives to rebel. On the other hand, elections are a public display of force: they indicate who would rebel if the incumbent tried to usurp power and, conversely, who

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<sup>59</sup> This, of course, does not mean that democracy can endure under any condition: economic development is crucial to the survival of democracy (see especially Przeworski 2005).

would support the incumbent if the opposition tried to grab power by force. Because they publicly indicate everyone's preferences, elections thus prevent contenders from developing optimistically biased beliefs regarding the outcomes of a conflict (on this latter point, see also Londregan and Vindigni 2008).<sup>60</sup> Moreover, a system of leadership selection based on majority rule has the consequence that political power broadly coincides with numerical strength: in democracies, groups that are strong enough to successfully challenge the government are precisely those that have the strongest incentives to comply.

The second part of the argument concerns incumbents. Incumbents are more likely to yield office instead of trying to cling to power (and perhaps risking a costly rebellion) if 1) they are confident that the new winner will do the same after his term in office, and that they themselves retain some chance of returning to office later (Przeworski 1999); and 2) they believe that the new incumbent will not use his power to imprison them, force them into exile or otherwise prevent them from running for office again (Przeworski 2015). This is why many former dictators who consider stepping down may nevertheless find it difficult to obey the results of elections, but this is also why, once a peaceful alternation has taken place, all contenders quickly acquire the habit of abiding by the rules and electoral competition becomes an equilibrium from which nobody has an incentive to deviate (Przeworski 2015; for variants of this argument, see Moehler and Lindberg 2009 and Svulik 2013b).

To sum up, democracy performs two related functions. First, because it shortens the waiting time until the next change of leadership, it decreases the opposition's incentives to resort to violence. Second, because it lowers the stakes of politics, it solves the "retirement problem" discussed in Section 3.1.1. Both mechanisms are mutually enforcing because repeated, peaceful alternations in power show that the opposition is better off waiting than

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<sup>60</sup> An interesting variant of this argument can be found in a recent article by Fearon (2011): an elected ruler might decide to stay indefinitely in power and refuse to organize elections. Yet, while any other infringement of voters' rights might not prompt any violent reaction – because citizens typically have disparate preferences and do not necessarily agree on what constitutes a transgression, and because some groups might acquiesce to transgressions perpetrated against other groups (see Weingast 1997) – cancelling elections or refusing to step down after an electoral defeat constitutes a clear abuse of power and hurts everyone's preferences in the same way. Knowing that citizens could easily overcome their coordination problems in such situations, incumbents have an incentive to accept election outcomes. Thus, the very existence of elections automatically generates an incentive for the incumbent to abide by the rules.

rebellious, and that the incumbent is better off surrendering power than attempting to usurp it. While democracy is not established easily, it becomes an equilibrium on the long run.

### **3.2.2. Self-enforcing institutions (2): Hereditary succession**

In *Autocracy*, Tullock (1987, 166) predicted that on the long run, most (nondemocratic) regimes would converge toward some form of monarchy. Almost thirty years later, it seems that history has given him right to some extent. First, contemporary monarchies are notoriously stable: they display the longest lifespan among post-1945 autocracies (Hadenius and Teorell 2007a, 150), and Middle East monarchies have proven much more resilient to the Arab Spring than neighboring republics (Bank et al. 2013). Second, hereditary succession – the defining feature of monarchy – is now becoming a widespread practice among authoritarian regimes, including those that are not formally monarchies (Brownlee 2007). Examples include cases as diverse as North Korea, Togo, Azerbaijan, the Dominican Republic, Singapore, Syria, or Haiti. All in all, this suggests that monarchies are not an endangered species, and that their share in the universe of authoritarian regimes is likely to increase in the near future.

According to Tullock (1987), the fundamental reason why hereditary succession is conducive to regime stability in the long run is that it provides both the dictator and its ruling coalition with a satisfactory solution to succession problems. From the perspective of the dictator, the most obvious problem is that appointing a successor increases his risk of being assassinated: “Obviously the sooner the successor becomes dictator in his own right the better from his standpoint and shortening the life of the current dictator is an obvious way of speeding up the succession” (Tullock 1987, 151). Hereditary succession (and especially primogeniture) mitigates this risk to the extent that, first, the heir is more likely than a member of the ruling coalition to simply wait for his father to die instead of overthrowing him, because the age discrepancy still gives him a chance of a long reign; second, the existence of a firm succession rule reduces the heir’s uncertainty about the current dictator eventually changing his mind, and thereby his incentive to make the first move and overthrowing him while he has the opportunity to do so; and third, the personal

relationships between father and son are likely to be better than between the dictator and his officials. So far, the empirical evidence clearly speaks in favor of this theory: Kurrild-Klitgaard (2000) compares succession rules in Denmark from 935 to 1849 and shows that monarchs became much less likely to be deposed by their successors as primogeniture became the established way of selecting kings. After comparing the succession rules in several European monarchies during the period from 1000 to 1800, Kokkonen and Sundell (2014) similarly show that monarchs were less likely to be overthrown in states practicing primogeniture than in states practicing other succession orders.

This, in turn, also explains why hereditary succession prevents regimes from collapsing as a consequence of succession crises. Members of the ruling coalition might have a preference for becoming dictators themselves, but what they want to avoid in the first place is losing in a succession conflict, or backing a candidate who turns out not to be the preferred option of the other members. Thus, provided with a credible alternative to open competition, they have an incentive to back the dictator's chosen heir simply because they expect other members to do the same. Primogeniture is, again, the best option since members of the ruling coalitions are most likely to view this choice as one the dictator will not renege on, and one other officials will very probably back. By providing members of the ruling coalition with a focal point on which they can coordinate their actions, hereditary succession reduces uncertainty and thereby prevents succession issues to degenerate in open conflict and eventually regime collapse (Tullock 1987, 157).

These arguments, however, beg further questions: while they shed light on why hereditary succession is conducive to regime stability in the long run, they do not explain how and why it gets established in the first place. The first obvious question is why a dictator would want to appoint a successor at all, since it is likely to adversely impact his survival in office (if not his life expectancy). Tullock's (1987) initial argument is that dictators are practically always better off not choosing any successor, unless they are interested in their legacy (p. 151) – which is not necessarily the case. In short: since succession struggles and regime breakdown upon the autocrat's death only affect members of the ruling elite and are not a concern for the autocrat himself, why should he be interested in designating a successor?

More recent studies on hereditary succession have provided a partial answer to this

question, by showing that competition between members of the ruling elite are likely to affect the current autocrat's survival in office: Kokkonen and Sundell (2014) as well as Brownlee (2007) argue that, assuming that members of the ruling coalition are primarily interested in maintaining their current status, they are most likely to invest in the current regime if they expect it to survive after the death of the dictator; by contrast, they are more likely to defect if they anticipate a succession crisis. Thus, the autocrat's ability to reward his supporters after his death helps him ensure his stability in office. Most importantly, in the absence of a designated successor, any member of the ruling coalition might anticipate the forthcoming power struggle and be tempted to make the first move (i.e., overthrow the current autocrat) to avoid being eliminated by the winning contender upon the ruler's death (see Section 3.1.2). A variant of this argument can be found in Kurrild-Klitgaard's (2000, 67) study: in open succession systems (i.e., when there is no formal succession rule, and members of the ruling coalition compete for appointments), the most serious contender faces a high risk of being eliminated by remoter contenders; anticipating this, he thus has an incentive to eliminate the current autocrat in order to secure his own position.<sup>61</sup>

The problem with these arguments is that they all rely on the assumption that members of the ruling coalition are risk-averse, that is, that they prefer formal succession rules (which imply that each member is certain he will not be chosen as a successor) over open competition (which implies that each member has a fair chance to become the next autocrat but also risks being eliminated by his competitors). Tullock (1987), Brownlee (2007), and Kokkonen and Sundell (2014) all explicitly assume that elites prefer maintaining their current status over engaging in a risky power struggle and hence are ready to accept any stable succession order, even if this means giving up the possibility to assume office themselves in the future. This assumption is probably correct when the rule

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<sup>61</sup> There are some other reasons why a dictator would want to designate an official successor, but these explanations rather suggest that the dictator would be better off selecting a member of the ruling elite rather than one of his relatives. One of these reasons is that, by officially designating a successor, the dictator is able to divert the attention of potential coup plotters from him to the successor (if the dictator is relatively old and the successor relatively young, conspirators have an incentive to eliminate the successor rather than the incumbent). Another reason is that it gives the successor a vested interest in the political survival of the autocrat and a strong incentive to betray any plot against him. A further potential advantage is that hurdles accumulate on potential challengers' path to power if they have to neutralize both the dictator and his successor, which can be enough to deter at least some of them from a coup attempt. Finally, knowing who is the most likely to try to overthrow him provides the dictator with the ability to monitor his most dangerous potential contender.

has already been in effect for a long time, i.e. in established monarchies: in these cases, elites enter their positions knowing that they have virtually no chance of becoming rulers, and the stability of the regime makes it less likely that the ruling coalition is made up of risk-seeking individuals who are ready to take any opportunity to attain power. Yet, the risk-aversion assumption is by far more questionable when the ruling coalition is made up of individuals who (1) assumed office at a time when the question of succession was still open and entered the ruling coalition in the hope that they could themselves attain power in the future; and/or (2) participated in the coup that overthrew the previous autocrat, which clearly demonstrates that they were willing to risk their current positions in order to have a better chance of becoming autocrats themselves.

It is thus debatable whether establishing hereditary succession helps dictators surrounded by ambitious and risk-seeking elites avoid being removed from office: by officially informing his supporters that none of them has a chance of taking office after his death, he provides them not only with a strong motive to overthrow him but also with the opportunity to do so, since the announcement gives them a solution to overcome their coordination problems (i.e., each member knows that other members will probably not back the dictator in case a coup is attempted, so everybody has an incentive to defect first). Brownlee's (2007) study on hereditary succession in contemporary autocracies provides interesting clues, since the analysis deals only with regimes that are not formally monarchies. He identifies twenty-two cases of dictators who attempted to install their heir in power, and finds that the ruling coalition's decision to back the dictator's choice or to oppose it depended on whether it enjoyed a precedent for selecting the successor from within its ranks: "established" parties that were involved in the selection of previous rulers were by far more likely to veto the dictator's decision than loose elite coalitions. Interestingly, Brownlee interprets this finding as evidence that elites prefer hereditary succession over open competition:

When there is no precedent for choosing a leader from within the ruling organization, elites may support hereditary succession as a method for preserving their influence and mitigating the uncertainty that surrounds an aging ruler and an impending leadership change (Brownlee 2007, 626).

There is yet another possible interpretation: the fact that dictators are more likely to

succeed in installing their heir in power in the absence of such precedents does not necessarily mean that elites are willing to back the dictator's choice in order to avoid power struggles, but could also be taken as evidence that they were simply too weak to impose their preferences. Furthermore, Brownlee's (2007) analysis reveals that many dictators try to establish hereditary succession even when another succession procedure exists: the conclusion that follows is that dictators, for some reason,<sup>62</sup> genuinely prefer hereditary succession (i.e., that their aim in imposing hereditary succession is not merely to avoid succession struggles) while the ruling coalition does not; and dictators who succeed in imposing their heirs are the ones who have managed to overcome challenges from their ruling coalition in the first place. In other words, hereditary succession might not be the cause but rather simply a symptom of dictators' security in office.

To conclude the discussion, the reasons why some autocracies establish hereditary succession while some others do not remain so far mysterious: while many authors argue that hereditary succession results from a consensus between the incumbent and his allies, available evidence rather suggests that it is often unilaterally imposed by powerful rulers (that is, paradoxically: by rulers that need it the least), for reasons that can be but are not necessarily related to concerns about the stability of the regime. Yet, regardless of the circumstances under which hereditary succession is established, the fact is that the system tends to be conducive to stability: first, as discussed above, rulers are *ceteris paribus* less likely to be overthrown under hereditary succession than under open succession rules (Kurrild-Klitgaard 2000). Second, although coups still happen fairly often in monarchies – albeit less often than in non-institutionalized autocracies – they usually do not end the regime: Cheibub et al. (2010) report that most monarchs who are overthrown are replaced by a family member. A study shows that the homicide rate among European monarchs in the period from AD 600 to 1800 was about 15% (Eisner 2011), and yet, monarchy remained the dominant form of political regime throughout this whole period. Finally, there is some evidence that regicides are less likely to result in severe regime crises than assassinations of political leaders in systems where there is no institutionalized succession

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<sup>62</sup> One reason could be that hereditary succession solves the “retirement problem” discussed in section 2.2.1: if a dictator wants to leave office, he does not have to be concerned about his successor's intention to eliminate him if said successor is a relative.

procedure (Iqbal and Zorn 2008).<sup>63</sup> All in all, this suggests that, like competitive elections, hereditary succession is not established easily but becomes stabilizing on the long run. More specifically, it contributes to the pacification of political conflicts by mitigating the two crucial problems identified in Section 3.1., namely the problem of uncertainty and the problem of adverse selection: the problem of uncertainty is solved by the very existence of a procedure for replacing leaders, which implies that regime elites are less concerned about the risk of losing their current position as the result of a change of leadership, and therefore less likely to attempt a preemptive coup. The problem of adverse selection is mitigated because elites know that their chance of attaining power under the current state of affairs is very low: thus, key regime positions are less likely to be filled by individuals who are solely motivated by the prospect of seizing power for themselves. In turn, a leader who attained power peacefully is less likely to be surrounded by ambitious and risk-seeking individuals. Finally, because of the combination of these two mechanisms, the risk of a subversive coalition forming against the current ruler decreases (which, in turn, reinforces both effects).

### 3.3. SOME EXPECTATIONS

Chapters 2 and 3 dealt with two broad issues: first, the definition of democratic and non-democratic regimes; and second, the extent to which the internal organization of each political regime type can be expected to affect the nature and intensity of political conflicts. Two broad categories of political regimes were distinguished: on the one hand, institutionalized regimes (monarchies and democracies), which rely on binding rules to regulate access to, and succession in power; and on the other hand, non-institutionalized regimes, in which access to political positions is unregulated and often involves substantial violence. Following these theoretical considerations, I put forward to general propositions:

*Proposition 1:* In general, we should expect non-institutionalized regimes to be

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<sup>63</sup> Unfortunately, Iqbal and Zorn (2008) use a measure of regulated succession that aggregates elections and hereditary succession. Since instances of hereditary succession are relatively rare in the period of investigation of their analysis, doubts remain as to whether the result is not entirely driven by democracies.

more vulnerable to intra-elite conflicts than institutionalized regimes.

*Proposition 2:* In general, we should expect democracies to experience fewer conflicts between elites and opposition groups than non-democracies (including monarchies).

These two general propositions are associated with several expectations, which will be examined throughout the dissertation. First, I expect instability at the elite level to impact the military's propensity to intervene in domestic politics. Second, the exposure to specific risks should have broader repercussions on the behavior of the ruling elites, and on their readiness to use violence: state repression, for example, is one response non-democratic states commonly use in order to protect themselves against threats posed by organized opposition groups. Finally, since non-institutionalized regimes are often the outcome of a violent power struggle that resulted in the forceful removal of the previous ruling coalition, we can expect the nature and intensity of political conflicts taking place in each country to affect the specific form non-institutionalized regimes will take: this will be the topic of the following chapter.

## 4

### **Not all regimes are born equal: How internal and external security threats shape domestic political structures**

A quick glance at the distribution of political regimes worldwide reveals a puzzling empirical regularity: island states – and particularly microstates such as the Solomon Islands or the Federated States of Micronesia – are overwhelmingly democratic (Anckar 2006, 2008; Srebrnik 2004). What can explain this striking pattern? So far, much of the relatively sparse literature on islands has focused on factors such as Protestantism, colonial heritage, ethnic or cultural homogeneity, or the remoteness and intimacy of islands which promote an increased sense of community, mutual trust and solidarity among citizens (e.g., Hadenius 1992; Anckar 1999, 2008; Srebrnik 2004; Ott 2000; Congdon Fors 2014; relatedly, Anckar 2004). But maybe there is a more straightforward explanation: what most of these states have in common, beside their remoteness and their small population, is the absence of a standing army.

Table 4.1. provides an overview of all island countries with fewer than one million inhabitants that were classified as democracies by Boix, Miller and Rosato (2013) for the year 2007. The right column indicates whether these countries had a regular military in the same year: The information is drawn from the Correlates of War National Material Capabilities dataset.<sup>64</sup>

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<sup>64</sup> I counted no regular military when both “mlex” and “milper” had a value of 0.

**Table 4.1. Armed forces in democratic island states (2007)**

<i>Country</i>	<i>Regular military</i>	<i>Country</i>	<i>Regular military</i>
Antigua and Barbuda	Yes	Micronesia	No
Bahamas	Yes	Monaco	No
Barbados	Yes	Nauru	No
Cape Verde	Yes	Palau	No
Comoros	Missing	São Tomé and Príncipe	Missing
Dominica	No	Solomon Islands	No
Grenada	No	St Kitts and Nevis	No
Iceland	No	St Lucia	No
Kiribati	No	St Vincent and the Grenadines	No
Malta	Yes	Tuvalu	No
Marshall Islands	No	Vanuatu	No

(Sources: Boix, Miller and Rosato 2013; Correlates of War project)

A possible reason why the overwhelming majority of these states has no regular army is the relative absence of foreign threats: because they have natural borders, these countries are less likely to be involved in a territorial dispute with one of their neighbors; and because territorial conflicts between neighboring states constitute the most frequent instance of international conflict (see Bremer 1992; Vasquez 1995), islands are much less exposed to the risk of foreign invasion than mainland countries. Of course, natural borders do not entirely preclude the risk of invasion by a major power: as a matter of fact, many of the countries listed in Table 4.1. used to be colonies and gained their independence only recently. But, since their natural insulation protects them against territorial claims by a neighbor, invasion by a major power is virtually the only scenario in which island countries could get involved in an interstate conflict; and in those cases, the power imbalance would make armed resistance futile. In short, for island countries, the risks associated with a standing army simply outweigh its potential benefits.

But what exactly are these risks? Recent research provides a clear answer: military coups are one of the most widespread symptoms of political instability. Since 1946, coups have removed more leaders from office than assassinations, rebellions, popular uprisings, and

foreign interventions combined (Goemans et al. 2009a; Svobik 2012). Likewise, coups have overthrown a large number of democratic governments and are responsible for the majority of authoritarian reversals (Maeda 2010, 1135); furthermore, states that inherit a powerful, interventionist military are believed to be less likely to democratize (Gibler 2007; Acemoglu, Ticchi and Vindigni 2008) and have been shown to be more likely to revert to authoritarianism after a democratic transition (Cheibub 2007).

Island states are arguably idiosyncratic cases and there are several competing hypotheses – besides the absence of a regular military – that could explain why they have managed to become and remain democratic. But consider, now, Costa Rica. In 1948, in the aftermath of a civil war won by opposition forces, the new President José Figueres Ferrer decided to disband the army. What motivated this decision is debatable (Høivik and Aas 1981), but its consequences less so: 1948 clearly marked the end of a long history of armed political struggles and irregular transfers of power (see Lehoucq 1996, 330). Since then, no Costa Rican president was ever overthrown by a military coup. In contrast to its Latin American neighbors, which experienced repeated military intervention in domestic politics, the country remained a stable democracy for the next six decades, although regularly surrounded by dictatorships.

The aim of the chapter is to analyze the impact of militarization on the emergence and survival of political regimes. More specifically, it investigates how the nature and intensity of security threats impact the risk of a military coup, and how repeated military interventions shape domestic political structures. It builds on several strands of literature that analyze the impact of intrastate (Finer 2009 [1962]; Nordlinger 1977; Acemoglu et al. 2008; Svobik 2012) and interstate (Dassel 1998; Desch 2001; Arbatli and Arbatli 2014; MacMahon and Slantchev 2015; Piplani and Talmadge 2015) conflicts on the military's propensity to intervene in domestic politics, and how these threats eventually affect the development and stabilization of political regimes (Midlarsky 1995; Thompson 1996; Rasler and Thompson 2004; Gibler 2007, 2010). The argument developed here can be summarized as follows: (1) When confronted with either external or internal security challenges (understood here as the risk of interstate or intrastate armed conflict, respectively), states react by building a strong military and investing resources into it.

(2) Militarization poses a well-known dilemma to states: A military that is powerful enough to prevail in a war is also powerful enough to impose its will on civilian governments. Investing in the armed forces means endowing them with sufficient resources to overthrow the system they are intended to protect (Feaver 1999). One consequence is that states with a weak military – or no military at all – are *ceteris paribus* less likely to experience coups.

(3) Provided that the military is powerful enough to seize power for itself, whether or not it will take this opportunity depends, in turn, on the nature and the intensity of the threat. Specifically, while internal threats monotonically increase the risk of a military intervention, the relationship between external threats and military coups takes the form of an inverted U: maximal external security threats tend to induce military loyalty and immunize regimes against coup risk.

This simple framework helps predict not only the occurrence of military coups, but also the type of political regimes that are most likely to emerge on the long run. Given that coups are the most frequent cause of democratic breakdowns (Maeda 2010) and that the military can have incentives to prevent democratization (Acemoglu et al. 2008), democracies are more likely to emerge and survive in the absence of serious security threats. By contrast, situations in which the military enjoys a pivotal position within the political system and has incentives to intervene – that is, moderately high external security threats and very high internal security threats – should be conducive to military dictatorship. Finally, highly centralized autocracies (that is, single-party regimes) should be concentrated in countries that experience chronic external security threats but low internal security threats.

The remainder of the chapter is organized as follows: next section reviews and discusses past scholarship on the impact of security threats on the military's propensity to intervene in domestic politics. Section 4.2 presents the theory and some empirically testable implications. Section 4.3. is devoted to the empirical analysis: proxying the risk of interstate war by the number of contiguous land borders, and the risk of civil war by the proportion of males aged 15 to 24 in the adult population, I find empirical support for my arguments. Section 4.4. concludes.

#### **4.1. BACKGROUND: HOW DOES THE RISK OF ARMED CONFLICT IMPACT POLITICAL STABILITY?**

International relations scholars have long argued that high threat environments were an impediment to democratization. This idea can be at least traced back to Otto Hintze (1970 [1906]), who famously contrasted continental, militarized states (e.g., Prussia in the 19<sup>th</sup> century) with insular states, which did not have to rely on a large standing army to protect their borders and could afford more decentralized political structures (e.g., England in the same period). Harold Lasswell's (1941) "garrison state" theory is also based on the premise that a challenging international environment should lead to the development of an authoritarian state dominated by the military.

The idea that domestic politics are partly shaped by the international environment has recently given rise to one of the most serious challenges to the democratic peace theory. As several authors have argued, the empirical relationship between democracy and peace is potentially due to the fact that states that find themselves in peaceful zones are both more likely to become (and remain) democratic and less likely to be at war with their neighbors (Midlarsky 1995; Thompson 1996; Rasler and Thompson 2004; Gibler 2007, 2010, 2014; Gibler and Tir 2010, 2014; Gibler and Miller 2013; Gibler and Braithwaite 2013; Miller and Gibler 2011). The underlying argument is that democracies only emerge in peaceful environments and that high external threats lead to dictatorship, either by delaying democratization processes or by facilitating authoritarian reversals.

Generally speaking, studies on international threat and political regime identify two paths toward autocracy. The first one is *state centralization*, i.e., a gradual concentration of power and resources in the hands of the chief executive or a small ruling elite (Thompson 1996; Rasler and Thompson 2004; Gibler 2010). This power and resources concentration arises either because of the direct effects of war involvement on social and economic structures (Thompson 1996; Rasler and Thompson 2004) or because rally effects provide the chief executive with an opportunity to neutralize opposition parties and eliminate institutional veto points in the political system (Gibler 2010). The second mechanism is *militarization*, i.e. the development of a powerful standing army that can be used by elites

to repress domestic opponents and prevent democratization (Gibler 2007; Gibler and Tir 2010; Miller and Gibler 2011). Although both mechanisms are arguably distinct in their logic and consequences, the majority of these studies have tended to treat them as similar (see, e.g., Gibler and Tir 2010).

Both explanations have received some support in the empirical literature. With regard to the state centralization hypothesis, there is growing evidence from the quantitative literature that high external threats tend to jeopardize political pluralism: territorial threats – the most salient ones – have been found to reduce political tolerance among the public (Hutchinson and Gibler 2007) and decrease the occurrence of anti-government protest (Hutchinson 2011). At the same time, studies on state repression regularly come to the finding that interstate conflicts have a detrimental effect on the protection of physical integrity rights and civil liberties (e.g., Poe and Tate 1994; Poe et al. 1999; Davenport and Armstrong 2004; Davenport 2007b). Wright (2014) shows that in spite of institutional constraints, even democracies tend to increase repression when involved in territorial conflicts, and argues that this is because voters – who would otherwise punish the leader for these practices – become more tolerant of such behaviors in wartime. Gibler (2010) demonstrates that external threats to the state's territory move opposition parties to support the chief executive – which results in a decrease of party polarization and a gradual removal of institutional checks on the executive – and provides compelling evidence that elites in nondemocratic systems similarly tend to circle the wagons when faced to a direct challenge to the territorial integrity of the state.

The militarization hypothesis rests on the argument that external threats give the military a pivotal position in the political system (which, in turn, reduces the prospects of democratization). This proposition has some advocates: for example, Dassel and Reinhardt (1999, 63) state that high external threats naturally lead civilian leaders to cede greater autonomy and political authority to the military, and go as far as to posit that this can motivate the military to unilaterally initiate interstate disputes in order to protect its interests when these are threatened (see also Dassel 1998). Interestingly, Acemoglu et al. (2008) argue that potential foreign threats should help countries democratize through this very mechanism: absent such threats, democratic leaders – because they do not depend on the military to stay in power – cannot credibly commit not to disband the armed forces,

which creates an incentive for the military to oppose regime transition (see also Arbatli and Arbatli 2014).

However, the very idea that foreign threats reinforce the military's position in the political system has become increasingly challenged in recent years. Desch (2001) was one of the first to argue that high external threats actually help civilian leaders control the military: challenging international environments, he argues, focus the military's attention outwards and reduce its dispositions to participate in domestic politics, enhance the cohesiveness of civilian institutions (thereby reinforcing their control on the armed forces), and lead to a convergence of the preferences of civilian elites and military officers.<sup>65</sup> More recent studies have examined the impact of external threats (including actual war involvement) on military coups, and found a negative effect (Arbatli and Arbatli 2014; Piplani and Talmadge 2015). Possible reasons for this negative relationship include rally effects that may deter the military from trying to overthrow the incumbent (Arbatli and Arbatli 2014), credible commitment effects (Acemoglu et al. 2008, Arbatli and Arbatli 2014), coordination and organization issues (e.g., the lack of physical access to the regime's key locations, or unpredictable relocations of military units) that limit the military's ability to plan and execute coups (Piplani and Talmadge 2015), or the fact that, in wartime, the military's primary concern is to avoid defeat (McMahon and Slantchev 2015). All in all, recent evidence suggests that the relationship between external threat and military intervention in politics may not be as straightforward as previously assumed.

Perhaps surprisingly, the effect of internal threats on military coups has received a more limited amount of attention in the recent literature. Although early researchers have consistently noted that coups tend to occur in periods of intense domestic turmoil, many recent studies on coup risk (e.g., Londregan and Poole 1990; Belkin and Schofer 2003; Powell 2012) either do not control for civil war or do not elaborate on its effects. The most recent advances in the literature on internal threats and military coups pertain to the moral hazard problems resulting from the use of the military as a repressive agent. For example,

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<sup>65</sup> Interestingly, this latter point echoes Dassel's (1998) argument on the impact of external threats on civil-military relations: the preferences of civilians and military officers may converge (thereby reducing the potential for conflicts) either because the military has an incentive to support the actual leadership or because war involvement makes civilian leaders completely dependent on the military for its own survival. As Desch (2001) notes, the absence of conflict is not necessarily indicative of good civil-military relations.

Acemoglu et al. (2008) argue that dictators who overly rely on their armed forces for internal repression run a high risk of being overthrown by the very same agents in charge of protecting them (see also Feaver 1999; Besley and Robinson 2009). Democratization does not solve the problem, since it only aggravates commitment issues: the expectation that it will lose its pivotal position within the political system increases the military's incentives to prevent democratization. Svobik's (2012) analysis is based on roughly similar premises, but he argues that the relationship between the magnitude of internal threats and the risk of military intervention is curvilinear, coups being the most likely at intermediate levels of threats: when threats arising from distributional conflicts are low, the military lacks resources to intervene in politics, but when these threats reach their maximal intensity, the government tends to systematically accommodate the military's policy preferences, which reduces the risk of intra-elites conflict and thereby the likelihood of overt military intervention. Although this latter argument is theoretically compelling, it is nevertheless at odds with the fact that early analyses of military coups and governments (e.g., Nordlinger 1977) as well as more recent studies (Desch 2001; Piplani and Talmadge 2015) find coups to be more likely in periods of violent domestic strife. It also fails to account for the fact that coups and civil wars tend to occur in similar contexts (Collier and Hoeffler 2005). Clearly, the question of how domestic strife and military coups relate to each other is still not settled.

#### **4.2. SECURITY THREATS, COUPS, AND POLITICAL REGIMES: THEORY AND HYPOTHESES**

The starting point of the argument developed here is what McMahon and Slantchev (2015) call the "guardianship dilemma." When confronted with either external or internal security challenges, most states react by spending more resources on the armed forces. This investment in security gives rise to the old problem of civilian control on the military, as summarized by Feaver (1999):

The very institution created to protect the polity is given sufficient power to become a threat to the polity. (...) On the one hand, the military must be strong enough to prevail in war. (...) On the other hand, just as the military must protect the polity from enemies, so must it conduct its own affairs so

as not to destroy or prey on the society it is intended to protect. Because the military must face enemies, it must have coercive power, the ability to force its will on others. But coercive power often gives it the capability to enforce its will on the community that created it.

From the perspective of the incumbent, this problem takes the form of a trade-off between the risk of losing a war and the risk of being overthrown by the military (see Roessler 2011). Confronted with this dilemma, rulers can solve it by purposefully weakening their military or disbanding it altogether, at the costs of leaving themselves exposed to threats. If these threats are serious enough, rulers most likely will not choose this option and will have to deal with a permanent and more or less acute coup risk.

Coup risk, in turn, varies according to the nature and magnitude of security threats. With regard to the magnitude of the threat, the central assumption I make is that the strength of the military increases monotonically as threats become more acute; and, as the power of the military increases, so does its ability to turn against the incumbent and seize power. With regard to the nature of the threat, I argue that, while internal threats monotonically increase the risk of a military intervention, maximal external security threats tend to induce military loyalty and immunize regimes against coup risk. The reason why we should expect internal and external threats to have so drastically different effects on coup risk can be summarized as follows: whereas foreign foes threaten everyone – including the military – domestic opponents' primary target is the ruling elite, which does not necessarily include the armed forces. Thus, high external threats bolster intra-elite cohesion and lead everyone to circle the wagons; by contrast, serious domestic challenges to the incumbent's authority force the military to take sides, and choose which orders to obey. In short, while the threat posed by foreign foes affects both the incumbent and the military, domestic turmoil threatens the military and the incumbent in an asymmetric way.

There are several aspects to this argument. First, while a military defeat resulting in a foreign occupation is clearly an undesirable outcome for the armed forces – to the extent that such an outcome would adversely affect their own organizational survival – regime transitions are not necessarily detrimental to the military's parochial interests (see Geddes 1999). If domestic threats become acute enough, the military still has the option of bandwagoning with the regime's opponents, and trade support for better wages, increased autonomy or other perks. As a matter of fact, provided that the military has become

dissatisfied enough with the current leader, defections in revolutionary situations happen fairly often (for discussions of individual cases, see Lee 2005, 2009; D'Anieri 2006; Hall 2010; Barany 2011, 2013). Furthermore, to the extent that domestic conflicts often concern the regime or the incumbent, the armed forces can view a change of leadership as an effective way of solving the problem: they can either choose to remove the incumbent from office and thus eliminate the source of the conflict altogether; alternatively, they can turn against the incumbent because they hold him responsible for the turmoil and believe they would be able to better handle the situation (Nordlinger 1977). By contrast, a leadership change is unlikely to affect the intensity of external threats to the same extent: on the contrary, officers that consider staging a coup must both successfully execute it and deal with the same threats that faced the previous incumbent (McMahon and Slantchev 2015). If anything, a forceful change of government during a period of crisis is likely to spark domestic turmoil and thus undermine the country's prospects of prevailing in the war (Arbatli and Arbatli 2014). In short, high external threats leave the military few choices but to support the incumbent in order to secure its own survival.

Second, the military rarely intervenes against governments that enjoy widespread support, and it rarely attempts coups that are likely to result in mass protests (Finer 2009 [1962]; Nordlinger 1977; Geddes 2006). Acute external threats tend to bolster public support for the incumbent through rally-around-the-flag effects, which, in turn, deter the military from staging a coup (Arbatli and Arbatli 2014). By contrast, high domestic threats indicate, if anything, that the incumbent is losing its legitimacy, which provides the military with better opportunities to attempt a coup that it believes will not be met by mass opposition (Nordlinger 1977). Relatedly, from the perspective of the military, firing at domestic opponents is not the same as firing at foreign foes: as researchers have regularly noted, attempts to use the military for internal repression can easily backfire on governments, and are a relatively widespread triggering factor for their overthrow (Finer 2009 [1962]; Needler 1975; Nordlinger 1977).

Finally, actual involvement in an interstate conflict is now widely believed to immunize regimes against coup risk, to the extent that it hinders both coordination between potential participants and physical access to key regime locations (see Tullock 1987; Belkin and Schofer 2005; Powell 2014b; Arbatli and Arbatli 2014; Piplani and Talmadge 2015). By

contrast, civil wars do not involve the deployment of military units out of the country, and thus often do not prevent regular military units from having access to the capital (Piplani and Talmadge 2015). Therefore, since domestic conflicts do not pose the same logistical obstacles as conflicts abroad do, they do not reduce the feasibility of intervention to the same extent.

I thus test the two following hypotheses:

*H1: there is a monotonic, positive relationship between the level of internal threats and the frequency of military coups.*

*H2: there is an inverted-U relationship between the level of external threats and the frequency of military coups.*

This framework helps predict not only the occurrence of military coups, but also the type of political regimes that are the most likely to emerge on the long run. First, the existence of a strong and possibly interventionist military is arguably an obstacle to democratization: a powerful military can have an incentive to prevent transitions to democracy because the new democratic government cannot commit not to reform or disband it altogether (Acemoglu et al. 2008). Second, a legacy of military intervention in domestic politics is one of the key factors that hinder democratic consolidation and leads to authoritarian reversals (Cheibub 2007; Svobik 2008). We should thus observe that countries that experience low levels of internal and external threats are on the whole more likely to become and remain democratic.<sup>66</sup>

*H3: democracies emerge by low levels of (a) internal and (b) external threats.*

So far, I have adopted Gibler's (2007, 2010; see also Gibler and Tir 2010, 2014) main argument, that democratization is more likely in weakly militarized societies. Yet, what these studies have overlooked is that strong militaries are not necessarily interventionist. In

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<sup>66</sup> I inspect the likelihood that a certain country is democratic or not at a certain time. This, in turn, can either be explained by the fact that countries that inherit a powerful military are less likely to democratize at all, or that coup-prone democracies do not remain democracies for long. My argument is agnostic as to which mechanism is at work.

turn, the absence of military intervention is not necessarily synonymous with democracy, or, to put it differently, the military's influence on domestic politics and propensity to intervene are not the same in all nondemocratic regimes: some of the most stable – and most repressive – authoritarian regimes display actually a fairly high level of civilian control of the military and have proven relatively immune to coups.

Which are these regimes? Prior studies on civil-military relations point to a clear tendency: on the whole, single-party autocracies (particularly Communist regimes) seem to have been the most successful in effectively controlling the military and avoiding coups, while at the same time maintaining large and highly professionalized standing armies (Perlmutter and LeoGrande 1982; Nordlinger 1977, 15-8). The Soviet Union is the most typical example, but this holds true for other single-party states such as China, Cuba, or Tanzania under Nyerere as well. Given that military coups are the most frequent form of irregular leadership change in authoritarian regimes (Svolik 2012), this relative consensus is consistent with the fact that party regimes are among the most stable dictatorships (Geddes 1999; Magaloni 2008). The source of this relative stability can be located in the internal structure of party regimes: because of the overlap of party and military positions, the absence of clear boundaries between both institutions, and the fusion of political and military leadership, overt conflicts between civilian leaders and military commanders are less frequent and less likely to result in the overthrow of the regime (Perlmutter and LeoGrande 1982). Furthermore, party-based regimes are generally able to implement a series of control mechanisms that make military takeovers less likely: such control mechanisms include dual command systems, the cooptation of military commanders into high party positions, and a system of appointments based on political loyalty, but also the extensive use of surveillance and targeted punishments (Nordlinger 1977; Perlmutter and LeoGrande 1982). As a result, frictions between civilian leaders and military commanders happen quite frequently in party regimes, but these are rather factional conflicts – in which the military intervenes to support one faction of the ruling party – rather than direct challenges to the party's rule (Perlmutter and LeoGrande 1982).

Thus, two “models” of nondemocratic systems can be distinguished: the first one is characterized by the pivotal position of the military within the political system and by recurring conflicts in which the military usually prevails, while the second one is

characterized by a high degree of civilian control of the military. This distinction broadly corresponds to the two pathways toward autocracy discussed by prior studies (see Thompson 1996; Rasler and Thompson 2004; Gibler 2010; Gibler and Tir 2010, 2014): party regimes are the result of the state centralization process, while militarization results in relatively unstable regimes in which the military frequently intervenes. One crucial implication is that these two processes are fundamentally incompatible and result in very different political structures, since power concentration should lead to (or be facilitated by) a low level of military involvement in domestic politics, while militarization should, on the contrary, lead to recurring frictions between civilian authorities and the armed forces.<sup>67</sup>

This, in turn, begs the question of why some authoritarian governments have been able to implement the control mechanisms described above while most others have not. One possible reason is that these control mechanisms require a strong party infrastructure to be effective, which would explain why only party-based autocracies have been able to successfully establish them. But this explanation is not entirely satisfying, because it does not account for the military's acquiescence during the early stage of the implementation of the system: as Nordlinger (1977, 17) notes, although this system of civilian control on the military has shown itself to be effective in maintaining the regime's stability on the long run, its implementation can prove difficult to the extent that it requires extensive political interference in military affairs, and a powerful military is likely to strongly oppose any attempt by civilian leaders to infringe on its autonomy. On the long run, the system becomes stable, but it may spark military interventions during the early years of its implementation. Thus, a more complete explanation of the lower coup risk in single-party dictatorships would first require identifying the factors that facilitated the implementation of a system of party-based control on the military.

The argument I propose is that the security environment in which most party regimes operated during the post-World War II period can partly explain why they were able to establish full civilian control on the military while most other dictatorships could not: one possible reason why China, Cuba, Tanzania, or the Soviet Union experienced few direct challenges from the military is that these countries were either surrounded by overtly

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<sup>67</sup> As noted above, prior studies usually do not make this distinction (see, for example, Gibler and Tir 2010, 958).

hostile states or regularly involved in interstate conflicts. If this explanation is correct, we should observe that party systems are more likely to emerge and to endure in threatening international environments: because of enduring external threats, incumbents were successful in both neutralizing threats coming from the military (see Desch 2001; McMahon and Slantchev 2015) and eliminate organized opposition while at the same time retaining the support of a significant part of the public (see Rasler and Thompson 2004; Gibler 2010; Hutchison 2011). By contrast, we should also observe that countries that are exposed to high internal threats are less likely to be ruled by a party regime, since power centralization requires that the military seldom participates in domestic repression. Importantly, this explanation suggests that party regimes' ability to maintain effective control on the military does not directly result from their institutional setup. Rather, both phenomena – party regimes and civilian control on the military – are the outcome of a particular security environment: because of a combination of low internal threat and high external threat, party systems inherit a quiet opposition that can easily be neutralized and a large but harmless military which attention is constantly focused on interstate conflicts rather than on domestic politics.

Thus, on the long run, patterns of civil-military relations in nondemocratic countries affect domestic political structures: countries that face relatively intense external threats and very acute internal threats are more likely to have a politically active military and, in turn, to experience intermittent military rule. By contrast, countries that face low internal threats and high external threats are characterized by a higher degree of civilian control on the military; in turn, highly centralized, closed party dictatorships are more likely to emerge and endure when the military is less prone to intervention in domestic politics.

*H4: Military dictatorships emerge by (a) high levels of internal threat and (b) intermediate levels of external threats.*

*H5: Single-party dictatorships emerge by (a) low levels of internal threats and (b) high levels of external threats*

### **4.3. EMPIRICAL ANALYSIS**

#### **4.3.1. Measuring internal and external security threats**

Measuring latent risk of interstate or intrastate armed conflict is not as straightforward as measuring the actual occurrence of such conflicts. Yet, directly measuring actual instances of intrastate or interstate conflicts is not a practicable option. As Svobik (2013a, 789) observes, directly measuring domestic turmoil or full-scale civil war can induce selection biases, because mass mobilization partly depends on the repressiveness of the regime. If military dictatorships are more likely to emerge in periods of domestic turmoil, but also more likely to respond to protests with repression (see, e.g., Poe et al. 1999; Davenport 2007b) and thus more likely to deter opponents from engaging in anti-regime activity in the first place, military intervention in politics acts as a negative confounder.

Thus, I rely on a proxy to capture the risk of intrastate armed conflict, namely the share of youths in the adult population. This measure has two advantages. First, the United Nations data (2013) – on which the measure is based – provide a good coverage with few countries missing. Second, the share of youths in the adult population has a clear theoretical link with intrastate armed conflict: although youth cohort size does not directly “cause” civil conflict, the fact that young men account for most acts of political violence is practically a self-evident truth, and research has consistently noted that revolutions, civil war outbreaks, and other major episodes of political instability are typically associated with the presence of a large proportion of youths relative to the total adult population (see, e.g., Moller 1968; Mesquida and Wiener 1999; Goldstone 2002; Collier 1999; Cincotta et al. 2003; Staveteig 2005; Urdal 2004, 2006). “Youth bulges” (i.e., a high proportion of youths aged 15 to 24) are prominent both in the “greed” and the “grievances” perspectives in the civil war literature. On the one hand, according to the “cohort size hypothesis”, the larger a youth cohort relative to the total population, the lower its income and the higher its unemployment rate: thus, grievances arise from a mismatch between employment prospects and the size of the labor force (Goldstone 2002; Cincotta et al. 2003; Staveteig 2005; Urdal 2004, 2006). On the other hand, because of lowered opportunity costs (e.g.,

the absence of alternative income-earning opportunities, or fewer responsibilities for families), young men are cheap recruits for rebel organizations (Mesquida and Wiener 1999; Collier 1999; Urdal 2006).

While some studies on civil war measure youth bulges as the proportion of young males in the total population (e.g., Fearon and Laitin 2003), I opt for the proportion of males aged 15 to 24 in the *adult* population (that is, the total population aged 15 or older), in order to avoid artificially deflating this ratio in countries with a large population under 15. As Urdal (2004, 2006) argues, the former indicator is questionable from a theoretical perspective, because if political violence arises from frustrations due to the overcrowding of the job market, it implies that only the youth-to-adults ratio (not the youth-to-children ratio) is causally related to civil conflict. Moreover, one prominent argument in the civil war literature (the “demographic bonus” or “demographic dividend” argument) is that youth bulges do not impact political violence if they are followed by significantly smaller cohorts: whereas an increased dependency ratio (i.e., a large share of nonworking population) adversely impacts economic growth, a relative increase of the working-age population may foster economic development through increased savings and thus provide youth cohorts with better employment prospects (Cincotta et al. 2003; Urdal 2006).

I therefore proxy the risk of civil war by dividing the number of males aged 15-24 by the number of males aged 15 and older, using UN data on population: The indicator ranges from less than 13% (Italy in 2008) to almost 50% (Cap Verde in 1983). Table 4.2. displays summary statistics.

**Table 4.2. Share of males aged 15-24 in the population over 15, 1950-2008.**

<i>Mean</i>	<i>Max.</i>	<i>Min.</i>	<i>Std. Dev.</i>
29.95%	48.05%	12.59%	6.71%

(Source: UN Population division, 2013)

The level of external threat is measured by the total number of direct contiguous borders of each state by land, based on the Direct Continuity dataset from the COW project (Stinnett et al. 2002). This strategy is justified by a widespread consensus in the international

conflict literature: the overwhelming majority of interstate rivalries, recurrent militarized disputes, and wars occur between neighbors. Possible explanations include the regular interactions and increased conflict potential between neighboring states, the fact that few states have a sufficient military reach to fight against non-neighbors (or, alternatively, that proximate states are generally perceived as more threatening), or the fact that neighboring states are simply more likely to have territorial disputes (for a discussion of these arguments, see Starr and Most 1976, and Vasquez 1995; for an empirical evaluation, see Senese 2005). Given that territorial disputes are more enduring, more prone to escalation, more likely to result in full-scale war, and more likely to be followed by recurrent militarized conflicts than disputes over policy or regime (see, e.g., Kocs 1995; Vasquez 1995; Hensel 1996; Vasquez and Henehan 2001; Senese and Vasquez 2003; Dreyer 2012), contiguity has emerged as one of the most powerful predictor of interstate war: for example, Wallenstein (1981) finds that most neighboring major powers have experienced war. Bremer (1992) finds contiguity to have a stronger impact on war than alliances, power parity, or economic development, and identifies it as the most decisive factor for the outbreak of a war between any pair of states. According to some accounts, war is about 300 more likely to occur between contiguous states than between noncontiguous states (Kocs 1995). A more recent study not only confirms that the probability of a militarized interstate dispute (MID) is higher for neighboring dyads than for non-neighbors, but also that contiguity additionally affects how states respond to other determinants of war, such as alliances or power parity (Reed and Chiba 2010).

Accordingly, a number of studies on the impact of the international environment on domestic structures have relied on this indicator or variations thereof to assess external threat (e.g., Gibler 2007; Gibler 2010; Gibler and Tir 2014). The measures used by these studies are arguably more sophisticated than the mere count of land borders: for example, Gibler and Tir's (2014) measure of potential territorial threat include information such as the history of violent or peaceful transfers of territory between neighbors, the onset of civil war in a neighboring country, or the existence of an active defense pact with all neighbors. I, however, prefer to rely on the raw count of land borders, because the aforementioned indicators are prone to displaying considerable short-term fluctuations that make them unsuitable to examine long-lasting effects of the international environment on domestic

political structures: states seldom disband their armed forces as soon as they start developing more peaceful relationships with their neighbors, and the military’s political involvement is unlikely to change overnight. Even if contextual factors are likely to affect the military’s resources endowment, the mere existence of land-contiguous countries should create a powerful incentive for states to maintain a standing army at all.

Table 4.3. displays summary statistics.

**Table 4.3. Land borders: Summary statistics, 1946-2008**

<i>Mean</i>	<i>Max.</i>	<i>Min.</i>	<i>Std. Dev.</i>
3.40	14	0	2.36

(Source: Correlates of War, Contiguity dataset)

#### **4.3.2. Internal and external threats and instances of military coups**

To test Hypotheses 1 and 2, I use logistic regression. Military coups – the dependent variable – are measured using Powell and Thyne’s (2011) dataset. Coups are defined as “illegal and overt attempts by the military or other elites within the state apparatus to unseat the sitting executive” (Powell and Thyne 2011, 252). I do not distinguish between successful and failed coups, and I collapse country-years with multiple coups into a single value.

**Table 4.4. Coup attempts, 1950-2005**

<i>Frequency</i>	<i>Percent</i>	<i>Total observations</i>
377	5.15	7,326

(Source: Powell and Thyne 2011)

The model further includes several control variables that can affect the expected relationship between security threats and military coups, and whose omission might bias the results. First, large countries – both in terms of population and area – are likely to have a larger number of land borders than smaller ones: I thus include the log of population and the log of country size in square kilometers in the model (data on both variables come

from the UN). Second, the share of youths in the population is strongly correlated to the level of economic development, as measured by GDP per capita: to the extent that coups happen more frequently in poorer countries (Londregan and Poole 1990), GDP per capita is a potential positive confounder. I thus add the natural log of GDP per capita in the model: data are drawn from the Maddison project dataset (Bolt and van Zanden 2013). For a similar reason, I add a Cold War dummy, since the frequency of both international conflicts and military coups has decreased after 1990 (Powell and Thyne 2011). Two further possible correlates of coups, namely ethno-linguistic fractionalization (Jenkins and Kposowa 1990, 1992) and economic growth (O’Kane 1981; Johnson et al. 1984; Londregan and Poole 1990) are also included. Data on ethnic fractionalization are drawn from the Ethnic Power Relations dataset (Cederman, Wimmer and Min 2010).

**Table 4.5. Summary statistics (1950-2005)**

	<i>Mean</i>	<i>Standard deviation</i>	<i>Observations</i>
(ln) Country size	12.30	1.81	7,284
(ln) Population	15.93	1.53	7,256
(ln) GDP/capita	7.92	1.06	7,054
Economic growth	0.02	0.06	6,943
ELF	0.41	0.31	6,858

(Sources: World Bank; Maddison project database; Cederman et al. 2010)

Finally, I address potential time-dependence issues by including a variable that records the number of years passed since the last occurrence of a coup. Standard errors are clustered at the country level. Table 4.6. displays the results.

Results displayed in Table 4.6. lend support to the two hypotheses. Consistently with Hypothesis 1, coup risk increases monotonically with the share of males aged 15-24 in the adult population, the variable being significant at the 1% level. For each unit increase in the percentage of youths in the population, the estimated coup risk increases by 5%. Given that the youths-to-adults ratio varies from about 10% to almost 50% in the sample, this effect is non-negligible: It means that, everything else being equal, a country with 50% of young males in its adult population is about 7 times as likely to experience a coup as a country with 10% of young males in its adult population. If the variable increased from its

minimal value (0) to its maximal value (100) in an average country, the odds of this country experiencing a coup would increase by a factor of about 130.

**Table 4.6. Internal threats, external threats, and coup risk, 1950-2005 (logistic regression, reporting odds ratios)**

	<i>Model 1: Military coups</i>
Years since past coup <sup>a</sup>	0.918*** (0.010)
GDP/capita <sup>a, b</sup>	0.913 (0.084)
Population <sup>a, b</sup>	0.937 (0.061)
ELF	1.224 (0.278)
Cold War	1.993*** (0.337)
Land borders	1.393*** (0.162)
Land borders (squared)	0.965*** (0.012)
Country size <sup>b</sup>	1.110 (0.075)
% males aged 15-24 <sup>a</sup>	1.054*** (0.017)
Growth <sup>a</sup>	0.056*** (0.054)
Constant	0.011*** (0.015)
Countries	147
Observations	6,286

Robust standard errors in parentheses

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

<sup>a</sup>Lagged one year <sup>b</sup>Log

Hypothesis 2 is confirmed as well: The predicted curvilinear relationship between external threats and military coups, approximated by the joint inclusion of a linear and quadratic term for the number of land borders in the model, is reflected by the positive coefficient associated with the linear term and the negative coefficient associated with the quadratic term. This indicates that, as expected, the risk of military coups first increases and then decreases with the level of external security threats. Both coefficients are significant at the 1% level.

Control variables, on the whole, impact the dependent variable in the expected way, but only some of them attain statistical significance. Economic growth displays a strong association with coup risk – coups becoming far less likely as growth rates increase. As expected, coups were more frequent during the Cold War than afterwards. Finally, I also find empirical support for the notion of “coup traps” (Londregan and Poole 1990): Countries that experienced a coup in the recent past are more likely to experience another one, this risk decreasing by 8% each year. By contrast, ELF and population are not remotely significant. The most surprising finding is that GDP per capita has no discernible impact on coup risk. However, the variable attains statistical significance as soon as the share of youths in the population is removed from the model, and displays an actually quite large (negative) effect on the dependent variable. This can, on the one hand, suggest that economic development has only an indirect effect on coups: economic development affects age structures which, in turn, affect coup risk. On the other hand, this finding can also be due to multicollinearity: given the high correlation between GDP per capita and share of youths in the population, these results should be approached with caution. Note, however, that the results regarding Hypothesis 1 are not due to multicollinearity: estimates remain quite stable regardless of whether GDP per capita is included in the model.

#### **4.3.3. Security threats and political regime type**

Hypotheses 3, 4, and 5 relate to the impact of internal and external security threats on the emergence and survival of political regimes. I begin with Hypothesis 3, which involves a comparison between democracy and dictatorship. It will be tested on the whole sample, using a binary dependent variable that takes on the value of 1 if the country is classified as democratic, 0 otherwise. To measure democracy, I use data from the Democracy & Dictatorship dataset (Cheibub et al. 2010).

The model includes, in addition, a small set of control variables. Three of these – GDP per capita, ethno-linguistic fractionalization, and the number of democratic regimes in the world – are relatively consensual covariates of democracy. Economic development is one of the most robust predictors of democracy (see Lipset 1959; Burkhart and Lewis-Beck 1994; Barro 1999; Przeworski and Limongi 1997; Boix and Stokes 2003; Epstein et al.

2006). Ethnic diversity, on the other hand, is widely believed to be detrimental to the development and persistence of democratic institutions (see Rabushka and Shepsle 1972; Horowitz 1993; Aghion et al. 2004; and, to a certain extent, Collier 2009). Finally, the yearly count of democratic countries in the world is intended to capture diffusion effects (see Przeworski et al. 1996; O’Loughlin et al. 1998; Brinks and Coppedge 2006).

The last control variable – population – is not a “standard” predictor in democratization studies: several authoritative studies on the determinants of democracy either fail to uncover any relationship between population and democracy (e.g., Barro 1999; Acemoglu et al. 2005, 2009) or do not consider it at all (e.g., Boix and Stokes 2003; Epstein et al. 2006). Yet, small population has long been deemed a prerequisite for the successful establishment of democracy (see Dahl and Tufte 1973), and small states are notably more likely to be democratic than large ones. Noting that small states receive by far better ratings on the Freedom House scale, Diamond and Tsalik (1999) argue that the size of political communities impacts a large number of outcomes such as the sense of political efficacy among citizens, political accountability, the representation of marginalized groups, and checks on the central government’s power. A related argument is that large polities are more likely to be ethnically or socially fragmented (Dahl and Tufte 1973). In a somewhat similar vein, several studies have argued that small states offer better conditions for the development of democracy by promoting an increased sense of community, mutual trust and solidarity among citizens (e.g., Anckar 1999, Ott 2000, Congdon Fors 2014). Since population is a potential correlate of democracy, it is therefore a potential confounder: omitting population from the model could introduce a bias inasmuch as countries with a low number of land borders are more likely to be small countries with a small population. Hypotheses 4 and 5 predict that, under certain structural conditions, a dictatorship will be more or less likely to take the form of a military regime or a party regime, respectively. The test for both hypotheses relies, again, on logistic regression. Dependent variables are measured using Cheibub et al.’s (2010) classification of nondemocratic regimes (see Chapter 2). Control variables are the same as in the previous model. Thus, the models used to test Hypotheses 4 and 5 are broadly similar to the one used to test Hypothesis 3, with two notable differences: First, instead of using a yearly count of democracies as control variable, I use a count of similar regimes (that is, the number of military dictatorships in

the world is used to predict the probability that a particular country is a military dictatorship at a certain point of time; the same goes for party regimes). Second, for Hypotheses 4 and 5, the sample is restricted to nondemocratic regimes, in order to avoid distorting the results: to the extent that GDP per capita is on average much higher in democracies than in autocracies (and that economic development, in turn, is likely to influence other variables such as age structure), including democracies in the sample could inflate or deflate the mean value of some of the predictors and therefore mask important differences between authoritarian subtypes.

Table 4.7. displays the results.

**Table 4.7. Security threats and political regimes, 1950-2005 (logistic regression, displaying odds ratios)**

	<i>Model 2: Democracy</i>	<i>Model 3: Military</i>	<i>Model 4: Party</i>
GDP/capita <sup>a, b</sup>	2.478*** (0.468)	0.813 (0.172)	0.509*** (0.119)
Population <sup>a, b</sup>	1.360** (0.192)	1.232 (0.172)	1.088 (0.147)
ELF	0.568 (0.334)	0.237** (0.140)	1.027 (0.508)
Land borders	0.762*** (0.053)	1.548** (0.312)	1.178** (0.087)
Land borders (squared)		0.955** (0.021)	
Similar regimes (count)	1.020*** (0.005)	1.030*** (0.010)	1.059*** (0.014)
% males aged 15-24 <sup>a</sup>	0.928*** (0.027)	1.079** (0.037)	0.824*** (0.033)
Countries	149	113	113
Observations	6,446	3,902	3,902

Robust standard errors in parentheses

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

<sup>a</sup>Lagged one year <sup>b</sup>Log

Estimates regarding democracy are displayed in Model 2. Hypotheses 3a (concerning internal security threats) and 3b (concerning external security threats) are both supported by the data: the proportion of youths in the adult population and the number of land borders are both negatively associated with a country's odds of being a democracy, and both significant at the 1% level. Land borders have a particularly strong effect, each additional border decreasing the odds by almost 25%. The effects of age structure are less

dramatic, but still non-negligible: for each additional percentage point, the odds decrease by a factor of 0.93. Given that the youths-to-adults ratio varies from roughly 10% to almost 50% in the sample, this means that, if the variable increased from its minimal to its maximal value in an average country, the odds of this country being democratic would decrease by about 280%.

In Model 3, the positive coefficient associated with the count of land borders and the negative coefficient associated with the quadratic term of land borders support Hypothesis 4b: The likelihood of a country being a military dictatorship first increases and then decreases with its number of neighbors. Both coefficients are significant at the 5% level. Likewise, the positive and statistically significant coefficient associated with the proportion of youths in the adult population lends empirical support to Hypothesis 4a: the higher the youths-to-adults ratio in one country's population, the higher the probability that this country is ruled by the military. Finally, Hypotheses 5a and 5b are both supported by the data, as can be seen in Model 4: party regimes are associated with a comparatively high number of land borders and a comparatively low proportion of young males in the adult population. According to the estimates in Model 4, each additional land border increases a country's odds of being a party dictatorship by nearly one fifth ( $p < .05$ ), while each unit increase in the ratio of youths to the adult population decreases the odds by a factor of 0.82 ( $p < .001$ ). The population's age structure – and therefore the risk of internal armed conflict – hence impacts party dictatorship even more dramatically than it impacts democracy.

With regard to control variables, the yearly count of similar regimes in the world has a positive influence on the dependent variables in each model. As expected, GDP per capita is positively related to democracy. It has, by contrast, no impact on a country's probability to be ruled by a military dictator, and a negative and significant effect on the likelihood that a country is a one-party regime. Even though none of the estimations return any anomaly, two surprising findings emerge: first, ethnic fractionalization is negatively correlated to military dictatorship in Model 3. This is unexpected inasmuch as ethnic diversity is generally deemed a powerful predictor of domestic conflict in general, and military coups in particular (see Jenkins and Kposowa 1990, 1992). Second, contrary to the widely held belief that democracy is associated with small political communities, I find that population size correlates positively with democracy. This result might seem puzzling

at first glance. However, it is not exceptional (see, e.g., Barro 1999), and it echoes one recent finding, that large populations enhance political competition at the district level (Gerring et al. forthcoming): since Cheibub et al.'s (2010) measure of democracy is predominantly a measure of contestation, this finding is perhaps not so startling after all. More broadly, it has two implications. First, it suggests that, to the extent that past research has uncovered a negative relationship between population size and democracy, this relationship might be spurious (i.e., driven by the fact that many small countries are islands, and many islands are democracies), and that more research is needed to disentangle the effects of demography and geography on political institutions. Second, this finding indicates that if insulated countries are more likely to be democratic, this is not because of their small population, but rather because of a relatively secure environment.

#### **4.3.4. Robustness checks**

In addition to the tests discussed above, I perform a series of robustness checks.<sup>68</sup> I first check whether the results are driven by a few influential observations (i.e., country-years that take on extreme values on one or several of the independent variables): this does not seem to be the case. Second, I investigate the results' sensitivity to the inclusion or removal of control variables. In particular, since the proportion of youths in the population is highly correlated with GDP per capita, I check whether the main results hold up when GDP is removed from the model. Yet, the proportion of youth remains statistically significant in all models. The results are not sensitive to the removal of other controls either.

I then retest Model 2 after replacing Cheibub et al.'s (2010) democracy measure with an alternative binary measure developed by Boix, Miller and Rosato (2013): the results are not affected. Likewise, they hold when the dependent variable in Model 1 is replaced by a measure of successful coups only (Powell and Thyne 2011).

Finally, I investigate the results' sensitivity to model specification, and re-estimate all five models using probit, rare-event logit and a random effects model.<sup>69</sup> I find that probit results

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<sup>68</sup> Estimates that are not displayed in the text are available on request.

<sup>69</sup> Given that military coups are comparatively rare, and that both political regimes and the independent variables exhibit a low variance over time, fixed effects models are inappropriate.

are highly similar to logit results: no main findings are overturned. The same is true of the rare events corrected estimations: the exponentiated coefficients associated with the independent variables remain the same as in the logit model, with a two-digit precision. Although random effects models are more severe, most findings remain unaffected. The main problem that arises regards Models 1 and 3: although land borders retain their effects and statistical significance in these models, estimates regarding the proportion of youths in the population become unstable. It turns out, however, that this is mainly due to the high correlation between age structure and GDP: youth bulges are positively and significantly associated with both military coups and military regimes as long as GDP is left out of the model.

#### **4.4. CONCLUSION**

All states are confronted with the well-known trade-off between security through and security from the armed forces: as they face increasingly acute security challenges, leaders often react by endowing the military with institutional or financial resources, thereby increasing the risk that it becomes powerful enough to blackmail, overthrow or replace them. One implication of this idea is that coup risk partly depends on the security threats states face.

I find support for this expectation to the extent that countries facing comparatively low levels of security threats are less likely to experience military coups. But coup risk also varies according to the nature of security threats. Because a high risk of international war fosters the military's neutrality while a high risk of civil war increases its incentives to get involved in domestic politics issues, these two types of security threats have a different effect on coup risk: internal threats monotonically increase the risk of a military intervention, whereas the relationship between external threats and coup risk takes the form of an inverted U. I also find support for the hypothesis that countries facing few serious security threats are, on the long run, more likely to become and remain democratic (see Midlarsky 1995; Thompson 1996; Gibler 2007; Gibler and Tir 2010). The nature and intensity of security threats also help predict the emergence and persistence of specific types of authoritarian regimes: because moderately high external security threats and very

high internal security threats give the military a central position in the political system and incentives to intervene in domestic politics, such a combination often results in the emergence of military dictatorships. By contrast, countries in which internal security threats are very low but external security threats are very high are more likely to be ruled by a highly centralized party regime.

These results have to be approached with caution – if only because the indicators used to measure internal and external security threats do not perfectly match the concepts – and would need refinement. But in spite of these limitations, these findings contribute to a growing literature on the impact of security threats on coup risk, and provide a further confirmation of Desch's (2001) hypothesis, that a hostile international environment is not necessarily detrimental to civilian control on the military. On the other hand, they also demonstrate that civilian control on the military cannot be equated with democracy, to the extent that it can also contribute to authoritarian stability.

The results sketched above also shed light on some recurring findings such as, for example, the fact that small and insulated states are more likely to be democratic than large or continental states: the persistence of democracy in island states is not necessarily due to cultural factors, and might instead be due to the fact that, in those countries, the military never was powerful enough to pose a serious threat to democratic institutions. Likewise, the finding that high external threats decrease the likelihood of a military coup helps explain why authoritarian leaders who face a high coup risk frequently initiate international conflicts (Miller and Elgün 2011; Powell 2014b).

**PART II:**  
**REBELLION UNDER DEMOCRACY AND DICTATORSHIP**

## 5

### **Intrastate armed conflicts and military coups in democratic and autocratic regimes**

This chapter deals with the effects of political regime type on the outbreak of intrastate armed conflict: the objective is to re-examine theoretical arguments on this relationship and empirically assess their validity. Special attention will be devoted to measurement issues, which tend to plague many studies on regime type and domestic conflict and have generated a great deal of controversy.

The first aspect of the problem I will address here concerns the relationship between democracy and intrastate armed conflict. The emerging consensus is that there is no “domestic democratic peace:” very few studies, if any, identify a monotonic relationship between democracy and intrastate armed conflict (for a review, see Hegre 2014). Yet, the overwhelming majority of these studies rely on the Polity index, which displays some particular properties that make it inappropriate for predicting intrastate armed conflicts (see Vreeland 2008). These issues are now well-known, but curiously, very few cross-national analyses on the relationship between democracy and civil war have so far been performed on the basis of measures other than the Polity IV index.<sup>70</sup> I will thus bridge this lacuna and reassess the relationship between democracy and civil war using a minimal, dichotomous democracy measure.

A second aspect of the problem concerns the differences between authoritarian regimes. In recent years, scholars have begun to move beyond simple bipartite or tripartite regime

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<sup>70</sup> Although the Freedom House index is often used as a proxy for democracy, it is not a democracy measure per se: it was originally developed as a measure of political rights and civil liberties, and does not include components measuring other aspects of democracy. Some studies on the effects of political regimes do not rely on Polity IV (Cohen 1997; Reynal-Querol 2005; Keefer 2007; Schneider and Wiesehomeier 2008) but they investigate the effect of particular democratic institutions, not the effect of democracy as a whole.

classifications, and to analyze how “qualitative” differences<sup>71</sup> between authoritarian regimes affect the risk of civil war. Authoritarian regime, so the argument goes, can be expected to behave in a different way, irrespective of their degree of “democraticness:” who governs and how political power is allocated between different groups is crucial for understanding the manner in which each regime will deal with organized opposition groups; in turn, these strategies of political control affect the opposition’s propensity to comply or resort to violent means to pursue its preferences. Three recent studies adopt such an approach and find significant differences between authoritarian regime types: Gurses and Mason (2010) find party regimes to be relatively immune to intrastate conflict, because they are able to either legitimize their rule through an all-encompassing ideology or to secure loyalty through corporatist bargaining between regime elites and representatives of important civil society groups. Fjelde (2010) relies on a broadly similar approach and finds a robust association between regime type and civil war onset. Specifically, she finds party regimes to be the most resilient and military dictatorships to be the most vulnerable: the underlying argument is that, while the former are internally cohesive and able to secure co-operation from both elites and citizens through a wide range of coercion and co-optation strategies, the latter are more brittle because of their insulation, the absence of a coherent ideology to mobilize mass support, and their lack of cohesiveness at the elite level, and often rely exclusively on raw repression to maintain their rule. Her findings are confirmed by a recent study by Wilson (2014): though Wilson is somewhat critical of Fjelde’s approach and argues for measuring co-optation capacity directly instead of using regime classifications, authoritarian regime type remains a significant predictor of domestic conflict in his replication.

Yet, because they primarily rely on casualties to define intrastate armed conflict, datasets on civil war often conflate rebellions and military coups into a single measure (Powell and Thyne 2011). This is the case of the UCDP/PRIO Armed Conflict Dataset, used by both Fjelde (2010) and Wilson (2014), and – to a lesser extent – of the Correlates of War dataset, used by Gurses and Mason (2010). This is problematic for two reasons: first, Fjelde’s (2010) and Wilson’s (2014) approach to co-optation and coercion – like most theories linking political regime type to the outbreak of civil war – explains only conflicts

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<sup>71</sup> The expression is from Hadenius and Teorell (2007a).

initiated by opposition groups against the regime apparatus, not intra-elite conflicts: thus, if further analyses revealed that authoritarian regime type impact civil wars and military coups in a similar way, this would indicate that something is wrong with the theory. Second and most importantly, given the relative rarity of civil wars – and the fact that there is a direct theoretical relationship between authoritarian regime type and military coups (see Chapter 3) – there is a possibility that the relationship between authoritarian regime type and civil wars is entirely driven by the inclusion of some military coups in civil war datasets. Therefore, it is necessary to test whether authoritarian regime types are related to civil war outbreak because they impact rebellion or because they affect coup risk.

The chapter is organized as follows: Section 5.1. offers a brief overview of the literature on political regimes and the outbreak of intrastate armed conflicts. Section 5.2. discusses some important problems in the measurement of intrastate armed conflict: the main point is that, while measures that rely on a low casualties threshold (such as the UCDP/PRIO Armed Conflict dataset) are generally preferable for assessing the relationship between political regime and domestic conflict, the main drawback of such data is that they do not allow for a careful distinction between rebellions and military coups. In Section 5.3., I will discuss theoretical arguments on the relationships between regime type, intrastate armed conflicts, and military coups: in particular, it will be examined whether there are theoretical reasons to expect the relationship between regimes and rebellions to be entirely driven by the relationship between regimes and coups. I will then proceed to test my hypotheses on a dataset that covers the period from 1950 to 2005: section 5.4. presents the data and the results of the empirical analysis. Finally, Section 5.5. reports the results of an additional test performed on a new dataset that covers the period from 1816 to 1913. Section 5.6. concludes.

## **5.1. BACKGROUND: DOES POLITICAL REGIME INFLUENCE THE RISK OF CIVIL WAR?**

So far, most of the quantitative, cross-national literature on political regimes and civil wars has focused either on whether democracy reduces the risk of civil war (hence treating authoritarian regimes as a residual category) or on the hypothesis that semi-democratic

regimes are more conflict-prone than either democracies or pure authoritarian regimes. In both cases, the empirical evidence is mixed. Democracy seems to decrease the severity of violent conflicts (Benson and Kugler 1998; Lacina 2006) and there is growing evidence that some specifically democratic institutions are associated with a linear decrease of the probability of conflict (see e.g. Cohen 1997; Reynal-Querol 2002a, 2002b, 2005; Carey 2007; Schneider and Wiesehomeier 2008; Gleditsch and Ruggeri 2010). Yet, there is overall no strong evidence of a monotonic relationship between democracy and civil war (Hegre 2014).

With regard to semi-democratic regimes, numerous studies have identified an inverted U relationship between democracy level and civil war: using Polity data (Marshall et al. 2011), they find that “hybrid” or “anocratic” regimes (i.e., regimes that are situated in the middle of the democracy-autocracy continuum) are systematically associated with higher levels of political violence (Henderson and Singer 2000; Hegre et al. 2001; Fearon and Laitin 2003; see also Muller and Weede 1990). The argument underlying this finding is that democracies are able to decrease the desirability of rebellion and autocracies to increase its costs (or limit the opposition’s opportunities to challenge the regime); by contrast, hybrid regimes typically allow opposition groups to mobilize support but do not provide them with peaceful strategies to access political power. Although the general argument is theoretically compelling, a study by Vreeland (2008) has challenged this finding on a methodological ground, showing that the Polity index includes several components that assign intermediate scores to countries experiencing political violence.<sup>72</sup> Thus, the statistical relationship uncovered by these studies is (at least partially) endogenous. After removing the biased components from the Polity index, Vreeland does not find any evidence for a U-shaped relationship between democracy and civil war. A more recent study by Regan and Bell (2010) has attempted to reassess the anocracy hypothesis. After controlling for the potential bias in the Polity data, they find that the association between hybrid regimes and civil war is mostly due to their transitional character: regime type per se does not seem to have any effect on the probability of conflict.

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<sup>72</sup> These components measure the degree of regulation of political participation (PARREG) and the degree of political competition (PARCOMP), respectively (see Vreeland 2008; Marshall et al. 2011). Both components take on intermediate values when partisan competition involves violence.

Thus, the empirical evidence for a causal relationship between political regime and civil war is weak at best. However, this could result from the widespread use of highly aggregated democracy measures: since the Polity index – on which most of past research is based – conflates different institutional features into a single dimension, the effects of institutional differences between regimes that reach the same score on the scale are not taken into account. Three recent studies, moving beyond this single-dimensional approach to political regimes, investigate these “qualitative” differences between nondemocratic regimes and their effects on intrastate armed conflict. Gurses and Mason (2010) contrast “exclusionary authoritarian regimes” (which they equate with the personalist autocracies from Geddes’ [1999] classification), that are characterized by weak institutions and a high vulnerability to elites defections and violent contestation, with “inclusionary authoritarian regimes” (i.e., one-party autocracies), that are able to legitimize their rule through ideology and to maintain their stability through a mix of sanctions and rewards (Gurses and Mason 2010, 143-144). Using Geddes’ (1999) data on authoritarian regimes, they find personalist regimes to run a higher risk of civil war than single-party regimes and monarchies. Fjelde (2010) also finds party regimes to be the least vulnerable to intrastate armed conflicts. Her results differ from those of Gurses and Mason in that she consistently identifies military regimes as running a higher risk of conflict than other authoritarian regimes. She argues that political regime type affects the extent to which the dictator is able to resort to co-optation or repression: this, in turn, explains why military dictatorships (that she argues rely almost exclusively on repression, which can easily backfire) are more conflict-prone than the average of authoritarian regimes, while single-party regimes (that are, according to her, able to co-opt opposition groups) are the most stable category. Finally, Wilson (2014) replicates and extends Fjelde’s (2010) analysis by controlling for the existence of democratic institutions in authoritarian regimes, and confirms her findings about military dictatorships and single-party regimes.

The problem with these studies is that they rely heavily on untested assumptions about both authoritarian regimes’ tendency to either co-opt or repress the opposition and the impact of co-optation and repression on the outbreak of armed conflict (the only – partial – exception being Wilson [2014], who argues against the use of regime typologies to measure co-optation capacities). This is problematic for two reasons. First, there are more

direct ways of examining the extent to which authoritarian regimes co-opt or repress and of testing the impact of co-optation and repression on armed conflicts (direct measures of political repression abound, and there are some ways of measuring co-optation: see, e.g., Arriola 2009). Second, most these arguments lack empirical evidence. For example, there is to date no evidence that co-optation reduces the opposition's propensity to rebel, and it is unclear whether co-optation is targeted at opposition parties or at key elite groups to begin with (see, e.g., Gandhi 2008a; Magaloni 2008; Gerschewski 2013). Relatedly, while it is now relatively well-established that party regimes are able to secure intra-elite cohesion and to tie key actors (military, business elites, etc.) to the regime (Geddes 1999; Brownlee 2005; Magaloni 2008; Magaloni and Kricheli 2010; Svobik 2012), these approaches say little about party regimes' ability to mediate conflicts between government and citizens. Likewise, there is no evidence that military regimes are unable to practice co-optation (see Geddes et al. 2014) or that regimes that do not practice co-optation are more repressive (Frantz and Kendall-Taylor 2014). Finally, Fjelde's (2010) claims, that military regimes are more repressive than average, and that repression leads to increased levels of violence, are not backed by solid empirical evidence (see Davenport 2007a, 2007b; Geddes et al. 2014). In short, relying on broad regimes categories and speculating about how each type is likely to deal with organized opposition is both unnecessary and potentially misleading. Yet, in spite of these shortcomings, these studies – by unpacking the heterogeneous category of nondemocracies according to their difference in nature instead of their democracy level – make a strong case against the widespread use of single-dimensional democracy measures, and suggest an interesting avenue for future research.

To sum up, according to the current state of knowledge, democracy appears to be unrelated to the risk of intrastate armed conflict, but this finding might be due to the widespread practice of using highly aggregated, single-dimensional measures of political regimes. In turn, there is some evidence that this practice is flawed, to the extent that not all authoritarian regimes exhibit the same probability of experiencing armed conflict. If there is any lesson to be drawn from the current state of the arts, it is that measures of political regimes are not interchangeable, and that the choice of a specific dataset is likely to drastically affect the observed relationship between regimes and conflicts. Yet, such findings are also likely to be affected by the way intrastate armed conflicts are measured:

curiously, this topic has received much less attention in current discussions about the impact of political institutions on armed conflicts. Issues related to the measurement of intrastate armed conflict will be discussed in the following section.

## **5.2. ON CONCEPTS AND NUMBERS: EXAMINING THE REGIME TYPE – CIVIL WAR RELATIONSHIP WITH QUANTITATIVE DATA**

As discussed in the last section, empirical findings about the relationship between political regimes and intrastate armed conflict seem to depend to a large extent on the peculiarities of each measure of political regime. But does the same hold true for civil war datasets? Measures of domestic conflict – unlike measures of political regimes – rely at least on a common definition of the concept. Most existing data collection efforts rely on a broadly similar operational definition of civil wars (see, e.g., Small and Singer 1982; Doyle and Sambanis 2000; Gleditsch et al. 2002; Fearon and Laitin 2003): virtually all of these definitions emphasize a common set of criteria, which can be summarized as follows:

- (1) Civil wars are fought over political issues, pertaining either to the government or the territory of a state, or both.
- (2) They are fought within domestic borders, and among domestic actors.
- (3) They involve the active participation of the state’s government and its army.
- (4) They involve the use of armed force between two or more organized parties.
- (5) They result in a certain number of battle-related fatalities.

These criteria are intended to distinguish intrastate armed conflicts from other, distinct forms of collective violence, such as interstate conflicts, coups, organized crime, terrorism, riots, politicide, or ethnic cleansing. Yet, their apparent simplicity is deceiving: deciding how “armed force” should be coded (i.e., what exactly counts as a weapon) or what “parties” are (i.e., to which extent they have to be organized) is no easy task. Likewise, the notion of “domestic borders” is somewhat ambiguous: most datasets – typically, the Correlates of War dataset (Small and Singer 1982) or the UCDP/PRIO Armed Conflict Dataset (Gleditsch et al. 2002) – treat civil wars and “extrastate” or “extrasystemic” conflicts (i.e., conflicts occurring between a state and non-state actors outside of the metropole, such as, e.g., colonial wars) as separate categories. Yet, as Sambanis (2004, 816) observes, distinguishing intrastate from extrasystemic wars is often difficult, since

some separatist conflicts can be viewed as wars of decolonization.<sup>73</sup> More generally, attempts to provide for a general definition of civil war (that is, one that could apply to a wide range of cases) raise two kinds of issues. First, it can be asked whether these definitions do not lump together very diverse phenomena, which would lead researchers to overlook important empirical associations to the extent that different kinds of civil war may have different causes and consequences (see, e.g., Sambanis 2001b). Second and conversely, these definitions tend to draw arbitrary boundaries between forms of political violence that could actually be causally interrelated and conceptually similar. As Sambanis (2004, 815) puts it:

In the end, it may be difficult to study civil war without considering how groups in conflict shift from one form of violence to another, or it may be profitable to analyze political violence in the aggregate, rather than cut across that complex phenomenon with arbitrary definitions.

Consequently, a growing attention has been devoted to the shifting shapes of political conflict in recent years. For example, Besley and Persson (2009) analyze how state capacities determine whether violent conflict between state and political opposition takes the form of repression or civil war: they find that, while the richest states are at peace and the poorest ones tend to experience civil war more often, repression is widespread at intermediate levels of economic development. De la Calle and Sánchez-Cuenca (2012) use a similar approach to predict whether opposition groups will resort to terrorism or to large-scale violence. Taken together, these findings suggest that political violence is best conceived as a polymorphous phenomenon, and highlight the necessity to go beyond simple, binary definitions of intrastate conflict.

In spite of the inherent difficulty of correctly measuring civil war, however, the most contested feature of these definitions – and also the decisive criterion for choosing one dataset instead of another – is the battle-deaths threshold. All quantitative measures of civil

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<sup>73</sup> Curiously, this aspect has received so far only limited attention. In my opinion, the real question is not how to distinguish civil wars from wars of decolonization, but rather whether it makes sense (theoretically speaking) to distinguish them at all. To the best of my knowledge, Fearon and Laitin (2003) are the only ones who include anti-colonial wars in their list of civil wars. They explicitly state (p. 76) that they see no reason why they should exclude these wars from their analysis. There are, of course, reasons that would speak against the inclusion of these conflicts in some empirical analyses – depending, again, on the hypotheses being tested – but such coding decisions are not self-evident and have to be justified.

war rely on some fatalities threshold conflicts have to reach to be included in civil wars datasets, but this requirement varies greatly from one data project to another: the COW project sets this threshold to 1,000 battle-deaths per year,<sup>74</sup> while the UCDP/PRIO Armed Conflict dataset relies on a less restrictive threshold of 25 battle-deaths.

The most obvious, potentially controversial question is whether operational definitions and measures of civil war should rely on a battle-deaths threshold at all: the implication that 999 deaths (or 24, depending on the threshold being used) are somehow not “enough” for an event to be coded as a war violates common sense. Yet, common sense also suggests that battle-related casualties are part of the very definition of war (and, by extension, civil war): everyone would agree that a conflict that does not result in any fatality is not a war, and after all, the fact that civil wars cause deaths is probably one of the most important reasons why we study them. Since the concept of war is dichotomous in nature while deaths counts are not, such thresholds are hence both necessary and arbitrary. Moreover, from a more practical point of view, analyzing the probability of civil war requires being able to identify the beginning of the event, and hence determining a cut-off point to distinguish between events and non-events.

The second question is how high this threshold should be.<sup>75</sup> As noted above, this is the main difference between the COW and the UCDP/PRIO measures of civil war:<sup>76</sup> while the 25 fatalities threshold on which the UCDP/PRIO dataset relies makes it a relatively inclusive dataset, the COW project has been subject to intense criticism for excluding from its war list major episodes of violence that just failed to pass the 1,000 fatalities threshold while fulfilling every other criteria.

Some of these critiques rest solely on appeals to common sense. For example, Gleditsch et al., referring to the Northern Ireland conflict (which never qualified for the COW battle-deaths threshold), point to the “*apparent anomaly* of excluding such *well-known* conflicts from a dataset on armed conflict” (Gleditsch et al. 2002, 617; emphasis added). Sambanis

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<sup>74</sup> This is true of most measures of civil war (e.g. Doyle and Sambanis 2000; Fearon and Laitin 2003).

<sup>75</sup> A third, related question (which I will not discuss here) is whether measures of civil war should be based on an annual or total deaths count (for a discussion, see Sambanis 2001a, 2004).

<sup>76</sup> Note, however, that the UCDP / PRIO Armed Conflict Dataset distinguishes between “armed conflicts” (resulting in fewer than 1,000 battle-related deaths per year) and “wars” (at least 1,000 battle-related deaths per year; see Gleditsch et al. 2002, 619). The debate, thus, is not a conceptual one (i.e., whether low-scale conflicts should be called wars), but rather an empirical one (whether low-scale conflicts should be included in statistical analyses).

(2001a) suggests adapting the battle-deaths threshold to each country's population size (i.e., replacing the absolute number of deaths with a per capita measure): "The argument in favor of such a measure," he argues, "would be that it would allow us to distinguish events of civil violence that are *significant* within the context of a country's size" (Sambanis 2001a, 6; emphasis added).<sup>77</sup> These arguments, however, are not entirely convincing: according to which specific criteria should the Northern Ireland conflict be included in civil war datasets, and what kind of methodological issues do arise when similar conflicts are excluded? After all, neither significance<sup>78</sup> nor visibility belongs to the core definitional features of civil war.

Sambanis (2001a, 2004) gives yet another reason why researchers should use either a lower absolute fatalities threshold or a per capita threshold: from a purely statistical point of view, conflicts that cross the 1,000 fatalities threshold are more likely in larger (i.e., populous) countries than in smaller ones. While this problem is partly corrected by including a control for population in statistical models, this selection bias might precisely be the reason why empirical studies on civil war repeatedly find a positive correlation between population size and civil war.

There are other methodological reasons for adopting a lower battle-deaths threshold. One of these reasons is purely statistical in nature: rare events are in general difficult to analyze, because most statistical procedures tend to underestimate their probability (King and Zeng 2001), and because the low proportion of ones makes the result sensitive to the inclusion or deletion of a few influential observations. Thus, the higher the battle-deaths threshold, the higher the proportion of zeroes, and the higher the probability of both Type I and Type II errors. A related problem is that highly asymmetric dependent variables limit possibilities in terms of sampling and model specification (Gleditsch et al. 2002, 617).<sup>79</sup>

Another potential issue is that conflict onsets are coded in the first year in which conflicts

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<sup>77</sup> For similar reasons, Sambanis (2001a) also calls for the inclusion of civilian deaths in the fatalities counts, to the extent that civil wars typically kill more civilians than combatants and that battle-deaths "do not reveal the extent of civilian suffering that takes place in civil wars" (p. 8). Yet, restricting deaths counts to battle-related deaths has a practical advantage, to the extent that it automatically excludes instances of one-sided killings in which the targeted group was not able to mount effective resistance. Relying on a total death count to measure civil war would result in blurring the boundaries between two-sided violence and state-sponsored mass killings.

<sup>78</sup> To be sure, civil wars generally are significant events, but not all significant events are civil wars.

<sup>79</sup> This, of course, does not mean that considerations of practical convenience should override concerns about measurement validity.

reach the battle-deaths threshold. Arguably, some conflicts may escalate slowly, i.e. produce fewer than 1,000 deaths in the first year(s) and subsequently escalate to large-scale violence. Consequently, in some cases, the duration of the conflict may be truncated, and its start and end dates incorrectly identified: this, in turn, might lead to reverse causation issues to the extent that even lower-scale conflicts can have an influence on, e.g., economic growth or political regime type (that is, typical predictors of civil conflict). This is less of a concern in the case of the UCDP/PRIO dataset (the lower battle-deaths threshold leaves less room for error), but some middle-intensity conflicts might last for years until they enter the COW war list.

Most importantly, high battle-deaths thresholds automatically exclude conflicts that break out but never escalate because they are either settled or repressed immediately: it is thus unsurprising that state capacities are regularly found to be one of the most powerful predictors of large-scale violence, since weak states do not have the capacity to effectively repress rebel groups or accommodate their demands. This, however, does not mean that grievances do not impact the probability of conflict (or, alternatively, that strong states do not produce grievances): actually, while the number of fatalities a conflict produces seems to be foremost dependent on relative capabilities, including factors such as government and rebel control over territory (Sambanis 2001a, 9; de la Calle and Sanchez-Cuenca 2012, 584), the determinants of conflict outbreak and conflict escalation are not necessarily the same. Relying on a too high battle-deaths threshold to measure civil war can lead to misleading results to the extent that such indicators conflate conflict outbreak and escalation into a single measure. For example, two authoritative studies on the causes of civil war (Fearon and Laitin 2003, and Collier and Hoeffler 2004) recently made a strong case against grievance-based approaches, and in favor of opportunity-based explanations: it is perhaps not entirely coincidental that both studies used a measure of civil war based on a 1,000 battle-deaths threshold. Thus, assessing the impact of grievances on the outbreak of intrastate armed conflict perhaps requires using a more inclusive measure of conflicts.

There are, yet, some reasons that speak against the use of a low battle-deaths threshold. One of these reasons is underreporting bias: put bluntly, an armed conflict resulting in 25

fatalities – or fewer<sup>80</sup> – having taken place in Germany in 2003 is more likely to appear on the UCDP/PRIO war list than a similar conflict in North Korea in 1962. While full-scale civil wars may not go unnoticed in either of these countries, smaller-scale episodes of violence might end up overrepresented in rich, democratic countries, and underrepresented in poor, nondemocratic countries.

One second potential pitfall is the inclusion of unrelated events in civil war datasets. Consider, for instance, the UCDP/PRIO's operational definition of armed conflict:

An armed conflict is defined by the Uppsala Conflict Data Project as a contested incompatibility that concerns government or territory or both where the use of armed force between two parties results in at least 25 battle-related deaths. Of these two parties, at least one is the government of a state (Gleditsch et al. 2002, 618-9).

This very concise definition nevertheless stresses several key features of domestic conflict. First and most obviously, the object of the incompatibility (government or territory) is political, which distinguishes internal conflict from organized crime or other forms of collective violence. Second, “parties” are not loose groups but relatively structured organizations: in practice, this means that rebel organizations must have names and be able to articulate clear demands for episodes of internal violence to be qualified as intrastate armed conflicts (see Strand et al. 2004), so that these can be set apart from spontaneous riots. Third, in order to differentiate civil wars from communal violence, the government must be directly involved in the conflict.<sup>81</sup> Finally, the requirement that both the government and rebel organizations make use of armed force<sup>82</sup> and are able to mount effective resistance makes it possible to differentiate civil conflicts from one-sided violence such as genocide or politicide (see Gleditsch et al. 2002, Strand 2004). Thus, this seemingly short operational definition actually succeeds in discriminating between

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<sup>80</sup> Some authors use a lower fatalities threshold. For example, de la Calle and Sánchez-Cuenca (2012) include in their analysis all armed organizations that killed at least 10 people if they were active during more than a year.

<sup>81</sup> There is a priori no clear reason why conflicts between different non-state actors should not be counted as intrastate armed conflicts in general. Yet, the decision to exclude these conflicts has some positive consequences for the empirical analysis I will carry out in the next sections, since I am primarily interested in conflicts between government and opposition groups.

<sup>82</sup> This criterion is somewhat loosened by the fact that “arms” do not need to be manufactured weapons, i.e., sticks and stones qualify as arms (see Strand et al. 2004, 3).

numerous forms of collective violence.<sup>83</sup>

There are yet some forms of political violence that are theoretically distinct from civil wars but nevertheless enter the UCDP/PRIO dataset because of the 25 battle-deaths threshold. For example, using a low threshold makes it difficult to distinguish civil war from domestic terrorism: as Sambanis (2008, 181) notes, the Irish Republican Army is alternatively coded as a terrorist organization or as a rebel movement engaged in a separatist war, depending on the authors and coding rules (recall that the Irish conflict does not appear in the COW war list). However, failing to distinguish between civil conflict and domestic terrorism might not be as problematic as it seems, to the extent that both phenomena often have the same underlying causes: in both cases, the use of violence is motivated by a desire to achieve national independence, change the government, or affect policies. Thus, terrorism and civil war do not differ in their causes but rather in their manifestation: after stating that what foremost distinguishes terrorism from civil war is power imbalance between the two parties, a lower level of active public support for the insurgents, and a lower level of violence (p. 181), Sambanis (2008, 183) deems terrorism as one of the many violent strategies that can be used – or not – during civil war. In turn, the intensity of violence and the way it is used seems to depend foremost on relative capabilities, i.e. the extent to which rebel organizations are able to control territory, recruit new members, and provide them with weapons: in a recent study, de la Calle and Sánchez-Cuenca (2012) find that state capacities predict well whether a given insurgency will take the form of a terrorist campaign (in which insurgents typically do not control territory and are forced to operate underground) or a civil war with rebel control on significant portions of the national territory; territorial insurgencies – which are the most lethal ones – are concentrated in weak states, while nonterritorial ones occur more frequently in countries with intermediate levels of development.<sup>84</sup>

A second – and by far more problematic – issue concerns the inclusion of coups d'état in

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<sup>83</sup> Of course, differentiating civil wars from state-sponsored violence, communal violence or criminal activity can get tricky in some borderline cases: Some rebel organizations might resort to criminal activity to finance themselves, so that they become indistinguishable from criminal networks on the long run; some governments might fight by proxy using militias, so that the war is primarily fought by non-state actors; and in some cases of state-sponsored violence, the targeted group is able to inflict some casualties on the stronger party, which makes some instances of genocide or politicide indistinguishable from civil wars (for a discussion, see Sambanis 2004).

<sup>84</sup> Interestingly, they note (p. 582) that territorial control does not depend on whether the conflict concerns territory or government.

civil war datasets, since coups fit into most operational definitions of civil war: they are contested incompatibilities regarding government, they involve the use of force among two organized parties – one of which is the state’s government – and they can, in some instances, result in 25 battle-deaths. This problem is arguably less acute in the case of the COW dataset – because few coups claim 1,000 fatalities – and in the case of separatist conflicts. Yet, a significant number of the events that appear in the UCDP/PRIO list of conflicts over government might be, in fact, military coups: Powell and Thyne (2011, 256) state that they have identified no less than 38 events in this dataset that could actually be best described as coups.

Why is it so problematic? First, because most theories that link political regimes and political violence are based on the assumption that civil wars are initiated by marginalized opposition groups.<sup>85</sup> By contrast, coups are typically assumed to be initiated by a faction of the political elite, generally with the active support of the military: for example, one of the most comprehensive datasets on coups (Powell and Thyne 2011) explicitly requires a seizure of the executive power to be perpetrated by regime insiders to be classified as a coup. Second, it is also a widely accepted premise that civil wars require the active participation of a substantial part of the public (see, e.g., Chacon, Robinson and Torvik 2011), while this is not necessarily the case of military coups. In a nutshell, coups and civil conflicts occur for different reasons, and their success is conditioned by different structural factors. Thus, conflating both phenomena into a single measure can result in two undesirable consequences. First, it could mask important statistical relationships, to the extent that countries that experience many civil wars but few coups appear to be similar to those that experience few civil wars but many coups. Second, this could lead to incorrect interpretations of the causal link between the independent variable of interest and the outbreak of domestic armed conflict.

This problem of both Type I and Type II errors is especially acute in the case of empirical analyses linking political regime type to intrastate armed conflict – specifically, when the theory to be tested aims for the explanation of conflicts initiated by opposition groups

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<sup>85</sup> While this assumption is most often implicit, the point pertains to virtually all cross-national quantitative studies on political regime type and civil war: studies on democracy and intrastate armed conflict typically adopt a grievance-based approach (see, e.g., Hegre et al. 2001; Reynal-Querol 2002a, 2002b, 2005; Lacina 2006; Regan and Bell 2010), as do recent studies on authoritarian regime type and domestic conflict (Gurses and Mason 2010; Fjelde 2010; Wilson 2014).

against the regime apparatus, and does not concern intra-elite conflicts. With regard to type I errors, while the influence of political structures on regimes' vulnerability to elite splits is now well-documented (see, e.g., Geddes 1999; Magaloni 2006, 2008; Svobik 2012), their impact on intrastate armed conflict remains controversial (see, e.g., Collier and Hoeffler 2004; Fearon and Laitin 2003). With regard to type II errors, many theories are based on the assumption that no single institutional arrangement allows political leaders to simultaneously counter threats from their elite (i.e., avoid coups) and successfully manage challenges from mobilized opposition groups (i.e., avoid revolutionary conflicts), and that there seems to be a trade-off between immunity to coups or elite splits and immunity to popular uprisings. For example, Acemoglu and Robinson (2006) argue that democracies are more likely to be challenged by elites and dictatorships from "below." With regard to dictatorships, O'Donnell and Schmitter (1986) argue in their prominent work on democratization that opposition mobilization is more likely to result in a peaceful transition when an authoritarian regime loses its cohesiveness. "Conversely, no transition can be forced purely by opponents against a regime which maintains the cohesion, capacity, and disposition to apply repression. Perpetuation in power or armed military struggle become the only likely outcomes of such cases" (O'Donnell and Schmitter 1986, 21). Svobik (2012) argues that dictators who rely extensively on the armed forces may be able to successfully avoid opposition mobilization but run the risk that the military – endowed with resources and privileges that make it able to overthrow the government – turns against the regime itself. The point is made most explicit in Roessler's (2011) analysis of African personalist regimes: by choosing to exclude powerful groups from the central government in order to avoid being overthrown, dictators simply choose to trade coup risk for civil war.

If some regimes are able to resist challenges from the opposition while some others are better at avoiding elite splits, testing hypotheses pertaining to the effects of political institutions on civil conflict requires carefully distinguishing between conflicts initiated by the opposition from those resulting from splits within the state apparatus. For example, the recurring finding that democracy is not sufficient to prevent civil war could result from the fact that democracies do experience fewer large-scale conflicts, but are structurally more vulnerable to coups than authoritarian regimes (see, e.g., Pilster and Böhmelt 2012).

Conversely, the statistical association between authoritarian regime type and civil conflict onset uncovered by recent studies (e.g., Fjelde 2010; Wilson 2014) could be entirely driven by the inclusion of military coups in the UCDP/PRIO Armed Conflict dataset.

### **5.3. REEXAMINING THE IMPACT OF POLITICAL REGIME TYPE ON INTRASTATE ARMED CONFLICT AND MILITARY COUPS: SOME EXPECTATIONS**

As discussed in the previous section, some military coups result in enough casualties to be included in datasets of intrastate armed conflicts. Yet, theoretically speaking, coups and intrastate armed conflicts are distinct phenomena: while the grievance-based civil war literature typically assumes that armed conflicts are initiated by dissatisfied opposition groups, coups are initiated either by regime insiders supported by some factions of the armed forces, or by the military itself – usually as a consequence of splits at the elite level (Powell and Thyne 2011): Thus, coups are essentially conflicts taking place between groups within the regime apparatus, which clearly sets them apart from intrastate armed conflicts.

Given that 1) coups and armed revolts are fundamentally different processes, and 2) extant measures of civil war possibly conflate both phenomena, it becomes necessary to reexamine the relationship between political regimes and intrastate armed conflicts. The remainder of this chapter will thus directly deal with a crucial question: are Fjelde's (2010) – and, by extension, Wilson's (2014) – findings regarding authoritarian regime type and civil war entirely driven by the inclusion of coups in intrastate conflicts datasets?

This question, in turn, implies that authoritarian regime type can be expected to impact the probability of a military coup. Therefore, as a first step, this section will present some theoretical expectations about political regimes' vulnerability to civil conflicts and military coups, respectively: the main point is that there are, indeed, reasons to believe that authoritarian regime type affects coup risk. More specifically, the argument presented here is that the distinction between democracy and nondemocracy reflects how political power is allocated between incumbents and opposition groups, while the distinction between authoritarian subtypes reflects how political power is shared between different factions of

the ruling elite. Thus, the former distinction is relevant to the explanation of conflicts between elites and citizens, whereas the latter is more useful to predict the occurrence of intra-elite conflicts. In more concrete terms, I expect democracies to experience fewer civil wars than dictatorships. I also expect substantial variations between authoritarian subtypes with regard to military coups, but not with regard to civil wars.

### **5.3.1. Political causes of civil war**

As discussed in Chapter 3, the minimalist approach to democracy (Przeworski 1999) holds that people submitted to an unwanted ruler tend to revolt only if there are no other ways to get rid of him: in democracies, institutional mechanisms – competitive elections – that allow for regular alternations in power incite dissatisfied groups to wait rather than to revolt. Furthermore, the fact that parties that lost the election can expect to win in the future round lessens the sense of permanent exclusion that ultimately leads groups to rebel. In short, democracies should experience fewer intrastate conflicts than nondemocracies because “only democracy provides an institutional framework that permits reform without violence” (Popper 1963, 4).

The fact that democracy decreases the likelihood of civil war is not well established in the quantitative civil war literature (for a review, see Hegre 2014). Yet, previous studies have mostly been challenged on a methodological (Vreeland 2008) rather than theoretical ground, and issues of data availability and quality could be part of the reasons why the empirical evidence is mixed: as noted above, most of the tests of the “domestic democratic peace” have been performed with the Polity index, which is now increasingly viewed as unsuitable to analyze the relationship between political regime type and the outbreak of intrastate armed conflict (see Section 5.1.). The specificity of the minimalist approach to democracy calls for more direct tests of this hypothesis.

I thus expect democracies to experience fewer domestic conflicts than nondemocracies. By contrast, I do not expect to uncover any significant difference between authoritarian regime types with regard to their vulnerability to armed conflict. One of the key findings of past research on authoritarian regimes is that those are more likely to break down as the result of splits within the ruling elite, and that the overwhelming majority of dictators who

lost power through irregular means were removed by government insiders or members of the armed forces (Svolik 2012; Kendall-Taylor and Frantz 2014). In short, popular uprisings are not the major threat to authoritarian rulers. Rather, it is the internal balance of power between factions of the elite that determines the stability of nondemocratic regimes and the fate of authoritarian rulers. Accordingly, most contemporary approaches to authoritarian regimes focus on how power is shared between regime insiders (Geddes 1999; Haber 2006; Gandhi 2008a; Magaloni 2008; Cheibub et al. 2010; Svolik 2012). Elites being the main threat to autocrats and the primary target of authoritarian power-sharing agreements, differences between authoritarian subtypes – or “qualitative” differences, as Hadenius and Teorell (2007a) put it – do not concern the mass public. Power-sharing arrangements between factions of the ruling elite are of little relevance to the voters, so if it is true that civil wars are initiated by dissatisfied citizens or marginalized opposition groups, variations in dictatorships’ proneness to civil conflict should be primarily explained by their degree of inclusiveness (or “democracy level”). In other words, there are few reasons to expect a politically insulated monarchy to behave differently from a politically insulated military dictatorship.

To sum up, I expect democracies to experience fewer intrastate armed conflicts than dictatorships, but I do not expect dictatorships to vary systematically in their vulnerability to armed conflict. I thus test the following hypotheses:

*H1: Democracies run a lower risk of intrastate armed conflict than autocracies.*

*H2: Authoritarian regime type does not impact the risk of intrastate conflict.*

### **5.3.2. Political causes of military coups**

The Chilean military dictatorship was in some respects a rather unconventional case. Contrary to most armed forces with a strong legacy of intervention in domestic politics, the Chilean military was comparatively cohesive and professionalized, and the four members of the junta rapidly agreed on a set of formal decision-making procedures upon the 1973 coup (Barros 2002). As a consequence, with its seventeen years of existence, the regime

lasted much longer than the average military dictatorship.<sup>86</sup> Yet, the junta almost imploded as one of its four key members, air force commander Gustavo Leigh, was forcefully removed from office in 1978. A major crisis ensued, as eighteen other air force generals resigned rather than accept to replace him.<sup>87</sup> The crisis originated in Leigh's objections to Pinochet's attempts to consolidate personal rule, and escalated as he vocally opposed the 1977 referendum called by Pinochet to seek public backing against the United Nation's condemnation of his human rights record, and the 1978 plebiscite that allowed Pinochet to proclaim himself President. In 1978, he even told the Italian newspaper *Corriere della Sera* that he was in favor of returning Chile to democratic rule, and added that "ideas cannot be abolished through decree laws" and that Chile should authorize left-wing parties "in the same way the Swedes do."<sup>88</sup> And yet, Leigh was by no means a "soft-liner:" following the 1973 coup, he famously stated that the junta's objective was to eradicate the "Marxist cancer"<sup>89</sup> from the country. He nonetheless disagreed with Pinochet about the very objectives of the military government: in one of his last television interviews, he stated that he was in favor of "putting the house in order, sweeping up, giving it a new coat of paint and handing it back over, calling elections and going home calmly with a sense of job accomplished. Not carrying on indefinitely."<sup>90</sup>

The Chilean case provides an interesting example of the divisions that can arise from interests conflicts between military leaders, and reveals, if anything, that military dictatorships are inherently brittle. This instability has been highlighted by many authors (e.g., Finer 2009 [1962]; Nordlinger 1977) and there is some empirical evidence that they tend to experience more coups than civilian-led regimes (Belkin and Schofer 2003; Powell 2012). Military regimes display several peculiarities that may explain this instability. First, leaders who came to power through extraconstitutional means – such as the vast majority of military leaders – are much more likely to lose power in an irregular way (Londregan and Poole 1990; Goemans et al. 2009a). A reason why this is the case is that the first coup

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<sup>86</sup> The average duration of post-World War II military dictatorships is estimated between five (Nordlinger 1977) and nine years (Geddes et al. 2014).

<sup>87</sup> Gunson, Phil. 1999. "Gustavo Leigh." *Guardian*, October 2, 1999, available at <http://www.theguardian.com/news/1999/oct/02/guardianobituaries1> (accessed April 30, 2015).

<sup>88</sup> Ibid. For an alternative view, see Barros (2002).

<sup>89</sup> Ibid.

<sup>90</sup> Honan, William H. 1999. "Gustavo Leigh Guzman, 79, who served in Chilean military junta." *New York Times*, October 1, 1999, available at <http://www.nytimes.com/1999/10/01/world/gustavo-leigh-guzman-79-who-served-in-chilean-junta.html> (accessed April 30, 2015).

sets a precedent and breaks the fragile equilibrium of political control on the military (see Chapter 3): for example, Belkin and Schofer (2003, 608) argue that “coups have a powerful symbolic impact by legitimizing extraconstitutional methods as acceptable mechanisms for political transitions.” Relatedly, Nordlinger (1977, 40) suggests that, while officers might be reluctant to overthrow a (legitimate) civilian regime, they might have fewer qualms about toppling a rival faction that seized power illegally. While discussing the dynamics of coups and countercoups in military dictatorships, Geddes states that in these regimes, coups “are primarily a way of changing leadership while maintaining the regime itself” (Geddes 1999, 135).

Second, as the Chilean example above highlights, the military – contrary to political parties – lacks a coherent ideology that could help maintain the cohesion of elites and prevent policy disagreements. Even if leading officers are united around some common goals such as national security, they typically disagree about how exactly these goals are to be achieved: as Nordlinger (1977, 145) notes, most coup plotters have “only vague ideas about their governing objectives and the means for attaining them.” While these disagreements are not necessarily conducive to factionalization as long as the military remains focused on national security tasks, they are likely to lead to serious conflicts if one faction of the military assumes power and begins to implement policies other officers dislike.

Third and most importantly, even coup leaders do not necessarily agree about how long they plan to stay in office before surrendering power or which conditions are appropriate for withdrawing (see, for example, O’Donnell and Schmitter [1986] on the divisions between “hardliners” and “softliners” in authoritarian elites coalitions). While many interventionist officers conceive their role as fundamentally temporary (Finer 2009 [1962]; Nordlinger 1977), some of them might either see it otherwise or simply find themselves enjoying political power after some time spent in office. These fundamental conflicts of interest are further aggravated by the fact that most officers except for the highest regime officials do not face a severe risk of prosecution after a regime transition, and that, while the survival of ruling parties after a regime change is dependent on their level of popular support, the military is likely to retain at least some of its influence and bargaining power (Geddes 1999; for some qualifications, see Debs 2010): as the regime becomes challenged,

while the highest ranking officials face an incentive to cling to power, the bulk of the officers might find a withdrawal a more attractive option.

Thus, we should expect military dictatorships to be, on average, particularly prone to intra-elite splits. But what about other authoritarian regimes? A quick glance at the average duration of authoritarian regimes provides some clues: monarchies and party regimes appear to be comparatively durable (see Geddes 1999; Hadenius and Teorell 2007a; Magaloni 2008). Since authoritarian regimes break down at different rates and coups are one of the main causes of dictatorships' demise, this already indicates that the frequency of coups should vary from one regime type to another. There are, moreover, some theoretical reasons to expect monarchies and party regimes to be relatively immune to coups. As discussed in Chapter 3, the institution of hereditary succession in monarchies is a powerful tool to prevent intra-elite splits. In turn, the stability of intra-elite relationships should deter the military from attempting to seize power: as prior research on civil-military relations has highlighted, the military seldom unilaterally intervenes unless the regime displays severe signs of internal instability (e.g., Finer 2009 [1962]).

These arguments, to a certain extent, apply to party-based regimes as well, since prior research has consistently highlighted both their relative stability<sup>91</sup> and their ability to maintain elite cohesion and avoid intra-elite splits (Geddes 1999; Brownlee 2005; Magaloni 2008). There are yet other reasons why we should expect party regimes to be less prone to coups than other types of dictatorships. One of these reasons has been discussed in Chapter 4: the persistence of external threats limits the military's ability to get involved in domestic affairs, enhances the cohesion of elites, and thus lessens the risk of conflicts between civilian rulers and armed forces (see Desch 2001; Piplani and Talmadge 2015; McMahon and Slantchev 2015). Another reason is that single-party regimes have by definition succeeded in either incorporating or eliminating organized opposition, which deprives potential coup plotters from any civilian support in case they manage to attain power.

A final reason why party regimes can be expected to experience fewer military coups is

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<sup>91</sup> This is not tautological. The fact that party regimes last longer could be explained by the fact that they are able to successfully deal with attempted coups (i.e., that coups are more likely to fail in party regimes than in other dictatorships). My argument, by contrast, pertains to all attempted coups, including failed ones: The idea is that, regardless of their odds of success, coups are attempted less frequently in these regimes.

given by Geddes (2006): the existence of a mass party makes it easier for the incumbent to mobilize support in case a coup is attempted. Given that the military is generally reluctant to intervene in the face of mass protest, anticipating large demonstrations in support of the dictator can deter potential coup plotters. Geddes is careful to note that this is not because parties genuinely increase popular support, but because they facilitate the organization and coordination of opposition and the monitoring of participants by party members (Geddes 2006, 4).

To sum up, I expect authoritarian regimes to vary systematically with regard to their vulnerability to coups, military dictatorships displaying the highest risk, and monarchies and party regimes the lowest risk of coup. I expect civilian autocracies to fall somewhere in the middle, and to experience fewer coups than military regimes but more than monarchies and party regimes.

*H3: Military dictatorships run a higher coup risk than other regimes.*

*H4: Monarchies and party regimes run a lower coup risk than other regimes.*

What about democracies? Empirical studies analyzing the frequency of coups across regime types remain rare, and mostly come to contradictory results (see e.g. Powell, 2012; Lehoucq and Pérez-Liñán, 2014; Thyne and Powell 2015). On the one hand, there are good reasons to expect democracies to be more vulnerable to coups than at least some types of autocracies. First, autocracies' higher repressive capacities (Davenport 2007a) make them able to raise the costs of coups: for a failed coup, conspirators might expect a harsher punishment in autocracies than in democracies, which makes the latter an easier prey for coup plotters. Second, democracies are less likely than autocracies to divide and weaken the military to avoid coups, because coup-proofing tends to make the military ineffective and military defeats have a direct potential impact on the probability of an elected leader being removed from office (Pilster and Böhmelt 2011; 2012).

On the other hand, the fact that in democracies, access to and alternation in power occur through regular procedures should reduce instability at the elite level and thereby decrease officers' incentives to get involved in domestic politics (see Chapter 3). Thus, although democracies are somewhat limited in their capacity to implement coup-proofing measures,

I still expect them to run a lower risk of coup than non-institutionalized regimes.

*H5: Democracies run a lower coup risk than non-institutionalized regimes.*

## **5.4. EMPIRICAL ANALYSIS**

### **5.4.1. The data**

#### **5.4.1.1. Dependent variable**

The dependent variable is a categorical variable that takes on the value of 1 in the years an internal conflict breaks out (subsequent conflict years are coded 0), 2 if there has been at least one attempted or successful military coup in the country-year, and 0 otherwise. Data on military coups are drawn from Powell and Thyne's (2011) dataset. Coups are defined as "illegal and overt attempts by the military or other elites within the state apparatus to unseat the sitting executive" (Powell and Thyne 2011, 252). I do not distinguish between successful and failed coups, and I collapse country-years with multiple coups into a single value.

With regard to intrastate armed conflicts, I use data from the UCDP/PRIO Armed Conflict Dataset (Gleditsch et al. 2002; Harbom and Wallensteen 2010), that defines armed conflict as a "contested incompatibility that concerns government or territory or both where the use of armed force between two parties results in at least 25 battle-related deaths" and in which one of the two parties is the government of a state (Gleditsch et al. 2002, 618-619). I specify the variable as follows:

(1) I use only data on purely intrastate or internationalized internal conflicts. I thus exclude interstate as well as extrasystemic conflicts. While some authors choose to include extrasystemic (i.e., anti-colonial) conflicts in their list of civil wars (see, e.g., Fearon and Laitin 2003), I see at least one good reason not to do so: since I am primarily concerned with the impact of regime type on intrastate conflict, including wars of decolonization would imply assigning the colonizers' political system to the

colonies; this would severely bias the analysis to the extent that colonized populations were virtually never granted the same political rights as the citizens of the metropole.

(2) I do not distinguish between conflicts over territory and conflicts over government: Both conflict types take on the value of 1 on the dependent variable.

(3) I do not distinguish between different intensity levels either. The UCDP/PRIO dataset makes it possible to distinguish between minor conflicts (that claim between 25 and 1,000 battle-deaths) and wars (that result in more than 1,000 battle-deaths), but I do not see any theoretical reason for treating these two types separately.

(4) The intermittency threshold (i.e., how many years of inactivity must have passed between two different observations of an active conflict before a new onset is coded) is set by two years. This reflects standard practice in the civil war literature (see, e.g., Fjelde 2010; Gleditsch and Ruggeri 2010).

(5) In the period from 1950 to 2005, there have been 248 outbreaks of civil conflict and 377 attempted or successful coups. In 52 cases, both a coup and a conflict onset were reported in the same country-year. Since it is unclear whether these observations are measurement errors (i.e., coups classified as intrastate armed conflicts) or whether some countries simply experienced both a domestic conflict and a military coup in the same year, I code these cases as missing.

#### 5.4.1.2. Independent variables

To measure political regime type, I use Cheibub et al.'s (2010) dataset (see Chapter 2). This measure has several advantages. First, the operational definition of democracy that underlies Cheibub et al.'s (2010) measure is directly related to the main theoretical argument regarding democracies and civil war – namely, that the mechanism linking democracy to civil war (if there is one) is the possibility of alternation through contested elections. Second, Cheibub et al.'s (2010) operational definition of military dictatorship also makes the measure suitable to test Hypothesis 2: regimes are coded as military dictatorships if they are not democratic and if the chief executive is or ever was a professional member of the military. In other words, their definition of military regimes is

based on the mere identity of the leader. Thus, since one common argument suggests that military dictatorships should not be particularly conflict-prone precisely because military leaders have the possibility to withdraw before public dissent escalates into civil war (see Geddes 1999; Debs 2010; Geddes et al. 2014), this definition is well-suited for the empirical analysis. Finally, contrary to some commonly used datasets – see, e.g., Hadenius and Teorell (2007b) – Cheibub et al. (2010) do not code rulers who came to power through insurgency as military, which avoids potential endogeneity issues.

The independent variables thus consist of five dummy variables measuring democracy, civilian autocracy, single-party regime, military dictatorship, and monarchy, respectively.

**Table 5.1. Summary statistics on regime type, coups and conflicts, 1950-2005**

	<i>Observations</i>	<i>Conflict outbreaks</i>	<i>Coups</i>
Military dictatorship	1,575	55	193
Monarchy	595	5	13
Civilian autocracy	1,549	59	49
Single-party regime	808	17	13
Democracy	2,807	56	53

(Sources: UCDP/PRIO Armed Conflict dataset; Cheibub et al. 2010; Powell and Thyne 2011)

#### 5.4.1.3. Control variables

In addition to the dependent and independent variables, I use several control variables which have been found to be robustly associated with the onset of civil war: I control for per capita income using data from the Maddison Project Database (Bolt and van Zanden 2013), and ethnic fractionalization using data from the Ethnic Power Relations dataset (Cederman, Wimmer and Min 2010). Since the UCDP/PRIO Armed Conflict Dataset requires an armed conflict to claim a minimal number of battle-deaths to be included in the data, I control for population (see Sambanis 2004): Data are taken from Ross' (2013) Oil and Gas dataset. Finally, I add a Cold War dummy which takes on the value of 1 from 1946 to 1990.

**Table 5.2. Summary statistics on control variables, 1950-2005**

	<i>Mean</i>	<i>Standard deviation</i>	<i>Observations</i>
(ln) GDP/capita	7.92	1.06	7,054
(ln) Population	15.93	1.53	7,256
ELF	0.41	0.31	6,858
Regime duration	16.39	13.97	7,334

(Sources: Maddison Project database; Cederman et al. 2010; Cheibub et al. 2010; Ross 2013)

#### 5.4.2. Results

To test my hypotheses, I use multinomial regression. This allows me to test whether each regime is more likely to be challenged from the inside or from below. Following Fjelde (2010) and Wilson (2014), I also control for regime duration, and add two variables that measure the number of years since the last occurrence of a conflict or a coup, respectively. I use robust standard errors clustered by country. All regime variables are lagged one year.

**Table 5.3. Regime type, civil conflict onset, and military coups, 1950-2005 (multinomial regression, reporting relative risk ratios)**

	(1) <i>Outcome: conflicts</i>	(2) <i>Outcome: coups</i>
Peace duration (conflicts) <sup>a</sup>	0.974*** (0.009)	1.000 (0.009)
Peace duration (coups) <sup>a</sup>	1.002 (0.011)	0.938*** (0.014)
GDP/capita <sup>a, b</sup>	0.872 (0.122)	0.783** (0.090)
Population <sup>a, b</sup>	1.438*** (0.091)	0.938 (0.054)
ELF	3.929*** (1.434)	1.725** (0.403)
Cold War	0.519*** (0.103)	1.653*** (0.302)
Democracy <sup>a</sup>	0.623* (0.158)	0.806 (0.162)
Civilian dictatorship <sup>a</sup>	0.659 (0.167)	0.601*** (0.113)
Party dictatorship <sup>a</sup>	0.645 (0.201)	0.300*** (0.08)
Monarchy <sup>a</sup>	0.428*** (0.139)	0.514** (0.141)
Regime duration <sup>a</sup>	0.992 (0.0134)	0.963*** (0.0127)
Constant	0.001*** (0.000)	1.765 (2.168)
Countries	150	150
Observations	6,464	6,464

Robust standard errors in parentheses.

\* p<0.10; \*\* p<0.05; \*\*\* p<0.01.

<sup>a</sup> Lagged one year; <sup>b</sup> Log

Table 5.3. displays the results. Model 1 shows the impact of regime type on the outbreak of intrastate armed conflict. In Model 2, military coups are the outcome. In both models, military dictatorships – that display, according to Fjelde (2010), the highest risk of civil conflict – are the reference category. Robust standard errors are reported in parentheses.

Model 1 reveals few clear tendencies regarding dictatorships' vulnerability to armed conflict: as expected (Hypothesis 2), most of the authoritarian regime dummies do not differ significantly from the baseline with regard to their risk of intrastate armed conflict. Importantly, the model indicates that military dictatorships do not run a higher risk of conflict than one-party regimes. In fact, the only regimes that run a significantly lesser risk of conflict than military dictatorships are democracies (which lends support to Hypothesis 1) and, somewhat more surprisingly, monarchies. While democracy is found to decrease the risk of conflict by approximately one third – the coefficient being only marginally significant – monarchies are less than half as likely to experience civil war as military dictatorships. The effect is strongly significant.

As expected, military regimes experience more coups than all of the other regimes (Model 2): all relative risk ratios associated with authoritarian regime dummies are below 1. The effect is statistically significant in the cases of monarchies, civilian dictatorships, and one-party regimes, the latter having the strongest effect: the estimated probability of experiencing a coup is more than three times as high in military dictatorships as in party regimes. Thus, even though single-party regimes neither increase nor decrease the risk of civil war compared to military dictatorships, they still display peaceful attributes: they appear to run the lowest risk of coup among all regime types. Civilian dictatorships and monarchies also experience significantly fewer coups than military regimes, both regimes decreasing coup risk by about 40% and roughly 50%, respectively.

Finally, even though the coefficient associated with democracies indicates a lower vulnerability to coups, the difference is not statistically significant. Additional tests with alternative binary democracy measures (Boix et al. 2013) further indicate that the democracy-dictatorship dichotomy is not relevant to predict the occurrence of coups: Hypothesis 5 is not supported.

Some interesting tendencies regarding control variables can be noted. GDP per capita has a

negative influence of both conflict risk and coup risk, but is only significant in the latter case. Conversely, population size only impacts (positively) the risk of a conflict onset. The duration variables that measure the number of years since the last occurrence of a coup and the last occurrence of a conflict impact negatively the probability of a coup and the probability of conflict, respectively. Finally, the effect of the Cold War dummy (significant in both models) is positive in the case of coups but negative in the case of conflicts.

To sum up, these findings lend some preliminary support to Hypotheses 2, 3, and 4: among all regime types included in the analysis, one-party regimes run the lowest risk, and military regimes the highest risk of coup d'état; yet, both regimes do not differ from each other with regard to their vulnerability to civil war. Overall, the type of authoritarian regimes does not seem to make any difference for the risk of intrastate armed conflict, with the notable exception of monarchies. Finally, the results confirm Hypothesis 1, but Hypothesis 5 is rejected: when compared to military dictatorships, democracies are associated with a lower risk of civil war, but are not immune to coups.

On the whole, the results sketched above are at odds with those of Fjelde's (2010) and Wilson's (2014) studies. But is this difference in findings due to the difference of datasets? Fjelde performs her tests using two different datasets, namely Hadenius and Teorell's (2007b), and Wright's (2008) update of Geddes' (1999) classification of authoritarian regimes. In his replication of Fjelde's study, Wilson (2014) obtains similar results with Wright's (2008) and Hadenius and Teorell's (2007b) datasets, but the regime dummies from Cheibub et al.'s (2010) dataset fail to attain statistical significance. Differences in coding could thus explain why I cannot replicate Fjelde's (2010) results: although military regimes are coded in a broadly similar way in these three datasets,<sup>92</sup> the measure I built for one-party regimes (that relies exclusively on the number of existing parties) is a somewhat crude measure of the concept, and only loosely related to the potential for co-optation or to the constraints on the autocrat's power (which are the main causal mechanisms linking one-party regimes to domestic conflict in Fjelde's analysis). Since Geddes et al. (2012) directly code party regimes, their data should allow me to test my hypotheses in a more direct way. I thus check whether my findings hold with an alternative measurement of

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<sup>92</sup> In spite of broadly similar operational definitions, there are still non-negligible divergences between Geddes et al.'s (2012) and Cheibub et al.'s (2010) coding of military dictatorships (see Croissant 2013b).

authoritarian regime types: Geddes et al.'s (2012) dataset distinguishes between monarchies, military dictatorships, personalist autocracies, and party regimes. I use the dummy variables from the dataset without modification.

**Table 5.4. Regime types according to Geddes et al. (2012)**

<i>Regimes</i>	<i>Observations</i>
Military dictatorships	554
Party regimes	2,073
Personalist autocracies	1,011
Monarchies	499

(Source: Geddes et al. 2012)

Note that, because the sample is restricted to nondemocracies, the number of observations drops to 3,852. The model specification and control variables remain the same (except for the regime duration variable, built on the basis of Geddes et al.'s classification). I use military dictatorships as the reference category. Table 5.5. displays the results.

Models 3 and 4 report the results for intrastate conflicts and coups, respectively. The different measurement of political regime type leads to some minor changes in the results, but overall, findings remain the same: autocratic subtypes do not have any effect on the probability of domestic conflict (Model 3). However, regime type has a strong effect on coup risk (Model 4): All relative risk ratios associated with regime types differ significantly from the baseline. Military dictatorships therefore run the highest risk coup risk, whereas party regimes and monarchies (displaying the strongest coefficients) are the least likely to experience a coup: both regimes are only about 25% as likely to experience a coup as military dictatorships.

The main difference between Models 1 and 2 – based on Cheibub et al.'s (2010) measure – and Models 3 and 4 – based on Geddes et al.'s (2012) measure – concerns monarchies. In Model 1, monarchies appeared to be less likely to experience civil war. This result is not confirmed in Model 3: although monarchy has a relatively large negative effect on the dependent variable, this effect is not statistically significant. By contrast, the effect of monarchy on coups is much higher in Model 4 than in Model 2. Moreover, monarchies emerge as the most stable regime type in Model 4, and even outperform party regimes. In

spite of these differences, the main findings remain the same: there is no variation in non-institutionalized regimes' probability of experiencing civil war.

**Table 5.5. Regime type, coups, and intrastate armed conflicts, 1950-2005 (multinomial regression, reporting relative risk ratios)**

	(3) <i>Outcome: conflicts</i>	(4) <i>Outcome: coups</i>
Peace duration (conflicts) <sup>a</sup>	0.981 (0.017)	0.990 (0.011)
Peace duration (coups) <sup>a</sup>	1.009 (0.019)	0.929*** (0.016)
GDP/capita <sup>a, b</sup>	0.914 (0.155)	0.901 (0.106)
Population <sup>a, b</sup>	1.410*** (0.134)	0.830*** (0.058)
ELF	3.134*** (1.314)	1.380 (0.383)
Cold War	0.405*** (0.097)	1.661** (0.345)
Monarchy <sup>a</sup>	0.518 (0.331)	0.241*** (0.066)
Personalist dictatorship <sup>a</sup>	0.579 (0.333)	0.603** (0.126)
Party dictatorship <sup>a</sup>	0.567 (0.298)	0.261*** (0.059)
Regime duration <sup>a</sup>	0.982 (0.024)	1.014 (0.011)
Constant	0.001*** (0.001)	5.588 (8.199)
Countries	115	115
Observations	3,852	3,852

Robust standard errors in parentheses.

\* p<0.10; \*\* p<0.05; \*\*\* p<0.01.

<sup>a</sup> Lagged one year; <sup>b</sup> Log

## 5.5. A CLOSER LOOK AT POLITICAL REGIMES AND CIVIL WAR: DEMOCRACIES AND MONARCHIES IN THE 19<sup>th</sup> CENTURY

The results discussed above point to two interesting tendencies regarding regime types and civil war. One of these results – the negative effect of democracy on civil war – is theoretically consistent; the second one – the fact that monarchy decreases the risk of civil war as well – is, by contrast, rather surprising. Albeit unexpected, this latter result is not a mere anomaly: Gurses and Mason (2010) obtain a broadly similar result.<sup>93</sup> But this finding

<sup>93</sup> More specifically, using Geddes' (1999) data, they find that monarchies and party regimes decrease the risk of civil war compared to personalist autocracies, while other regime types (e.g. military

is still surprising, given the restrictions placed on political participation in those regimes and the fact that there is absolutely no constitutional way of removing an unpopular monarch from power. Actually, given the relative rarity of both monarchies and civil wars, this finding might be a statistical artefact: monarchies represent only roughly 8% of all observations for the period from 1946 to 2008. Moreover, the share of monarchies steadily decreases over time, representing about 7% of the sample for the period from 1975 to 2008, and about 6% for the post-Cold War period. Finally, the distribution of monarchies displays a strong regional variation: apart from ceremonial monarchies that are de facto ruled by an elected official, monarchies are inexistent in Europe, America and Oceania, and virtually absent from Sub-Saharan Africa – with the notable exceptions of Swaziland, Burundi and Ethiopia, the two latter of which are not exactly known for their peacefulness. Most of contemporary monarchies are instead concentrated in oil-exporting countries of the Middle East. Accordingly, the average per capita income of monarchies diverges strongly from the one of other authoritarian regimes, as can be seen in Table 5.6.

**Table 5.6. Per capita income in monarchies and non-monarchic dictatorships, 1946-2008**

	<i>Mean income</i>	<i>Std. deviation</i>	<i>Max. income</i>	<i>Min. income</i>
Monarchies	6,184.42	6,982.66	42,916	390
Non-monarchic regimes	2,393.08	2,453.84	28,138	207

(Sources: Maddison Project database; Cheibub et al. 2010)

On average, monarchies are roughly 2.5 times as rich as non-monarchic dictatorships. Note that the mean per capita income of monarchies increases substantially if the two poorest monarchies (which are, unsurprisingly, Burundi and Ethiopia) are removed from the sample. Thus, given the small number of monarchies and their similarities, it appears difficult to distinguish the specific effects of monarchy from those of its covariates (e.g. income, rentierism, or other geographic, cultural or religious factors). To further complicate matters, regime type is not independent from regime tenure: Hadenius and

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dictatorships) do not differ significantly from personalist autocracies.

Teorell (2007a) find that monarchies – with an average duration of 25 years – display the longest life span among authoritarian regimes. This might further bias analyses since most regimes are more prone to civil war within the period that immediately follows political transitions (see, e.g., Hegre et al. 2001; Regan and Bell 2010).

All in all, these statistical patterns cast doubt on the validity of any empirical association between monarchy and civil war. Even if this relationship exists independently from all correlates of monarchy, this would still not exclude possible reverse causation issues: the causal mechanism underlying the statistical association between monarchy and civil war might not be that monarchy “causes” peace (or displays institutional features that make it inherently immune to civil war), but rather that countries that managed to avoid civil conflict (for reasons unrelated to regime type) are on the whole less likely to have undergone a regime transition than countries that experienced internal turmoil. This explanation is not implausible if we keep in mind that most of the regimes prior to the 20<sup>th</sup> century were monarchies (which were later replaced by other regime types), and that civil wars do not happen randomly (i.e., that some countries display characteristics that make them prone to civil conflict, while some others never experience internal turmoil). With these basic facts in mind, if we assume that regimes that repeatedly experience armed conflict are on the long run more likely to transition than those that remain unchallenged, and that monarchies are less likely to emerge from a successful rebellion than other regime types, then the share of monarchies at war should mechanically decrease as time passes: history has gradually eliminated fragile monarchies, and the only ones that survived throughout the 20<sup>th</sup> century are by definition those in which civil war, for some reason, never occurs. This scenario provides an explanation for why there are so few monarchies left at the beginning of the 21<sup>st</sup> century, and why the remaining ones seem to be relatively immune to intrastate armed conflict.

Deciding between both hypotheses (monarchies are immune to civil war vs. only peaceful countries managed to remain monarchies) requires testing these on a sample of regimes that includes not only old and rich monarchies, but also newly established or relatively poor monarchies. As discussed above, such cases are rare in the contemporary period, especially after 1975. However, this was not always the case: monarchies were one of the most widespread regime types prior to the 20<sup>th</sup> century. I thus reassess the relationship

between regime type and civil war using a dataset that covers the period from 1800 to 1913. To test whether monarchies really are immune to civil conflict, I develop my own dataset on monarchies. The dataset also includes a measure of democracy recently developed by Boix, Miller and Rosato (2013): This allows me to check whether findings regarding democracy's negative effects on civil war can be generalized.

I begin by summarizing the coding rules underlying the classification of regimes I use for the empirical analysis (Section 5.5.6.). Section 5.5.7. provides an overview of the data and methods and presents the main results.

### **5.5.1. Political regimes in the 19<sup>th</sup> century**

This section presents an overview on political regimes in the 19<sup>th</sup> century. The dataset covers the years from 1800 to 1913, and includes 71 independent countries. States are considered independent if they appear in the Correlates of War State System Membership list (2011).

The first step consists in distinguishing democracies from nondemocratic regimes. Since Cheibub et al.'s (2010) "Democracy and Dictatorship" dataset – on which the empirical analysis in the previous sections was based – only covers the post-World War II era, I use another recent democracy measure developed by Boix, Miller and Rosato (2013). Their dataset covers all independent countries from 1800 onwards: to date, this is the most comprehensive dichotomous measure of democracy. Boix et al. (2013) rely on a minimal, Schumpeterian definition of democracy, that is conceptually similar to Cheibub et al.'s (2010) definition. To qualify as democracy, a country must meet following requirements: (1) the executive must be directly or indirectly elected in popular, free and fair elections, and be responsible either directly to voters or to a legislature; (2) the legislature must be elected in free and fair elections; (3) a majority of adult men must be entitled to vote. Thus, the residual category of authoritarian regimes includes all regimes that fail to satisfy at least one of these necessary conditions.

Boix et al.' (2013) measure of democracy presents several advantages compared to other datasets (e.g., Polity IV) that cover a similar period. First, their measure is dichotomous and provides a clear-cut distinction between democracies and authoritarian regimes.

Second, contrary to the Polity data, Boix et al.'s (2013) measure does not include any indicator that is directly or indirectly related to the outbreak of internal political violence (see Vreeland 2008). Third, Boix et al. (2013) include a minimal suffrage requirement (which is absent from Polity IV) for a country to be classified as a democracy. Most currently available democracy datasets exclude this criterion for legitimate reasons: for example, Alvarez et al. (1996, 5) explicitly state that, for datasets that classify countries from 1946 onwards, suffrage is not an issue since it became universal in virtually all countries after WWII. However, omitting the participation criterion becomes problematic when it comes to identifying democracies in the 19<sup>th</sup> century, since universal suffrage was by no means granted. Finally, as already noted, Boix et al. (2013) and Cheibub et al. (2010) define and measure democracy in a broadly similar way, although there are some slight differences in the coding rules on which both measures rely:<sup>94</sup> This allows for a direct comparison of the results obtained for the two periods.

The second step consists in distinguishing monarchies from other nondemocratic regimes for the 1800-1913 period. To my knowledge, there is to date no publicly available, comprehensive dataset that provides such information for the entire period under consideration: I thus construct my own dataset on nondemocratic regimes in the 19<sup>th</sup> century.<sup>95</sup> The category of nondemocratic regimes comprises all regimes that receive a score of 0 in Boix et al.'s (2013) dataset, with the exception of those which have practically ceased to function due to foreign occupation or other factors: these cases are identified through the standardized authority scores from the Polity IV project<sup>96</sup> (Marshall et al. 2011) and coded as missing. Remaining regimes are then sorted into two categories: the first one includes monarchies, that is, regimes in which the effective ruler bears the title of “king” or equivalent *and* has attained power by rules of hereditary succession. The second category includes all regimes in which the head of state either bears the title of “president” or equivalent *or* has accessed power by other means than hereditary

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<sup>94</sup> The major difference – besides the participation criterion – is that the alternation rule is absent from Boix et al.'s (2013) measure. This involves a loss of transparency in the coding rules (it is not entirely clear how Boix et al. measure competition) but solves the problem of retroactive coding (see Ulfelder 2006).

<sup>95</sup> The coding is based on the Encyclopædia Britannica (various years) and Wikipedia. The latter is now widely accepted as a reliable source (see Eisner 2011; Kokkonen and Sundell 2014) and, to the extent that I have been able to cross-check, Wikipedia information is indeed accurate. Coding errors are my own.

<sup>96</sup> Only countries given a score of -66 (foreign interruption) are considered undergoing a period of effective interruption. Scores of -88 (transition) are not taken into account. To avoid endogeneity, I do not take scores of -77 (interregnum) into account either.

succession: for convenience purposes, these regimes are hereafter referred to as “republics.”

**Table 5.7.: Democracies in the world, 1800-1913 (by continent and date of transition)**

<b>Europe</b>	<b>North America</b>	<b>South America</b>
France, 1848-1851; 1870-1913 Switzerland, 1848-1913 Greece, 1864-1913 United Kingdom, 1885-1913 Luxembourg, 1890-1913 Belgium, 1894-1913 Netherlands, 1897-1913 Norway, 1900-1913 Denmark, 1901-1913 Portugal, 1911-1913 Sweden, 1911-1913	United States, 1800-1913 Canada, 1867-1913	Chile, 1909-1913 Cuba, 1909-1913 Argentina, 1912-1913
<b>Asia</b>	<b>Africa</b>	<b>Oceania</b>
	Orange Free States, 1854-1902	New Zealand, 1857-1913 Australia, 1901-1913

(Source: Boix, Miller and Rosato 2013)

In order to facilitate the comparison of results, I use the same definition of monarchy as Cheibub et al. (2010): monarchies are defined as regimes in which the legal, regular way of accessing political power is hereditary succession. By “regular way”, I mean that hereditary succession has to occur in accordance with custom or the constitution (de jure rule) and must take place effectively (de facto rule). This excludes regimes in which a leader chooses a family member as his successor without hereditary succession being a formal rule, as well as failed attempts by new leaders to establish a monarchy.

**Table 5.8. Monarchies in the world, 1800-1913 (by continent and year)**

<b>Europe</b>	<b>Africa</b>	<b>Asia</b>
Austria, 1800-1913 Bavaria, 1800-1871 Denmark, 1800-1900 Portugal, 1800-1806; 1820-1909 Prussia, 1800-1867 Spain, 1800-1807; 1814-1872; 1875-1913 Sweden, 1800-1911 United Kingdom, 1800-1884 Wuerttemberg, 1800-1871 Saxony, 1806-1871 Belgium, 1811-1893 France, 1814-1847 Modena, 1815-1860 Netherlands, 1815-1896 Parma, 1815-1860 Sardinia, 1815-1861 Tuscany, 1815-1860 Two Sicilies, 1816-1860 Baden, 1819-1870 Serbia, 1830-1913 Greece, 1832-1863 Italy, 1861-1913 Romania, 1866-1913 Hungary, 1867-1913 Luxembourg, 1867-1889 Germany, 1868-1913 Bulgaria, 1886-1913	Morocco, 1800-1912	Afghanistan, 1800-1913 China, 1800-1911 Iran, 1800-1913 Japan, 1800-1913 Korea, 1800-1910 Nepal, 1800-1913 Oman, 1800-1913 Russia, 1800-1913 Thailand, 1800-1913 Turkey, 1800-1913 Bhutan, 1907-1913
<b>North America</b>	<b>South America</b>	<b>Oceania</b>
	Brazil, 1824-1888	

Likewise, coding rules for identifying monarchies are similar to those underlying Cheibub et al.'s (2010) classification. To be classified as monarchies, countries have to meet two requirements: first, the ruler has to bear the title of “king” or equivalent;<sup>97</sup> second, de facto hereditary succession must have taken place at least once (i.e., the current leader has inherited power or is replaced by rules of hereditary succession). The first criterion refers

<sup>97</sup> E.g., “sultan”, “emperor”, “prince”, “shah”, or “grand duke”.

to the de jure rule, whereas the second one is intended for newly established regimes or newly independent countries. If the new leader succeeds in transmitting power to a family member, the predecessor and the successor are both coded as monarchs. If he fails, however, the regime is not considered a monarchy and is instead classified in the category of “others”. For example, French ruler Napoleon I bore the title of emperor, but did not have any hereditary predecessor or successor: he was not related to the previous royal family, he accessed power via a coup d’état, and his regime was abolished before his successor could effectively reign. His heir, Louis-Napoleon Bonaparte, took the throne as Napoleon III forty years later, in 1852. Yet, he did not access power by rules of hereditary succession either (he was instead elected by popular vote as president of the Second Republic) and the monarchy was abolished before he could pass power to his son. Therefore, none of these two rulers is coded as a monarch.

There are, however, two exceptions to this rule. First, periods of interruption are not taken into account: when a monarchic regime is abolished and subsequently restored, both periods are coded as monarchies as long as the last monarch of the first period and the first of the second period belong to the same family and at least the second one took the throne by rules of hereditary succession. Second, leaders without hereditary predecessor and successor are still considered monarchs if they are directly affiliated to another country’s royal family. For example, Otto I, the first monarch of the newly established Kingdom of Greece, was removed from the throne in 1863 without having managed to pass power to his designated successor. Yet, since he was the son of King Ludwig I of Bavaria, I code him as monarch.

### **5.5.2. Democracies, monarchies, and civil war: An additional test with new data**

I now move on to the empirical analysis. I still expect democracy to lower the risk of civil war (see Section 5.3.1). By contrast, I expect monarchies to run the highest risk of civil war: first, the restrictions placed on political participation deprive citizens of any peaceful way of making demands on the state; second, the monarch typically rules without constraints, except from those of a narrow ruling elite; third and most importantly, citizens do not choose their leader and cannot remove him from office. Recall Przeworski’s (1999)

argument on democracy's advantage compared to other regime types: people submitted to an unwanted leader revolt when there is no other way of getting rid of him, but prefer to wait when they expect an alternation in power to take place reasonably soon. Thus, in democracy, people do not revolt because leaders are selected for short mandates and are not reelected if they lose public support. If we extend this argument, in a typical dictatorship, alternation occurs either through the leader's death or through a revolution. In monarchies, even the leader's death does not lead to alternation because he is immediately replaced by a member of the ruling dynasty: rebellion is thus the only way of getting rid of an incompetent or unpopular leader.

I thus test following hypotheses:

*Hypothesis 6: Democracies run a lower risk of civil war than monarchies and republics.*

*Hypothesis 7: Monarchies run a higher risk of civil war than democracies and republics.*

Since the UCDP/PRIO data (on which the empirical analysis in section 5.4. was based) is only available from 1946 onwards, I rely on the Correlates of War (COW) Intra-State War data, version 4.0 (Small and Singer 1982; Sarkees 2010) to measure civil war. Data are available from 1816 to present. The dataset includes any armed conflict that takes place within the boundaries of a state and involves "effective resistance by both sides, and a total of at least 1,000 battle-deaths during each year of the war" (Sarkees 2010, 5). "Effective resistance" means either that "both sides [are] initially organized for violent conflict and prepared to resist attacks of their antagonists" or that "the weaker side (...) is able to inflict upon the stronger opponents at least five percent of the number of fatalities it sustains" (Small and Singer 1982, 215): these criteria are used to distinguish wars from one-sided state killings. The threshold of 1,000 fatalities is much higher than the one of the UCDP/PRIO dataset used in the previous analysis, which raises some comparability issues. Yet, one advantage of this threshold is that it limits the risk that coups are included in the civil wars list.

The COW project operates a further distinction within the category of intrastate armed conflict: civil wars, which involve the participation of the national government (war types 4 and 5 in the COW wars typology), are differentiated from regional internal wars (war

type 6), in which at least one of the participants is a local or regional government, and intercommunal wars (war type 7), in which none of the participants is a government (Sarkees 2010, 8). My analysis includes war types 4, 5, and 6, but I exclude intercommunal wars. However, given the relative rarity of wars, I do not distinguish between conflicts for control of the central government (war type 4) and conflicts over local issues (war type 5): both categories are collapsed into a single value. Finally, I only report the first year of the conflict: subsequent war years are coded 0.

**Table 5.9.: Civil war onset (1816-1913)**

	<i>Observations</i>	<i>Percent</i>
Civil war onset	106	2.08
Peace and ongoing conflict years	4,979	97.92
Total	5,085	100

(Source: Correlates of War)

From the same data source, I generate a variable (peace duration) that records the number of years passed since the last instance of intrastate armed conflict. The variable is used to address time-dependence issues.<sup>98</sup>

Some of the control variables included in the previous analysis (e.g., ethnic fractionalization) are not available for the period under consideration. However, data can be found for two of the most robust predictors of civil war, namely GDP per capita and population. To measure GDP per capita, I use the same data as in section 5.4: the Maddison project database (Bolt and van Zanden 2013) spans the whole period between AD 1 and 2010 and incorporates estimates of GDP per capita for most of the countries under investigation. Data on population are drawn from the National Material Capabilities dataset from the COW project (Singer et al. 1972): Population values are available from 1816 to present. Note, however, that data on both variables (especially GDP per capita) is extremely sparse. In particular, estimates on GDP are almost inexistent for the period from 1815 to 1860 as well as for countries outside of Europe and North America.

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<sup>98</sup> All duration variables are left-censored.

**Table 5.10. Descriptive statistics**

	<i>Mean</i>	<i>Std. deviation</i>	<i>Nonmissing observations</i>
Population (in thousands)	20202.03	53786.31	3162
GDP/capita	2070.432	1153.438	1626

(Source: Maddison project database; COW National Material Capabilities dataset)

I begin with a simple logit model without any control variables. I use republics as reference category, since I expect these regimes to run a higher risk of civil war than democracies but a lower risk of civil war than monarchies. To address reverse causation issues (since regimes are coded on the 31<sup>st</sup> of December), both independent variables are lagged one year. Results are displayed in Table 5.11, Model 5.

**Table 5.11. Democracies, monarchies, and civil war onset, 1816-1913 (logistic regression, reporting odds ratios)**

	<i>Model 5</i>	<i>Model 6</i>
Peace duration <sup>a</sup>		0.994 (0.011)
Monarchy <sup>a</sup>	0.701 (0.342)	0.190*** (0.100)
Democracy <sup>a</sup>	0.255** (0.145)	0.251** (0.143)
Population <sup>a, b</sup>		1.439** (0.251)
GDP/capita <sup>a, b</sup>		0.388*** (0.135)
Regime duration <sup>a</sup>		0.999 (0.003)
Countries	68	32
Observations	4,850	1,454

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

<sup>a</sup>Lagged one year <sup>b</sup>Log

As expected, democracy appears to have a relatively strong, negative effect on civil war onset: democracies' risk of experiencing armed conflict is only about 25% as high as republics' risk. The effect is significant at the 5% level. This lends preliminary support to Hypothesis 6. Monarchies appear to be negatively related to the outbreak of civil war, but the effect is moderate and the variable does not pass the 10% significance threshold.

Model 6 includes GDP per capita, population, regime duration, and peace years as control variables, and uses robust standard errors clustered by country. Given the large amount of

missing data for GDP and population, the sample shrinks dramatically: the number of observations is reduced to 1,454 and half of the countries are excluded from the analysis. Yet, results regarding democracy hold up: democracies are four times less likely to experience civil war than republics. Even when relevant controls are included, the variable remains significant at the 5% level. Note, however, that this only holds true when democracies are compared to republics: a pairwise comparison of democracies and monarchies reveals that the former perform slightly better than the latter, but the effect is negligible and not statistically significant.

What about monarchies themselves? When controls are included, the effect of the variable increases dramatically and becomes significant at the 1% level: according to the estimates of Model 6, monarchy divides the risk of civil war by five. This finding invalidates Hypothesis 7. Monarchy does not increase the risk of civil war. On the contrary, the fact that monarchies have a consistently negative effect on civil war onset gives some confidence that the peaceful attributes of monarchies are not a mere statistical artefact, and that this effect is not a mere regional effect.

## 5.6. CONCLUSION

Contrary to previous studies (Gurses and Mason 2010; Fjelde 2010; Wilson 2014), I do not find authoritarian regime type to affect the probability of civil conflict. More specifically, my findings question the finding that military regimes are internally more violent than other dictatorships, and challenge the emerging view of party regimes as “gentler and kinder”<sup>99</sup> dictatorships: the stability of the latter can be explained by their ability to manage intra-elite conflicts, but is not related to their ability to channel public dissent more effectively. Conversely, the instability of military dictatorships is primarily due to their vulnerability to coups: I do not find any evidence that they are more likely to be challenged from dissatisfied citizen groups. To the extent that prior research has uncovered a statistical relationship between authoritarian regime type and domestic conflict, I argue that it is probably spurious: the fact that military dictatorships experience more coups than

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<sup>99</sup> The expression is from Lijphart (1999), who called consensus democracies “kinder and gentler” democracies.

party regimes, and that some widely used civil war datasets include some instances of military coups in their list of conflicts, might partly explain this finding.

I also find that, compared to dictatorships, democracies are less likely to experience civil war. The fact that this effect holds up across different periods lends strong support to the theoretical expectation that democracies exhibit institutional features that make them inherently less vulnerable to rebellion, regardless of contextual factors. This result also contradicts one recurring finding, that democracy is not sufficient to prevent domestic conflict (see Hegre 2014): as discussed in Sections 5.1. and 5.2., this finding might result from an incorrect specification of the dependent or independent variables.

These results, so far, are consistent with the theoretical expectations formulated in Section 5.3. Yet, two surprising findings emerge: first, in spite of the institutionalization of the chief executive's selection, democracies are not immune to coups. Second, contrary to theoretical expectations, monarchy does not lead to an increased risk of civil war: as a matter of fact, the negative effect of monarchy on intrastate armed conflict remains robust to several model specifications and for different periods of investigation.

These findings need further investigations, but they point to two clear tendencies: first, they highlight the relative stability of institutionalized regimes (democracies being less prone to civil war, and monarchies being immune to both coups and conflicts). Second, they confirm the idea that authoritarian regime type is not a predictor of intrastate armed conflict: in non-institutionalized regimes, large segments of the population are typically excluded from decision-making, regardless of the identity of the rulers or the way they access and leave office; thus, these institutional features do not increase or decrease citizens' incentives to revolt, but they are powerful predictors of autocrats' ability to manage intra-elite conflicts and to avoid extraconstitutional seizures of power.

## 6

### **Alternation through death: Is gerontocracy an equilibrium?\***

According to the minimal approach to democracy, the reason why civil wars are less frequent in democracies is that opposition parties can expect a leadership change in a foreseeable future: although they would be better off in the short run rebelling rather than complying, they are, on the long run, better off complying if they have a reasonable chance of winning the next elections (Przeworski 1999). In dictatorships, waiting until the dictator dies is generally much costlier. This waiting time, however, is considerably shortened when the dictator is old. Therefore, the risk of domestic conflict should decrease along with the age of leaders.

The peace-inducing effect of aging might be a reason why, in primitive and agrarian societies, elderhood was often associated with high status (Flanagan 1989; Posner 1995) and why, in many tribes, the chief was chosen among the oldest men (Simmons 1945; Read 1959; Spencer 1965). This is classically explained by two main groups of factors, based either on social ties which are acquired with age or on personal characteristics, such as wisdom or knowledge (Werner 1981). However, the explanation according to which older leaders are more competent is contradicted by recent evidence showing that aging leaders tend to produce less economic growth (Jong-A-Pina and Mierau 2011) and to be more often involved in interstate wars (Horowitz et al. 2005; Potter 2007; Bak and Palmer 2010). Furthermore, both explanations fail to explain why, in modern democracies, gerontocracy declines. The explanation I put forward is that old leaders are preferred over younger ones unless there is a regular, institutionalized mechanism to change the leader

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\* Parts of this chapter appear in: Magni Berton, Raul, and Sophie Panel. 2017. Strategic gerontocracy: why nondemocratic systems produce older leaders. *Public Choice* 171 (3): 409-427.

without bloodshed: gerontocracy in nondemocratic systems is a functional equivalent of periodic elections and fixed terms in democracies.

More specifically, if this explanation is correct, we should expect systems in which the leader is elected but cannot be removed to produce older leaders than systems in which the leader is selected through other procedures than elections (typically, dictators): if there is no institutional mechanism to remove the leader, voters face a clear incentive to select a candidate who is likely to give up power after a few years, in order to minimize the possible loss and avoid the worst-case scenario (i.e., the scenario in which a bad ruler stays in office for decades). Since future leaders may be appreciated or hated, the minimax strategy consists in shortening the time of their reign. The other crucial implication is that introducing a fixed term in office and regular elections in these systems should automatically decrease the average age of elected leaders.

These consequences are difficult to observe empirically because systems in which the leader is elected but not removable have become relatively rare since the Middle Age, as most monarchies stopped practicing elections and gradually adopted hereditary succession (see Kokkonen and Sundell 2014). In most post-World War II political systems, the leader either inherits power (as in hereditary monarchies), accesses power by force (as in most dictatorships), or is elected for a fixed term (as in democracies). A quick glance at history lends some credence to the intuition that leaders selected for life tend to be fairly old: for example, in Venice, most elected officials were selected for a fixed term in office except for one who was elected for life, namely the Doge (Tullock 2002). As a consequence, while European kings started their reigns at age 33 on average, the Doge's mean age at start of reign was about 68 (Eisner 2011).<sup>100</sup> This combination of mechanisms for limiting incumbency has proven fairly stable in the long run, since Venice experienced few coups or inter-family wars in its several centuries of existence (Tullock 2002, 252).

In the contemporary period, there is one instructive case of a political system in which the leader is elected for life, namely the Vatican. Although the pope is elected, there are no mechanisms but his death to dismiss him. As a consequence, since 1503, the average age of popes on the day of their election is 64. In particular, the last two popes were over 75 at their election, which reflects the increase in life expectancy: no re-elective democracy is so

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<sup>100</sup> Means are calculated for the period from AD 600 to 1800.

gerontocratic. Electing old popes produces a limitation in incumbency (the average length of pontificate is 10 years), which could approach that of a re-elective democracy.

The case of the Vatican is emblematic because elections are clearly the way by which the leader is selected. As noted above, systems in which individuals self-select into leadership positions – as it is the case of most contemporary dictatorships – cannot be expected to be as gerontocratic, since there are no ways to prevent a young dictator from taking over power.<sup>101</sup> There are, yet, some recent examples that approximate the Vatican case: for example, Tullock (1987, 159) points to a similar tendency to move toward gerontocracy in the USSR, which political system provided a fairly routinized procedure to select the General Secretary of the Party but none to dismiss him. Indeed, while Stalin was fairly young (44 years) as he accessed office, the age of the following General Secretaries increased continuously and peaked with Chernenko (aged 73 as he assumed office). The average age of Stalin's successors at the time they took office was 62 years. By contrast, Mexican Presidents from 1933 to 2000 took office at the mean age of 48 years. This average drops to 46.5 years if an outlier (Adolfo Ruiz Cortines, who entered office at age 63) is removed from the sample, and is not influenced by the evolution of life expectancy, since the last two PRI Presidents entered office at the age of 40 and 43 years, respectively. The USSR and the PRI regime were both party dictatorships, but the PRI dictatorship displayed an interesting feature: a constitutional reform in 1933 introduced a fixed six-year presidential term without the possibility of reelection. Since the expected tenure of Mexican leaders was truncated, this lowered the incentive to select aging leaders.

To give a final example, Xi Jinping was appointed General Secretary of the Chinese Communist Party at age 59, and became thereby the youngest General Secretary since 1980. China after Mao started moving toward gerontocracy as well. Yet, the Communist Party recently introduced an age limit for party leaders and a limit of two terms for presidents (Köllner 2013). Both measures exclude the possibility of lifelong tenure, and, if fully implemented, can be expected to produce younger leaders on the long run. They can

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<sup>101</sup> Indeed, coup plotters can be very young, like for example Sierra Leone's Valentine Strasser (who became at age 25 the world's youngest head of state as he took over power in 1992). Another famous coup plotter, General Augusto Pinochet, was in his late fifties when he became President of Chile – but then again, he was appointed by the other members of the junta by joint decree. Incidentally, Geddes (1999, 123) notes that one of the reasons why he was first chosen to lead what was supposed to be a collegial junta was that he was then its most senior member.

also be expected to mitigate the violent power struggles that usually characterize political succession in China (see Sandschneider 1985): according to Köllner (2013, 97), Jiang Zemin's succession in 2002 was the first one that occurred without significant intra-party tensions.

These examples lend some credence to the idea that selecting aging leaders does not necessarily reflect people's genuine preferences or expectations about the competence of their rulers, but is simply a way to foster civil peace. There are yet other empirical consequences that can be derived from this hypothesis. For example, high-ranked civil servants in democracies are likely to be considerably older than elected officials.<sup>102</sup> In general, non-removable offices are likely to be held by old people, with the exception of military members who tend to retire earlier. The main testable consequence of this hypothesis, however, is that, in political regimes which are deprived of an institutional way to dismiss the leader, the risk of violent conflict decreases as he gets older.

So far, the relationship between leaders' age and domestic conflict has never been investigated. While leaders are more and more considered as one of the main units of analysis in international relations, current research almost exclusively focuses on the impact of leaders' features on their own behavior (e.g., Horowitz and Stam 2014), including studies centered on the age of dictators (Horowitz et al. 2005; Potter 2007; Bak and Palmer 2010). The argument presented here departs from prior research on age and leadership to the extent that it investigates the impact of leaders' age on the expectations and subsequent behavior of people ruled by these leaders, rather than on leaders' own behavior. This difference probably explains why, while the relationship between age of dictators and international wars has been largely studied, nothing is yet known about age and civil wars. Indeed, unlike international wars that are initiated by leaders, civil wars are often triggered by an organized part of civil society.<sup>103</sup> Personal characteristics of leaders indubitably influence their activity – like their foreign policies, for example – but they also influence the activity of their environment, like the opposition's propensity to revolt.

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<sup>102</sup> If this explanation is correct, this provides a justification for the seniority principle that regulates promotions in most public administrations. This also provides an explanation for the seemingly absurd way of selecting members of the French Constitutional Council: The only members who are appointed for life are retired Presidents. Other members serve a non-renewable term of nine years.

<sup>103</sup> Although it might be difficult to identify the initiator of a conflict with accuracy, most studies on political institutions and civil war at least implicitly rely on the assumption that civil conflicts are initiated by dissatisfied opposition groups, not the incumbent (see Chapters 3 and 5).

In the next section, I add some qualification to this general argument, and present the hypotheses. Section 6.2. provides an overview on the data and methods. Section 6.3. presents the results. Section 6.4. concludes.

### 6.1. AGING AUTOCRATS AND CIVIL WAR: THEORY AND HYPOTHESES

The general hypothesis tested here is that, in systems in which there is no institutional way of getting rid of unwanted rulers, the risk of revolt varies according to their age: the older the leader, the lower the risk of domestic conflict. The underlying argument is that leadership change generally occurs through three mechanisms: a regularized procedure to dismiss the leader, the leader's death, or a revolution.<sup>104</sup> Rebellion becomes desirable when opposition parties cannot expect any alternation in power reasonably soon, i.e. when the leader is young *and* there is no formal limitation to his tenure. Thus, introducing a rule that reduces leaders' expected tenure (such as regular elections and fixed terms in office) *or* selecting a relatively old leader should decrease the desirability of rebellion.

This formulation implies that the impact of leaders' age on revolt is observable only under certain conditions. The most straightforward implication is that regimes that have regular, competitive elections (i.e., democracies) make null the impact of age on civil wars because they rely on other mechanisms to get rid of the leader. Since even young leaders can be dismissed when they become unpopular, age becomes entirely irrelevant.

*H1: In democracies, the age of the leader does not impact the risk of civil war.*

There are yet other potential exceptions to the general rule. According to Przeworski (1999), democracy produces social peace through two related but nevertheless distinct mechanisms: first, leaders change regularly; second, the "losers" can expect to become winners in the next round. Distinguishing between these two mechanisms is less relevant in the case of democracies since they produce both effects. However, things get tricky in the case of nondemocratic regimes in which alternation in power occurs regularly but all leaders belong to the same ruling coalition. Indeed, the theory sketched above is silent

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<sup>104</sup> Strictly speaking, another mechanism is voluntary retirement. But this is very rare (Goemans et al. 2009a).

about what foremost leads opposition groups to rebel. A “soft” version of the theory would state that rebellion is motivated by the opposition’s willingness to avoid being ruled by a bad leader for a long period of time. A harder version would state that, even when alternation in power occurs fairly regularly, opposition groups will always tend to rebel unless they have a chance of attaining power in a foreseeable future.<sup>105</sup> These assumptions about the motivations of opposition groups have implications for the relationship between leaders’ age and rebellion: if the prospect of a future alternation in power – regardless of who will take office next – is sufficient to produce social peace, we should expect all nondemocratic regimes to be more stable if they are ruled by relatively old leaders. If, however, the decisive factor is the opposition’s chance of attaining power, the impact of leaders’ age should depend on the regime’s ability to produce successors to the incumbent. Monarchies constitute an emblematic case, since their defining feature is hereditary succession. Like every ruler, monarchs grow old and eventually die; yet, they are immediately replaced by their designated heir upon their death. Alternation through death does occur, but does not significantly affect the opposition’s chances of being in power: the softer version of the theory would predict that the likelihood of rebellion decreases as the monarch grows older, but it seems more reasonable to assume that, for the opposition, there are few incentives to wait until his death since it will not increase its own chances of taking power. Thus, in monarchies, the incentives to rebel are stable over time and are not affected by the monarch’s age. Monarchies cancel the impact of the leader’s age on the risk of rebellion exactly as democracies do, but for completely different reasons: whereas democracies impact this relationship by shortening time horizons, monarchies do by lengthening them to infinity.

*H2: In monarchies, the age of the leader does not impact the risk of civil war.*

The three remaining regimes – military dictatorships, party regimes, and civilian autocracies – are more ambiguous. All of these regimes are poorly institutionalized, display strong tendencies toward power personalization, are not based on a clear time

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<sup>105</sup> This distinction resembles Collier and Hoeffler’s (2004) greed and grievances approach, applied to political regimes: in the former case, opposition groups are simply unhappy about the current incumbent; in the latter case, they want to be in power themselves.

horizon, and do not rely on transparent rules regarding the access to and succession in power: we should then expect the leader's age to be empirically related to domestic conflict in all of these regimes.

Yet, party regimes and military dictatorships display a particular characteristic that distinguishes them from civilian autocracies: both rely on a strong organization that is somehow involved in the selection and removal of leaders. This has two implications. First, even though most of these regimes (with some exceptions, such as the PRI regime discussed above) do not provide a transparent, regular procedure for alternation and leadership succession, these organizations are able to remove and replace leaders fairly often in practice. Second, even though leadership change mostly occurs in an irregular fashion and often involve purges, assassinations and other forms of violence (for succession in party regimes, see Svobik 2012; for succession in military dictatorships, see Geddes 1999), it usually does not end the regime, and, most importantly, it tends to bring regime insiders to power. This means that, regardless of the degree of power concentration and personalization in each regime, the mere existence of an organization able to select successors from within its own ranks should cancel the peace-inducing impact of the dictator's aging: opposition groups considering rebellion cannot expect to have better chances of accessing power upon the leader's death. Note also that in party and military regimes, the launching organization's ability to remove leaders from power should lessen the risk that the country is ruled by a completely incompetent autocrat, and thereby reduce the potential for mass discontent. The most important point, however, is that, in these regimes, individual characteristics of the leader are likely to be less relevant for explaining the occurrence of civil war.

*H3: In single-party regimes, the age of the leader does not impact the risk of civil war.*

*H4: In military dictatorships, the age of the leader does not impact the risk of civil war.*

By contrast, civilian autocrats do not have a strong organization on which to rely (see Cheibub et al. 2010, 86). They can rely on the military to maintain their rule, but do not belong to the core organization; they can be backed by a party, but the operational definition of civilian autocracies implies that this party is weaker than in typical party

regimes, and does not preclude the existence of other parties who compete for office. All in all, this implies that, when the leader dies, his launching organization is usually not able to replace him, which provides a window of opportunity for opposition groups. In those regimes, we should expect a negative relationship between the leader's age and the risk of civil war.

*H5: In civilian autocracies, the age of the leader decreases the risk of civil war*

To sum up, elections are not the only path toward social peace: regimes in which there are no institutionalized ways of removing leaders can still be relatively stable as long as the leader's expected tenure does not exceed a reasonable amount of time, that is, when the leader is relatively old. However, this mechanism is at work only in a specific subset of regimes, namely those in which the opposition can reasonably expect that the incumbent's launching organization will not be able to impose a successor upon his death. Interestingly, this might be one of the reasons why some personalist autocrats are able to stay in power for decades (see Geddes 1999), and why dictators' overall risk of losing office decreases throughout their tenure (Bienen and van de Walle 1989, 1992; Svobik 2012, Chap. 3): the length of time dictators spend in office does not only affect their political resources and their ability to control their ruling coalition, but also the desirability of rebellion (since long-lasting autocrats are likely to be older on average).

Before turning to the empirical analysis, a caveat is in order: it is important to keep in mind that the hypotheses to be tested only pertain to the timing of the rebellion within each regime category, and say nothing about each regime's baseline risk of experiencing a civil conflict. A source of potential confusion resides in the fact that regular alternations in power were used in the previous chapters to explain why democracies generally experience fewer civil conflicts than nondemocracies. This, of course, does not pertain to the comparison between dictatorships that will be performed in the following sections: the argument is that, in some regimes, the probability that a violent conflict breaks out will depend on the age of the incumbent, while in some others, this probability remains constant over time.

## 6.2. DATA AND ESTIMATION STRATEGIES

I estimate the probability of armed conflict onset with logistic regression. I use two different estimation strategies: first, I distinguish five subsamples that correspond to the five regime types I investigate. Second, I conduct analyses based on interaction terms: on the whole sample, I estimate the impact of each political regime interacted with the leader's age. All models include controls for time-dependence and use robust standard errors clustered by country.

The dependent variable is a dummy that takes on the value of 1 in years an internal conflict breaks out if at least two years have passed since the last observation of the conflict. Subsequent conflict years are coded 0. The measure of armed conflict is based on the UCDP/PRIO data. I include conflicts over government and conflicts over both government and territory; however, I exclude separatist conflicts, since they do not concern the central government and hence are not necessarily related to alternation in power. For reasons exposed in Chapter 5, I also exclude country-years in which both an armed conflict and a coup (according to Powell and Thyne 2011) are reported: these observations are coded 0. To address potential time-dependence problems, I compute a variable measuring the time in years since the most recent occurrence of civil conflict, and add it to the list of predictors.

**Table 6.1. Distribution of political regimes and intrastate conflict outbreaks, 1946-2008**

	<i>Total observations</i>	<i>Conflict outbreaks (frequency)</i>	<i>Conflict outbreaks (% years)</i>
Military dictatorship	1,694	43	2.54
Monarchy	656	3	0.46
Civilian autocracy	1,703	44	2.58
Single-party regime	841	15	1.78
Democracy	3,209	39	1.22

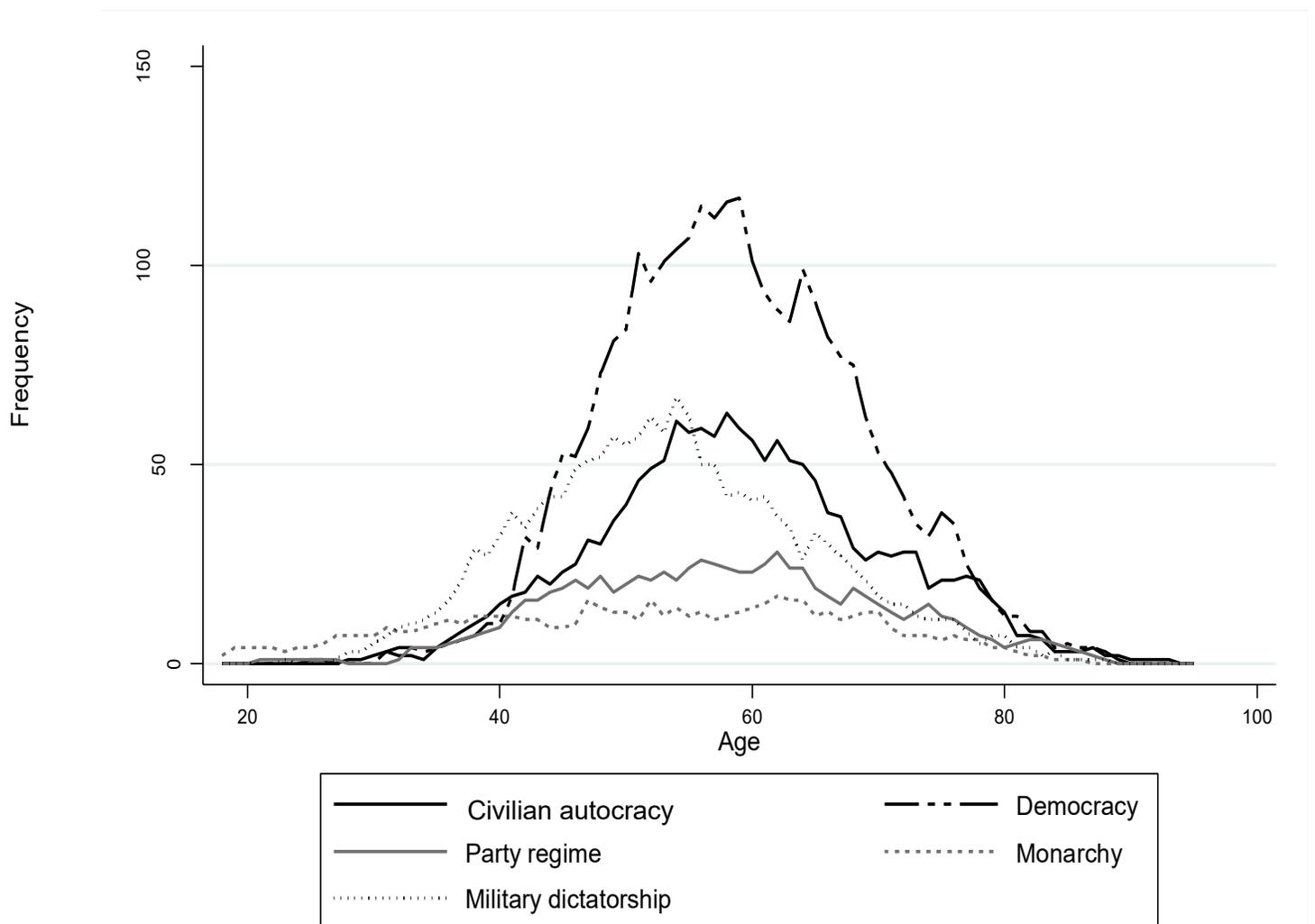
(Sources: Cheibub et al. 2010; UCDP/PRIO Armed Conflict Dataset)

Data on regime type are drawn from Cheibub et al.'s (2010) dataset (see Chapter 2).

Descriptive statistics on the distribution of political regimes and on the frequency of intrastate conflict are shown in Table 6.1.

The leaders' age is the main explanatory variable. Data are provided by the Archigos database (Goemans et al. 2009a). In years where two (or more) leaders were reported, I select only the age of the leader in office on First January, in order to avoid reverse causation issues: the leaders' age comes ever before the conflict. However, I also add a dummy that indicates the years a change of leadership occurred.

**Figure 6.1. Leaders' age by regime type, 1946-2004**



The main problem with the leader's age is that it is correlated with the number of years the leader has been in office. Thus, the effect of age on civil war could be due to the negative correlation between leaders' tenure and the probability of a civil war outbreak: the longer leaders stay in office, the lowest the risk of armed conflict, either because they have had time to reinforce their control on the country or because their long tenure precisely indicates they have been popular enough to avoid being violently overthrown (for a discussion, see Bienen and van de Walle 1989, 1992). In order to distinguish both effects, I also add a variable that measures the number of years the leader has already been in office. Descriptive statistics on these variables are displayed in Table 6.2.

**Table 6.2. Regime duration, leaders' mean age and leaders' average tenure by regime type, 1946-2004**

	<i>Total observations</i>	<i>Average regime duration</i>	<i>Leaders' mean age</i>	<i>Leaders' mean tenure (years)</i>
Democracy	2,585	31.0	58.8	3.0
Military dictatorship	1,534	12.5	53.3	7.6
Single-party regime	802	12.9	57.7	9.7
Monarchy	587	32.5	51.6	10.4
Civilian autocracy	1,468	12.4	59.1	6.4
<b>Total</b>	6,976	22.2	56.9	6.1

(Sources: Cheibub et al. 2010; Archigos)

Table 6.2. reveals that civilian autocracies are the most gerontocratic, slightly more than democracies. However, they are not those in which the leaders' average tenure is the longest. Monarchs and party leaders govern roughly ten years on average, while civilian autocrats stay in office roughly six years on average. This difference can be partly explained by the fact that monarchs tend to access office earlier, and that the average regime duration is relatively low for civilian autocracies: they display the shortest timespan among all regimes.

Finally, in addition to leader duration and peace duration, I include all control variables I used in Chapter 5, that is: the natural log of GDP per capita (Bolt and van Zanden 2013); Cederman et al.'s (2010) index of ethno-linguistic fractionalization (ELF); population

(Ross 2013); regime duration, based on the modified version of Cheibub et al.'s (2010) regimes data; and a Cold War dummy.

Table 6.3. displays descriptive statistics.

**Table 6.3. Descriptive statistics by regime**

	<i>Democracy</i>	<i>Military</i>	<i>Monarchy</i>	<i>Party</i>	<i>Civilian</i>	<i>Sample</i>
Cold War (N. obs.)	1,641	1,232	467	725	1,005	5,114
Post Cold War (N. obs.)	944	302	120	77	463	1,906
ELF: mean (std. dev.)	0.304 (0.266)	0.412 (0.306)	0.533 (0.335)	0.500 (0.319)	0.485 (0.319)	0.404 (0.309)
(ln) GDP/capita: mean (std. dev.)	8.567 (0.949)	7.364 (0.730)	8.103 (1.128)	7.269 (0.790)	7.534 (0.824)	7.895 (1.039)
(ln) Country size: mean (std. dev.)	12.208 (1.847)	12.604 (1.516)	11.797 (2.063)	12.625 (1.688)	12.316 (1.873)	12.328 (1.797)
(ln) Population: mean (std. dev.)	16.174 (1.484)	15.947 (1.319)	14.753 (1.569)	15.686 (1.413)	15.915 (1.652)	15.907 (1.530)
Peace duration: mean (std. dev.)	18.827 (17.175)	13.099 (11.827)	14.929 (12.567)	14.249 (11.830)	11.195 (12.233)	15.058 (14.476)
Leader duration: mean (std. dev.)	3.013 (3.986)	7.555 (8.250)	10.407 (9.529)	9.668 (8.383)	6.420 (6.703)	6.108 (7.267)
Leader change (% of obs.)	24.14	14.28	6.30	6.11	11.31	15.68
Regime duration: mean (std. dev.)	31.003 (31.852)	12.486 (10.076)	32.596 (28.061)	17.372 (13.773)	15.406 (13.017)	22.216 (24.346)

### 6.3. RESULTS

I first run the model for each regime subsample. Table 6.4. displays the results.

Results reported in Table 6.4., Model 1 indicate that age has a negative and significant impact on the dependent variable in the case of civilian autocracies: in those regimes, the risk of civil war decreases by about 5% as the age of the leader increases by one year. Furthermore, as expected, the impact of age is almost inexistent in the cases of democracies (Model 2), monarchies (Model 3), party regimes (Model 4) and military

dictatorships (Model 5). Results concerning monarchies have to be interpreted cautiously: the small number of valid observations (309) as well as the positive sign of peace duration cast some doubts on the validity of the inference, and two other control variables (Cold War and Leader change) are omitted from the estimation due to perfect prediction of the 0 outcome.<sup>106</sup> These anomalies notwithstanding, all hypotheses are so far supported.

**Table 6.4. Leaders' age and civil conflict onset by regime type, 1946-2001 (logistic regression, reporting odds ratios)**

	(1) <i>Civilian</i>	(2) <i>Democracy</i>	(3) <i>Monarchy</i>	(4) <i>Party</i>	(5) <i>Military</i>
Peace duration	0.966 (0.020)	0.991 (0.0119)	1.302** (0.134)	0.934** (0.0292)	1.015 (0.0169)
Age	0.952** (0.020)	0.993 (0.0212)	1.029 (0.0301)	1.009 (0.0398)	1.029 (0.0186)
Leader tenure	1.040 (0.050)	0.937 (0.0634)	0.736*** (0.0354)	1.036 (0.0464)	0.924*** (0.0140)
Regime duration	0.950** (0.028)	1.016 (0.0168)	1.007 (0.0622)	0.977 (0.0528)	1.043** (0.0195)
Leader change	4.295*** (1.680)	1.507 (0.563)	<i>omitted</i>	4.028 (4.107)	1.308 (0.604)
ln(GDP/capita)	1.740** (0.453)	0.627* (0.151)	0.382** (0.180)	1.303 (0.476)	0.396** (0.162)
ln(Population)	1.333** (0.179)	1.333** (0.158)	3.070 (3.873)	1.008 (0.282)	1.025 (0.163)
ELF	1.959 (1.401)	7.731*** (6.064)	0.0578 (0.278)	0.485 (0.445)	4.064** (2.593)
Cold War	1.262 (0.593)	0.629 (0.391)	<i>omitted</i>	0.248** (0.146)	0.485 (0.246)
Constant	5.09e-05*** (0.000)	0.00345* (0.0117)	2.43e-08 (4.60e-07)	0.0101 (0.0429)	1.775 (4.837)
Observations		2,616	309	729	1,503

Robust standard errors in parentheses.

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

On the whole, control variables display the expected sign, with a notable exception: GDP per capita has a positive and significant effect in Model 1. The fact that regime duration increases the risk of civil conflict onset in military dictatorships is somewhat surprising,

<sup>106</sup> A closer look on the data reveals that none of the three conflict outbreaks reported in monarchies occurred after the end of the Cold War.

but nonetheless consistent with Fjelde's (2010) findings: one explanation could be that military dictatorships first enjoy some support from the public, but their popularity declines over time (see Nordlinger 1977). Perhaps the most interesting finding is that leadership change is only associated with civil conflict in the case of civilian autocracies: the effect of the variable is consistently positive across all regimes, and non-negligible in the case of party regimes; yet, it does not pass the statistical significance test except in civilian autocracies (where it increases the risk of conflict by a factor of 4.3). Since leadership change and civil conflict outbreaks are measured in the same year, it is impossible to know whether succession crises in civilian autocracies have a tendency to escalate into mass conflict or whether civil conflicts result more often in a change of leadership in these regimes (i.e., whether leaders of civilian regimes simply lose more wars than other leaders). However, this lends support to the idea that civilian autocracies are the only ones in which changes of leadership deeply destabilize the whole regime.

As a first check for the sensitivity of the results, I rerun Model 1 with country and year fixed effects. Although fixed effects are a highly conservative estimation method given the structure of the data, the impact of age on civil conflict outbreak holds with these estimates. Actually, the results regarding age even improve: the variable becomes significant at the 1% level and has a larger estimated effect on the dependent variables, the risk of civil war decreasing by 10% for each additional year. I also try King and Zeng's (2001) correction procedure for rare-event data: neither the coefficients nor the significance of the variables of interest vary substantially.

I now move on to report the results of estimations with interaction terms. Results are displayed in Table 6.5.

Models 6 to 8 display coefficients for age, regimes and the interaction of both. In Model 6, the impact of the leader's age in civilian autocracies is compared with its impact in all other regimes (including democracies). Model 7 is the same model restricted to nondemocratic regimes. Finally, Model 8 includes interaction terms for all regimes, which allows me to observe the effect of age in all of them; military dictatorships are the reference category.

**Table 6.5. Leaders' age and civil conflict onset, 1946-2001 (logistic regression, reporting odds ratios)**

	(6) <i>Ref.: all regimes</i>	(7) <i>Ref.: autocracies</i>	(8) <i>Reference: military</i>
Peace duration	0.986 (0.00962)	0.990 (0.0130)	0.987 (0.00963)
Age	1.003 (0.0104)	1.007 (0.0119)	1.013 (0.0170)
Age * civilian	0.968* (0.0190)	0.963** (0.0176)	0.958** (0.0197)
Civilian	8.824* (10.49)	9.307** (10.26)	11.22** (13.08)
Democracy			0.946 (1.536)
Age * democracy			0.989 (0.0278)
Party			1.595 (2.576)
Age * party			0.990 (0.0292)
Monarchy			0.983 (0.0341)
Age * monarchy			0.633 (1.217)
Regime duration	1.001 (0.0147)	0.984 (0.0167)	1.003 (0.0145)
Leader's tenure	0.986 (0.0207)	0.989 (0.0225)	0.976 (0.0217)
Leader change	1.961*** (0.454)	2.305*** (0.613)	2.082*** (0.490)
ln(GDP/capita)	0.699* (0.131)	0.883 (0.181)	0.759 (0.150)
ln(Population)	1.225*** (0.0908)	1.153 (0.101)	1.235*** (0.101)
ELF	2.468** (1.009)	1.559 (0.625)	2.600** (1.050)
Cold War	0.590** (0.146)	0.477** (0.138)	0.563** (0.144)
Constant	0.008** (0.0162)	0.006** (0.0140)	0.003*** (0.006)
Countries	151	117	151
Observations	6,569	3,953	6,569

Robust standard errors in parentheses.

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

In all models, I find that the risk of armed conflict significantly decreases as the dictator gets older: for each year added to the leader's age, there is a drop of 3% to 4% in the odds of civil war. This means that a 30-year-old leader is 4.5 to 7.5 times as likely to experience civil war as an 80-year-old leader. Note that the results hold even though I control for the leader's tenure. By contrast, none of the other interaction terms in Model 8 attains statistical significance: civilian autocracies are the only regime in which the leader's age has an influence on rebellion. Taken together, these results lend further support to all hypotheses being tested.

All models estimate a large, positive and significant effect of civilian autocracies on civil war. Although this result makes intuitive sense – to the extent that it is easier to overthrow a regime which is not based on a solid institutional setup (see Acemoglu et al. 2010; Gurses and Mason 2010) – it should be recalled that coefficients of constitutive terms of

interaction terms cannot be interpreted as “normal” coefficients (see Brambor et al. 2006).<sup>107</sup> With regard to other political variables, neither leaders’ tenure nor regime duration attain statistical significance in any of the models. By contrast, change of leadership is consistently positively correlated to the outbreak of civil conflict. Other control variables display the expected signs, but their level of statistical significance and the magnitude of their estimated effect vary across models. Interestingly, yet, political variables are the only ones that are significantly associated with the onset of intrastate armed conflict when the sample is restricted to nondemocratic regimes: neither GDP per capita nor population nor ethno-linguistic fractionalization attain statistical significance in Model 7.

The same aforementioned robustness checks are performed. Findings hold when standard errors are clustered by countries or by year; they hold under country and years fixed effects; and they are confirmed using rare-event logit.

#### 6.4. CONCLUSION

When submitted to an unwanted ruler, people must decide whether to rebel or to wait until the next change of leadership. If no alternation in power can be expected within a reasonable length of time, rebellion then becomes the preferred option. Peaceful alternation can occur either through institutionalized mechanisms (like elections) or through the leader’s death: this is why, in regimes that lack such institutionalized alternation mechanisms, the leader’s aging reduces the probability of a rebellion.

I find that, in democracies, the age of the leader does not impact the risk of domestic conflict, while in some dictatorships the leader’s age correlates negatively with the outbreak of civil war. This finding helps understand why primitive and agrarian societies

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<sup>107</sup> These coefficients should be interpreted as the marginal effects of each constitutive term when the other constitutive term is equal to zero (see Brambor et al. 2006). For example, in Model 8, civilian autocracies are associated with an increase of the odds of civil conflict by a factor of 11 relative to military dictatorships. Since civilian autocracies are included in the model together with an interaction term of age and regime, this indicates that civilian dictatorships are 11 times as likely to experience civil war as military dictatorships *when the dictator is 0 years old*. Conversely, the very small (and insignificant) coefficient of age in all models indicates that age has no impact on civil wars when the civilian dictatorship dummy is set to 0.

were typically ruled by old men, while this is less the case of contemporary regimes. However, simply contrasting democracies – that display regular mechanisms to get rid of the leader – with autocracies – that lack such mechanisms – may be an oversimplification. After taking into account the diversity of contemporary authoritarian regimes, it appears that the impact of the leader's age on civil conflict is dependent on the institutional context: in military dictatorships, party regimes, and monarchies, the age of the leader does not have any discernible effect on civil wars, while it is highly relevant to explain the occurrence of civil conflict in civilian autocracies. Two conclusions can be drawn from these findings: first, collective leadership reduces the impact of the leader's personal characteristics on the behavior of citizens. Second, in dictatorships, the ruling coalition's ability to replace aging autocrats alters citizens' time horizons and thus cancels the impact of age on rebellion.

**Party competition, alternation, and rebellion:  
How election outcomes affect the risk of intrastate armed conflict**

According to Przeworski (1999), civil war is less likely to break out when elections take place regularly and when opposition parties have a fair chance of winning them. A relatively straightforward implication with regard to electoral competition follows: democracy produces social peace when no single political force dominates (Przeworski 1999). For political parties to choose compliance instead of rebellion, all of them must have a fair chance of attaining political power: if a party repeatedly wins elections by a large margin, opposition parties are better off rebelling. By contrast, if election outcomes are close – i.e. if popular support is evenly distributed between the several political forces – opposition parties can reasonably expect to win in the next round, and lose their incentive to rebel.

Although theoretically compelling, this argument recently came under fire. Using electoral data from Colombia, Chacon et al. (2011) show that in fact, violent conflict is *ceteris paribus* more likely to occur in situations in which electoral support is evenly distributed between ruling party and opposition party. Their underlying argument is that compliance depends not only on opposition groups' probability of doing well under current institutions, but also on their probability of prevailing in an armed confrontation. Since the fighting strength of opposition groups is related to their electoral support, democracies are more likely to achieve stability when the balance of power between government and opposition parties is asymmetric: parties without popular support may not have any chance of winning the next election, but they also have few chances of winning a war. The somewhat counterintuitive consequence is that weak parties – those that would have the strongest incentive to rebel – are also the most likely to comply.

The argument I put forward is that both views are correct. Opposition groups, on the one hand, are *ceteris paribus* more likely to rebel when they enjoy broad popular support. On the other hand, they are *ceteris paribus* less likely to rebel when they have a reasonable chance of attaining political power under the current institutions. Consequently, there are two paths toward social peace: either opposition parties are deterred from rebelling by the incumbent's overwhelming electoral victories, or they perform well at the polls and are subsequently allowed to take office and sit in the legislature. By contrast, conflict breaks out in situations where the political power opposition parties are granted does not match their level of popular support: in these cases, opposition parties are better off rebelling than complying.

Next section offers a brief review of contemporary scholarship on authoritarian elections and explores how the argument sketched above can pertain to both democracies and authoritarian regimes. In Section 7.2., I derive an empirical expectation from this argument. Section 7.3. gives an overview on the data and methods and presents the results. Section 7.4. concludes.

## **7.1. THE STABILIZING FUNCTION OF AUTHORITARIAN ELECTIONS**

The overwhelming majority of contemporary political regimes – including many highly repressive ones – hold regular elections at the national level. But, obviously, not all electoral regimes are the same: elections are held for different bodies, and vary enormously with regard to their competitiveness. In established democracies, both the executive and the legislature are elected, with typically close electoral outcomes and narrow margins of victory; on the other side of the spectrum, the most closed authoritarian regimes hold elections in which the chief executive's seat is not at stake at all, or that the incumbent repeatedly wins with 100% of the votes. Between these two extreme poles, features of elections vary greatly, especially in authoritarian regimes (see Gandhi and Lust-Okar 2009; Hyde and Marinov 2011).

The diffusion of elections across political regimes, especially in recent years (see, e.g., Levitsky and Way 2002; Hadenius and Teorell 2007a), has given rise to a growing body of

research examining their role and effects in authoritarian regimes. Early works have approached authoritarian elections from the perspective of the opposition, and investigated – with varying degrees of optimism – whether semi-competitive elections can contribute to the liberalization of authoritarian regimes on the long run (see, e.g., van de Walle 2002; Schedler 2002; Levitsky and Way 2002; Howard and Roessler 2006; Lindberg 2006a, 2006b). More recently, observing that dictators sometimes face a serious risk of losing elections, scholars have begun to wonder why they hold them at all. Consequently, an emerging body of research has begun to analyze elections from the perspective of the ruling regime, starting from the assumption that elections should provide autocrats with some benefits that outweigh these risks. The underlying argument is that authoritarian elections are not necessarily concessions extracted from the incumbent by opposition forces (see, e.g. Acemoglu and Robinson 2006). Nor are they mere window-dressing: they can also be instruments of control that contribute to the regime’s stability.<sup>108</sup>

Broadly speaking, arguments pertaining to the function of elections in authoritarian regimes can be sorted into three groups: elections are assumed to perform either a cooptation function, an information function, or a deterrence function. With regard to the cooptation function, Gandhi and Przeworski (2006, 2007) argue that some authoritarian regimes choose to hold multiparty legislative elections in order to broaden their basis of support when they need to neutralize threats from potential opposition groups: by giving these groups a vested interest in the survival of the regime, dictators who allow an elected, multiparty legislature reduce their risk of losing office. Relatedly, Lust-Okar (2004, 2006) analyzes the manner in which relatively competitive elections can contribute to the stability of authoritarian regimes: by deciding to open electoral competition for opposition parties, incumbents can choose to allow some parties to compete in elections – which, in turn, provides these parties with access to state resources they can distribute in exchange for electoral support – while excluding others. This gives legal and illegal opponents conflicting interests, which, in turn, strengthens the incumbent – legal opponents becoming less willing to challenge the regime, and the most radical opponents being marginalized. According to the second line of argumentation, autocrats can use elections to garner crucial

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<sup>108</sup> For an exhaustive review of the different arguments about elections in authoritarian regimes, see Gandhi and Lust-Okar (2009).

information about the support they enjoy among the public, or the strength of potential opponents. One of Cox's (2008) main arguments is that bargaining failures resulting in coups often arise from the fact that autocrats often have only incomplete information about their potential challengers: elections can solve this problem by giving both incumbent parties and opposition parties the ability to credibly communicate their relative strength (see also Londregan and Vindigni 2008). Magaloni (2006) similarly suggests that authoritarian elections are used to gather information about the extent of the party's support and its geographic distribution, which, in turn, allows incumbents to reward their supporters and punish defectors. In a somewhat different but related way, Malesky and Schuler (2011) argue that, although single-party regimes that do not allow contested elections are per definition unable to gain information about organized opposition, they can still use elections to monitor the competence and compliance of lower-level party officials, and thereby help autocrats discipline party cadres and alleviate principal-agent problems. They show that leaders in Vietnam gather this information by sending centrally nominated candidates to compete against local candidates in provincial elections, and assessing their respective electoral performance.

Finally, authoritarian elections can serve a deterrence purpose. Geddes (2006) argues that dictators create parties – made up of individuals with a vested interest in the regime's survival – in order to counterbalance threats posed by other members of the ruling elite or by the military: because military officers usually avoid interventions they believe would be opposed by popular protests, and because parties make it easier for the dictator to mobilize mass opposition to attempted coups, the existence of a party deters potential coup plotters from intervention. Winning elections with overwhelming majorities, in turn, simply helps dictators demonstrate the strength of the party: elections, she argues, “help to persuade potential challengers that the dictator's party is the only game in town and that there is no point in trying to organize an opposition movement” (Geddes 2006, 5). She consistently finds that authoritarian regimes that rely on a party and hold regular elections tend to last longer than those without them. Magaloni (2006) makes a similar point about the PRI in Mexico: by winning elections with large margins of victory, the PRI was able to deter dissatisfied politicians from defecting and to avoid intra-elite splits. These ideas have received further empirical support in a recent study showing that coups occur more often

after authoritarian elections in which the support for the incumbent has dropped significantly (Wig and Rød 2014).

One interesting implication of this latter approach to authoritarian elections is that electoral outcomes do not have to be credible in order to perform their function: on the contrary, the incumbent's very ability to mobilize voters in elections without real stake is a way to signal his strength. Supermajorities, or, in the case of uncontested elections, high electoral turnout demonstrate that the dictator has full control over the press and the security apparatus, and that he is able to monitor citizens and punish those who abstain or vote against him. Consequently, as I shall argue below, large margins of victory do not only deter members of the ruling coalition from defecting, but also opposition members from rebelling.

By contrast, the two former approaches imply that elections have to be relatively competitive in order to fulfill their purpose: gathering knowledge about the incumbent's extent of mass support, identifying opposition strongholds, or coopting potential challengers require opposition parties (or at least some of them) to be free to organize autonomously and to compete in elections without being subject to significant amounts of repression. Obviously, uncontested elections cannot perform these functions. One of the most striking findings that emerge from Magaloni's (2006) study of the PRI rule in Mexico and Lust-Okar's (2006) analysis of elections in Jordan is that most elections in these countries are held without significant fraud and perform their function precisely because they are competitive. There is hence a sheer incompatibility between the different functions assigned to authoritarian elections: while signaling strength requires the incumbent to be able to secure large margins of victory (which, in turn, requires dramatically limiting the extent of real competition), garnering information about the opposition involves at least some degree of contestation. Which of these strategies is the most effective remains to date an open question.

## 7.2. ELECTION OUTCOMES AND REBELLION: THEORY AND HYPOTHESES

Chacon et al.'s (2011) study raises an interesting paradox in Przeworski's (1999) original formulation of his theory of minimal democracy: if public support for opposition parties amounts to public support for the incumbent, then opposition parties are admittedly more likely to win the next election, but they are also more likely to prevail in an armed confrontation should they choose this option. If, by contrast, the balance of power is asymmetric, opposition parties are better off not rebelling even though they do not have any reasonable chance of attaining political power.

There are some reasons to believe that incumbents run a lesser risk of rebellion if they manage to demonstrate the regime's strength by securing large margins of victory in elections. According to Kuran (1989, 1995), citizens submitted to an authoritarian government tend to misrepresent their feelings about the regime – even if they truly dislike it – as long as the opposition appears weak. The mechanism is self-reinforcing: the more unshakable the regime appears, the more reluctant citizens are to express their discontent publicly, and the more likely they are to manifest overt support for the incumbent. Regimes based on “preference falsification” are susceptible to crumble as soon as a protest is initiated, since more and more people then have an incentive to jump on the revolutionary bandwagon (see also Tullock 2005 [1974]); yet, these regimes tend to last relatively long, and their breakdown typically takes everyone by surprise.<sup>109</sup>

In light of this approach, the stabilizing role of weakly competitive elections becomes obvious: if it is the public expression of discontent that weakens regimes, unpopular incumbents are better off hiding information about the extent of their support, and inciting voters to misrepresent their true preferences. This might also explain why some autocrats run in uncontested elections or claim margins of victory that are not remotely credible (i.e., why holding sham elections better serves incumbents' purposes than holding no election at all). As already noted, the incumbent's ability to mobilize voters in such elections demonstrates the regime's full control on the citizenry: high electoral turnout and broad margins of victory signal not only voters' passive acquiescence, but also their readiness to

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<sup>109</sup> Kuran's (1989) initial study was mainly devoted to the East European revolutions, but the Arab Spring could be considered another obvious example of “revolutionary surprise.”

express active support for the regime.<sup>110</sup> Conversely, if elections serve communication purposes, holding no elections at all might be interpreted as a sign of weakness, or as evidence that the incumbent either has lost the ability to mobilize voters or is reluctant to publicly display his extent of public support.

By contrast, close election outcomes publicly signal that a significant part of the citizenry has withdrawn its support from the incumbent. Consider, for example, the “colored revolutions” in post-communist countries. The popular uprisings that overthrew the Serbian, Georgian, Ukrainian, and Kyrgyz authoritarian regimes from 2003 to 2005 all broke out as a result of fraudulent elections (see Tucker 2007; Tudoroiu 2007; Kalandadze and Orenstein 2009). According to Tucker (2007), these protests occurred precisely because the amount of electoral fraud committed by these regimes solved the collective action problem faced by citizens. The striking pattern is that these elections were, in fact, not only contested but also fairly competitive in terms of outcomes: in Serbia in 2004, the main opposition party claimed 48.2% of the votes by official counts (Tucker 2005, 537). In Ukraine in 2004, both the incumbent and the opposition leader received about 40% of the votes in the first round of the presidential election, and results were similarly close in the second round (Tucker 2005, 538). One observer of the 2005 legislative elections in Kyrgyzstan commented: “Ironically, the parliamentary elections that ultimately led to Akayev’s downfall were probably the freest and fairest that Central Asia has yet seen” (Radnitz 2006, 134). But maybe this is not so ironic after all: the conclusion that post-election protests are more likely to break out after relatively competitive elections might be unwarranted, but the idea would be worth exploring.<sup>111</sup> If, as Przeworski (1999) puts it, voting is “flexing muscles,” it is perhaps not so surprising that these protests broke out after opposition parties had the opportunity to publicly display their public support.

To illustrate the point, Table 7.1. reports the percentage of the votes won by the incumbent or his party in the cases of (successful or failed) “electoral revolutions” analyzed in Kalandadze and Orenstein’s (2009) study on authoritarian elections.<sup>112</sup> The percentage of

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<sup>110</sup> In Londregan and Vindigni’s (2008) framework, such elections are a means to alleviate the consequences of “optimistic biases,” i.e. the fact that both the incumbent and his opponents tend to overestimate their own strength, hence making conflict more likely.

<sup>111</sup> To the best of my knowledge, no empirical study has systematically investigated the relationship between the competitiveness of elections and the occurrence of street protests.

<sup>112</sup> I was not able to find the results of the 1996 local elections in Serbia, the 2004 parliamentary elections in

votes won by the incumbent or his party in these cases ranges from a stunningly low 21% in the Georgian 2004 parliamentary elections to 84.4% in the 2006 presidential elections in Belarus. In spite of this variability, however, most cases are clustered between 40% and 60% – results that would be deemed fairly normal in any established democracy – and in 6 out of 14 elections, the incumbent did not manage to secure the absolute majority of the votes. In one of these cases (the 2001 presidential elections in Madagascar), the incumbent even claimed fewer votes than his challenger. In regard to these patterns, Lukashenko's overwhelming electoral victories are rather the exception than the rule, and do not even amount to the performances of Turkmen president Berdimuhamedov (97% of the vote)<sup>113</sup> or Kazakhstani president Nazarbayev (96% of the vote) in the 2012 and 2011 elections – none of which, needless to say, were reported in Kalandadze and Orenstein's (2009) analysis of electoral revolutions. On the whole, this anecdotal evidence suggests that opposition parties are more likely to protest election outcomes when they believe they were close to winning them.

To sum up, we should expect rebellion to be *ceteris paribus* less likely to break out when the government's party enjoys overwhelming electoral support. Yet, if rebellion requires both motive and opportunity, as Collier and Hoeffler (2004, 563) put it, this hypothesis offers only a partial explanation for why opposition groups revolt: a more complete theory of rebellion would require incorporating motives into the general picture. In other words, if relative power parity were the only explanation for rebellion, we should observe recurring civil wars in most democracies instead of peaceful alternations in power.

One reason why this is not the case is that, in a typical democracy, opposition parties are allowed to sit in the legislature and exert some influence on policies – and, in the case of parliamentary democracies, can even remove the chief executive from office – and can reasonably expect that the incumbent will relinquish power in case they win the next elections. But the extent to which opposition parties are granted effective political power varies from one political system from another: to begin with, electoral systems are more or less proportional, which means that the influence of opposition parties during the incumbent's term varies greatly from one democracy to another. This variation is even

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Belarus, and the 2005 elections in Kyrgyzstan.

<sup>113</sup> Berdimuhamedov's predecessor Niyazov did even better (he managed to win 98.3% of the votes in the 1990 election, and 99.5% of the votes in the 1992 election) but he ran uncontested.

higher when nondemocratic systems are considered. In some countries, such as Myanmar until recently, only a certain share of the seats in the legislature is up for election, the rest of the seats being reserved for appointees. Some other countries hold free and fair elections, but opposition parties are prevented from taking office in case they win: Incumbents refuse to step down, close the legislature, and cancel election results. These are extreme cases, but they are nevertheless not infrequent (see Alvarez et al. 1996). In other words, political parties' electoral support only imperfectly predicts their political power: while the relationship between parties' electoral support and their probability of prevailing in a civil war can be assumed to be independent from the type of political system, the relationship between electoral support and political power is not a constant. Therefore, a more complete theory of rebellion requires taking these sources of variation into account as well.

**Table 7.1. Elections outcomes and electoral revolutions, 1991-2005**

<i>Country</i>	<i>Year</i>	<i>Type</i>	<i>Results 1<sup>st</sup> round</i>	<i>Results 2<sup>nd</sup> round</i>
Armenia	1996	Presidential	51.3	N.A.
Armenia	2003	Presidential	49.48	67.45
Azerbaijan	2000	Parliamentary	62.3	N.A.
Azerbaijan	2003	Presidential	75.38	N.A.
Azerbaijan	2005	Parliamentary	62	N.A.
Belarus	2001	Presidential	77.4	N.A.
Belarus	2006	Presidential	84.4	N.A.
Ethiopia	2005	Parliamentary	59.8	N.A.
Georgia	2003	Parliamentary	21.3	N.A.
Madagascar	2001	Presidential	40.61	N.A.
Peru	2000	Parliamentary	42.2	N.A.
Peru	2000	Presidential	49.9	74.3
Togo	2005	Presidential	60.15	N.A.
Ukraine	2004	Presidential	39.26	49.46

(Sources: Kalandadze and Orenstein 2009; Wikipedia.org)

Such an approach would solve the paradox raised by Chacon et al.'s (2011) study. Chacon

et al. show that, in situations in which electoral support is evenly balanced between incumbent and opposition, conflict is more likely to break out because both parties have a sufficiently high chance of winning. They interpret this as evidence that Przeworski's (1999) original hypothesis – that parties will choose to comply if they anticipate that they have a chance of winning the next elections – is flawed. Yet, because their own empirical analysis is focused on one single case (namely Colombia), variations between political systems are held constant: the fact that, everything else being equal, parties are more likely to opt for violence when they enjoy widespread support does not contradict Przeworski's hypothesis, because the extent to which the share of votes parties obtain in elections predicts their probability of doing well under current institutions varies from one political system to another. In other words, Chacon et al. (2011) demonstrate that opportunities are decisive when motives are held constant; they do not demonstrate that motives are irrelevant.

The argument I put forward is that the extent to which electoral support impacts civil conflict depends on each party's political power under current institutions; conversely, the risk that opposition parties rebel when they are durably excluded from power depends on their level of support. If opposition parties are represented in the legislature and can expect the incumbent to yield office in case he loses the next elections, they will not rebel because they will lack the motive to do so. If, by contrast, they are not represented but do not enjoy strong electoral support either, they have a motive to rebel but lack the opportunity to do so. I expect conflict to be most likely to break out in situations where opposition parties are allowed to display their strength – i.e., to participate in relatively competitive elections – but are prevented from taking office or sitting in the parliament.

The outbreak of the Algerian civil war in the aftermath of the 1991 legislative elections exemplifies this logic. Since independence, Algeria was ruled formally by a single party (the FLN) and de facto by the military (Pierre and Quandt 1995; Mortimer 1996; Viorst 1997). In 1988, the economic crisis induced by the fall of oil prices led to a first episode of social unrest, which was sparked by sharp price increases but conveyed a distinctly religious tone. The then-President Chadli Benjedid reacted by initiating a series of political reforms, among which the abolishment of the single-party system, the legalization of opposition parties, and the organization of the first multiparty elections since

independence. Apparently convinced by the polls that the FLN would retain its majority (Mortimer 1996, 21), the government authorized the main Islamist opposition party (the FIS, officially founded in 1989) to run first in the municipal, and then in the legislative elections held in December 1991. Yet, the military badly underestimated the electoral potential of the Islamists, who won twice as many votes as the FLN in the first round of the legislative elections (Quandt 2002, 16). The incumbents also believed that the electoral system – a top-two runoff system similar to the French one – would help them secure a large majority in the new National Assembly, but instead, given the dispersion of the non-Islamist and non-FLN votes, this system only bolstered the FIS' electoral advantage: of 430 seats in the parliament, the FIS won 188 in the first round and led in 140 of outstanding contests, and it appeared likely to secure a two-thirds majority in the new parliament (see Pierre and Quandt 1995, 135; Mortimer 1996, 25; Viorst 1997, 92).

It appears in hindsight that the military had been willing to let the FIS enter the parliament in order to channel contestation and curb mass dissent, but only under the condition that it remained a minority force (Mortimer 1996, 21): in January 1992, faced with the prospect of a crushing electoral defeat, the military reacted by canceling the second round of the elections, declaring a state of emergency, banning the FIS and arresting its members, and – apparently suspecting that he had made a deal with the Islamists (Pierre and Quandt 1995, 135) – overthrowing Benjedid. Soon after the coup, FIS supporters who managed to escape arrest formed themselves into several armed groups and launched an armed rebellion against the FLN: by then, Algeria was on its way to civil war.

Whether or not these aborted elections were the ultimate cause of the Algerian civil war, they clearly triggered the rebellion. This makes it an instructive case for several reasons. First, there was a lot at stake in these elections: the 1989 Constitution not only gave the National Assembly the right of initiative, but also enabled it to dismiss the Prime Minister by voting a motion of censure.<sup>114</sup> Allowing the FIS to sit in the legislature would have thus clearly decreased its incentives to challenge the regime violently. Second, even though the President could not be dismissed by the parliament, the same Constitution stated that he was to be elected by direct universal suffrage (Art. 71). Since the 1991 legislative elections

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<sup>114</sup> Articles 135 to 137. See [https://www.constituteproject.org/constitution/Algeria\\_2008.pdf](https://www.constituteproject.org/constitution/Algeria_2008.pdf) (accessed May 7, 2015) and [http://confinder.richmond.edu/admin/docs/local\\_algeria.pdf](http://confinder.richmond.edu/admin/docs/local_algeria.pdf) (accessed May 7, 2015). The new regime was semi-presidential.

were the first ones the regime authorized, they were an occasion to observe the incumbent's true intentions: by canceling the second round of the legislative elections, the FLN not only deprived its main opponent of any legal way to pursue its demands, but also clearly signaled that it was not willing to yield office if the opposition won the presidential elections. Third, a more cynical way of interpreting the events would be to suppose that, if the FLN had somehow managed to secure more votes, prevented the FIS from participating in the elections, or not held any elections at all, the war could have been avoided.

I thus test the following hypotheses:

*H1: Rebellion becomes ceteris paribus less likely as the incumbent's share of votes increases.*

*H2: Rebellion becomes ceteris paribus less likely as the ruling party's share of seats in the parliament decreases.*

### **7.3. EMPIRICS: PARLIAMENTARY ELECTION OUTCOMES AND DOMESTIC CONFLICT**

#### **7.3.1. Data and methods**

Data on the dependent variable (civil conflict outbreak) are drawn from the UCDP/PRIO Armed Conflict Dataset. The measure is the same as in Chapter 5: the variable takes on the value of 1 if a new civil conflict breaks out in the country-year, and subsequent conflict years as well as instances of coups are coded 0.

Data on the two independent variables (i.e., the share of the votes government parties<sup>115</sup> won at the last parliamentary election, and their share of seats in the legislature) are drawn

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<sup>115</sup> I use the share of votes and share of seats of the government party rather than the ones of the largest opposition party for theoretical reasons: In authoritarian regimes, the opposition is often fragmented (see, e.g., Lust-Okar 2004), which artificially deflates the opposition's electoral outcomes and misrepresents the real balance of forces. Yet, even if opposition parties are fragmented, they can still exert some influence in the legislature if the government's party does not obtain the absolute majority of the seats, because they can collectively veto laws or (in parliamentary and semi-presidential regimes) remove the chief executive from office.

from the Database of Political Institutions (Beck et al. 2001), and are available from 1975 onward. For each country-year, the outcomes of the last parliamentary election are reported: in election years, the outcomes reported are those of the previous election.

Table 7.2. displays descriptive statistics: the share of votes won by the incumbent’s party is reported in the first row. The second row displays the share of seats of the chief executive’s party in the legislature. Both variables display a high variation and range from less than 10% to their maximal value 100%. Unsurprisingly, the share of seats in the legislature obtained by the chief executive’s party tends to be higher on average than its share of votes (64% vs. 72%).

The theoretical expectations and the formulation of the hypotheses would require including the share of votes and the share of seats of government parties in the same model. Yet, both variables are obviously highly correlated (0.93). To avoid introducing unnecessary multicollinearity in the models, I compute the difference between both variables (hereafter SE-VO) and use it as independent variable. SE-VO theoretically ranges from -100 to 100: Positive values indicate that the government parties are overrepresented in the legislature (relative to their vote share), negative values indicate that opposition parties are overrepresented. Scores close to zero indicate that the share of seats of the government party closely matches its share of votes. I therefore expect a positive relationship between SE-VO and armed conflict outbreak. Descriptive statistics on SE-VO are displayed in the last row of Table 7.2.

**Table 7.2. Parliamentary election outcomes, 1975-2008**

	<i>Mean</i>	<i>Std. deviation</i>	<i>Min.</i>	<i>Max.</i>	<i>N</i>
% votes	64.30	23.90	2.23	100	3,075
% seats	72.41	22.44	9.28	100	4,303
SE-VO	6.71	9.01	- 32.86	54.28	3,065

(Source: Database of Political Institutions)

The mean of SE-VO is – as expected – positive, but relatively low: On average, the share of seats of government parties only exceeds its share of votes by about 7 percentage points.

Yet, SE-VO also exhibits a high variance, and ranges from -32.86 (Central African Republic, 2006-2008) to 54.28 (Nicaragua, 1993-1994).<sup>116</sup>

Finally, the empirical analysis includes a small set of control variables (see Table 7.3. for the descriptive statistics): I control for per capita income using data from the Maddison Project Database (Bolt & van Zanden, 2013), population using data from Ross (2013), and ethnic fractionalization using data from the Ethnic Power Relations dataset (Cederman, Wimmer & Min 2010). I also add a cold war dummy which takes on the value of 1 from 1946 to 1990, and a variable measuring the time since the last incidence of an intrastate armed conflict, build from the UCDP/PRIO data. I use logit analysis, with robust standard errors clustered by country. Next section presents and discusses the results.

**Table 7.3. Descriptive statistics: GDP per capita, population, and ethnic fractionalization, 1975-2008**

	<i>Mean</i>	<i>Std. deviation</i>	<i>Min.</i>	<i>Max.</i>	<i>Observations</i>
ln(GDP/cap.)	8.06	1.12	5.33	10.45	4,927
ln(Population)	15.99	1.56	12.05	21.00	5,121
ELF	0.42	0.31	0	1	4,261

(Sources: Maddison Project database; Cederman et al. 2010; Ross 2013).

### 7.3.2. Results

Results of the logistic regression can be seen in Table 7.4. below. Model 1 displays the impact of SE-VO on the risk of civil conflict onset: as expected, the effect of the variable is positive. The magnitude of the effect is moderate (for each one-unit increase of the variable, the odds that a country experiences an intrastate armed conflict increase by approximately 3%) but nevertheless non-negligible given that SE-VO ranges from -32 to 54. The effect is significant at the 5% level: So far, this result corroborates theoretical

<sup>116</sup> With regard to the Central African Republic, the DPI reports 80.94% of the votes in favor of the chief executive's party, and 48.08% of the seats in the legislature. The Nicaraguan ruling party won 40.84% of the votes and obtained 95.12% of the seats in the legislature, according to the same data source.

expectations.

In Models 2 and 3, I replace SE-VO with the share of seats of government parties and their share of votes, respectively. Neither of these variables has any discernible impact on its own, though: as expected, when analyzed separately, neither the electoral support for government parties nor the extent of their control on political institutions has any effect on the risk of intrastate armed conflict.

On the whole, control variables behave consistently across the three models (with the exception of peace duration, which is only significant in Model 2) and corroborate prior findings: intrastate armed conflicts are more likely in highly populated, ethnically heterogeneous countries, and tend to occur more frequently since the end of the Cold War. Yet, although GDP per capita displays the expected sign, it fails to attain statistical significance in all three models.

**Table 7.4. Parliamentary election outcomes and civil conflict onset, 1975-2005 (logistic regression, reporting odds ratios)**

	<i>Model 1</i>	<i>Model 2</i>	<i>Model 3</i>
Peace duration <sup>a</sup>	0.991 (0.008)	0.973*** (0.008)	0.990 (0.008)
SE-VO	1.028** (0.014)		
% seats		1.006 (0.007)	
% votes			1.003 (0.006)
ELF	5.219*** (2.793)	4.281*** (1.906)	4.726*** (2.457)
GDP/capita <sup>a, b</sup>	0.769 (0.126)	0.914 (0.133)	0.795 (0.128)
Population <sup>a, b</sup>	1.618*** (0.134)	1.453*** (0.141)	1.634*** (0.147)
Cold War	0.507** (0.156)	0.590** (0.133)	0.497** (0.154)
Constant	3.80e-05*** (6.72e-05)	7.26e-05*** (0.000)	2.79e-05*** (5.46e-05)
Countries	137	150	137
Observations	2,508	3,491	2,517

Robust standard errors in parentheses.

\*p < 10%; \*\*p < 5%; \*\*\*p < 1%.

<sup>a</sup>Lagged one year; <sup>b</sup>Log

I now check for the sensitivity of the results reported in Table 7.4., Model 1. I first run a probit model, a fixed effects logit model, and a rare event logit model (not displayed here). In the probit and rare event logit models, the independent variable of interest remains

statistically significant and positively related to the outbreak of intrastate armed conflict. When country and year fixed effects are included, estimates regarding SE-VO actually improve substantially: the variable is now significant at the 1% level and is found to increase the odds of civil conflict onset by a factor of 1.13 when it increases by one-unit (which means that, for each additional percentage point of difference between the share of votes and the share of seats of the government party, the risk of conflict increases by 13%). Note, however, that a large number of observations have been excluded from the analysis because of multicollinearity and perfect prediction of the 0 outcome: there are only 717 observations (42 countries) left in the model. All in all, however, these results indicate that the main findings are robust to a number of alternative model specifications.

I then check whether the results are entirely driven by a small number of cases that display extreme values on both independent variables: I rerun the model after excluding cases in which the government party controls more than 80% of the seats in the parliament, and cases in which it received fewer than 20% of the votes. In both cases, results remain unaffected. Interestingly, however, SE-VO loses its significance either when the government party does not have the absolute majority of the seats in the parliament or when its share of votes exceeds 35%. This indicates that, when the government party either enjoys widespread electoral support or does not monopolize seats in the parliament, the discrepancy between political power and electoral support does not matter: conflicts simply do not happen in these cases.

Finally, I investigate whether these findings pertain to all types of intrastate armed conflict or only to a subset of these: I rerun the model first with conflicts over government, and then with territorial conflicts as dependent variables. Results are displayed in Table 7.5.

Unexpectedly, SE-VO has no discernible effect on intrastate conflicts over government (Model 4): the change in the odds is negligible and the variable fails by far to attain statistical significance. By contrast, SE-VO has a strongly significant and relatively high effect on the risk of territorial conflict: each one-unit increase of SE-VO increases the risk of territorial conflict by roughly 5%, and the variable is significant at the 1% level. Why election outcomes have a stronger impact on territorial conflicts than on conflicts over government is rather mysterious. Yet, this finding lends some empirical support to the idea that territorial conflicts are not solely caused by cultural factors, and that they can be

avoided by a better system of representation.

Results regarding control variables are less surprising. What foremost seems to predict the onset of intrastate conflicts over government is poverty (see Collier and Hoeffler 2004): a one-unit increase in the log of GDP per capita almost halves the risk of civil conflict outbreak in Model 4. These conflicts are also influenced by large populations and a high ethnic fractionalization. Yet, both population and ELF display a stronger impact on conflicts over territory (Model 5): a one-unit increase in the natural log of population almost doubles the risk of territorial conflict, and when ELF increases from 0 to 1 (i.e., from its minimal to its maximal value), the odds of a country experiencing a territorial conflict increase by a factor of 14. By contrast, GDP per capita does not seem to impact the onset of territorial conflict.

**Table 7.5. Parliamentary elections outcomes and the onset of conflicts over government and territory, 1975-2005 (logistic regression, reporting odds ratios)**

	(4) <i>Government</i>	(5) <i>Territory</i>
Peace duration <sup>a</sup>	1.002 (0.012)	0.977* (0.013)
SE-VO	1.010 (0.0213)	1.051*** (0.016)
ELF	3.017* (1.846)	14.05** (15.18)
GDP/capita <sup>a, b</sup>	0.566*** (0.108)	1.309 (0.390)
Population <sup>a, b</sup>	1.328** (0.156)	1.995*** (0.306)
Cold War	0.553 (0.251)	0.412** (0.176)
Constant	0.008** (0.020)	3.15e-09*** (1.06e-08)
Countries	137	137
Observations	2,508	2,508

Robust standard errors in parentheses.

\*p < 10%; \*\*p < 5%; \*\*\*p < 1%.

<sup>a</sup> Lagged one year; <sup>b</sup> Log

#### 7.4. CONCLUSION

There is a persistent debate regarding the impact of electoral competition on the stability of political regimes. On the one hand, if the incumbent's party repeatedly wins elections by large margins of victory, opposition parties – knowing that they do not have any fair chance of attaining political power by legal means – face a strong incentive to rebel. On the other hand, when electoral support is evenly distributed between ruling party and opposition parties, the latter's chance of prevailing in a civil war clearly improve. When and why, then, do opposition parties rebel?

My findings lend support to both views. Opposition parties are deterred from rebelling when the incumbent wins elections by large margins, and they are incited to comply when they know the incumbent would relinquish power if they won the next elections. In short, rebellion does not occur when the political influence opposition groups are granted under the current institutions roughly corresponds to their level of electoral support. If this is not the case, opposition parties are better off rebelling than complying and conflict will ensue.

In a certain way, these results support some of the arguments that have been made about “hybrid” or “anocratic” regimes (see, e.g., Muller and Weede 1990; Hegre et al. 2001; Regan and Henderson 2002; Regan and Bell 2010): prior research has characterized these regimes as inherently unstable because they allow opposition groups to mobilize popular support without giving them the possibility to pursue their demands in a regular, institutional way. Although the “anocracy hypothesis” has been seriously challenged in recent years (see Vreeland 2008; Regan and Bell 2010), there seems to be some truth in the underlying argument that democracies are generally able to decrease the desirability of rebellion and autocracies to increase its costs or limit the opposition's opportunities to challenge the regime.

Finally, these findings shed light on how and why elections can contribute to the stability of political regimes. In particular, for authoritarian elections to perform their stabilizing function, incumbents must carefully decide how free and fair these elections should be, how much competition they want to allow, and to which extent they are ready to grant opposition parties a real political influence in case these perform well at the polls. In this regard, the worst possible strategy consists in allowing opposition parties to display their

electoral strength while depriving them of any real possibility to gain political influence. At the same time, however, these findings beg further questions, to the extent that, while cases of huge disproportionality between parties' share of votes and their share of seats are clearly destabilizing, they are not exceptional. Thus, one interesting avenue for future research would be to investigate whether these cases always result from a political miscalculation – as in the Algerian case discussed above – or whether they convey some strategic benefits that outweigh the associated risks.

**PART III:**  
**POLITICAL REGIMES AND THE STRATEGIC USE OF POLITICAL VIOLENCE**

## 8

### **Revisiting the moral hazard in authoritarian repression: The role of the military**

*“You can do anything with bayonets except sit on them.”*

*Thomas Hardy.*

A gap between qualitative and quantitative scholarship on the military’s participation in domestic repression has appeared in recent years. While recent qualitative scholarship has widely acknowledged that the military’s support for authoritarian regimes should not be taken for granted and that defections are by no means rare (D’Anieri 2006; Hall 2010; Barany 2011, 2013; Pion-Berlin et al. 2014), this idea is still relatively absent from cross-national comparative studies on the topic: with a few exceptions (e.g., Geddes 1999), many authoritative works on political transitions tend either to treat the military as a passive agent of the ruling elite or not to address the question at all (e.g., Boix 2003; Acemoglu and Robinson 2006). This sometimes even pertains to studies that explicitly deal with the impact of repression on the political survival of dictators (e.g., Escriba-Folch 2013a) or on the regime’s prospects for democratization (e.g., Albertus and Menaldo 2012). The few authors who have devoted attention to the role of the military in political transitions have been primarily concerned with what McMahon and Slantchev (2015) label the “guardianship dilemma,” i.e., the fact that incumbents’ reliance on coercion gives the armed forces sufficient power to impose their own preferences and possibly overthrow the

current regime (Acemoglu et al. 2008; Besley and Robinson 2009; Svobik 2012). Some studies have more directly tackled the issue of the moral hazard in state repression, but they mostly deal with situations in which a lack of political control on repressive agents results in more severe human rights violations (see, e.g., Butler et al. 2007; Conrad and Moore 2010; Cingranelli et al. 2014).

Thus, our understanding of the military's attitudes toward domestic repression remains so far fragmented. This issue is nevertheless fundamental. State repression is often thought of as one of the most important "pillars" (Gerschewski 2013) on which autocrats rely to maintain their rule: Yet, whether repression "works" or not – that is, whether it is an effective way of ensuring the regime's survival in the last resort – is still an open question. Shifting the focus toward the agents in charge of carrying out repression thus represents an important first step toward understanding how authoritarian regimes break down, because when the incumbent has lost popular support and mass dissent breaks out, the armed forces are the last bulwark against the regime's downfall. Knowing whether or not military officers are likely to refuse to obey orders to repress and turn against the incumbent is thus crucial for understanding dynamics of regime transition and leadership changes in dictatorships.

The basic idea I put forward is that what makes repression especially risky is a potential mismatch between the preferences of incumbents and the preferences of the military: the moral hazard in state repression arises from the fact that, while repression is an attractive option for incumbents who have lost popular support, the agents in charge of carrying it out – that is, members of the armed forces – are generally hostile to it. Yet, this idea is at odds with one dominant assumption in the quantitative literature on state repression, that militaries tend to favor the use of force over peaceful conflict resolution, either because they do not know other ways of reacting to political conflicts or because they have an incentive to push for repressive policies in order to increase their own influence on the political system (see Davenport 2007b, 487). Furthermore, if military officers are indeed reluctant to carry out domestic repression, it becomes quite mysterious why military dictators tend to make a more extensive use of coercion than civilian autocrats (Poe et al. 1995; Davenport 2007b).

In the following sections of this chapter, I will empirically analyze two aspects of the

problem sketched above. I will first examine whether state repression – conceived here as physical integrity rights violations perpetrated by state actors – affects coup risk: this will be, to the best of my knowledge, the first cross-national statistical analysis on this topic. Second, I will test whether systems in which the influence of the military on politics is maximized – that is, military dictatorships – are more coercive than regimes ruled by civilians.

The chapter is organized as follows. Next section will review prior works on the role of the military in state repression. Section 8.2 will discuss some theoretical expectations and present the working hypotheses. Section 8.3 will deal with some general issues in the measurement of state repression, and will discuss the consequences of these measurement issues for the study of regime type and repression. In Section 8.4, I then proceed to test my hypotheses. Section 8.5 concludes.

## **8.1. WHAT DO WE KNOW ABOUT THE MILITARY’S ATTITUDE TOWARD DOMESTIC REPRESSION?**

Early analysts of civil-military relations have repeatedly emphasized the deep reluctance of military officers to participate in domestic repression. For example, Needler (1975, 70) mentions the desire to maintain good relations with the civilian population as one of the core institutional interests of the military and notes that governments’ attempts to use the military as a repression force has often led to their overthrow. Finer (2009 [1962], 27) notes that this is one of the reasons why professionalization may sometimes lead to military intervention:<sup>117</sup> As he sums up, for professional soldiers, “the foreigner is the enemy, not a fellow national.” Nordlinger (1977) has a more cynical interpretation – “officers see themselves as professional and heroic managers of violence,” he says, and using force against their fellow citizens “impugns their professional standing, martial qualities, and heroic image” (p. 54-5) – but also stresses officers’ aversion to be used to coerce domestic opponents.

These considerations are relatively absent from the recent quantitative scholarship on both

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<sup>117</sup> Military intervention refers here to military intervention in domestic politics, not to, e.g., foreign military intervention in civil wars.

repression and military coups (for a notable exception, see Geddes 2006). On the one hand, the military's involvement in domestic repression is practically never mentioned as a possible determinant of military intervention in cross-national comparative analyses of coups, even when these studies include the current regime's legitimacy in their list of determinants (e.g., Belkin and Schofer 2003). On the other hand, while numerous studies have investigated the effectiveness of repression, these studies were either concerned with the impact of repression on mass dissent (see, e.g., Lichbach 1987; Gupta et al. 1993; Moore 1998; Regan and Norton 2005; Carey 2006; Davenport et al. 2008; Bell et al. 2013; Young 2013) or with its consequences for broader issues such as dictators' survival in office (e.g., Escriba-Folch 2013a) or democratization (e.g., Albertus and Menaldo 2012). In most of these studies, the interests and preferences of the agents in charge of carrying out repression is still the unknown in the equation.

Some other studies have devoted more attention to principal-agent issues in domestic repression. First, Acemoglu et al. (2008) as well as Svobik (2012) argue that states that overly rely on repression face a moral hazard: the very material resources and institutional autonomy that allow the regime's repressive agents to participate in internal repression also provide them with leverage they can exploit for their own benefit, and enable them to turn against the regime itself. Second, some empirical studies have investigated how agency loss might lead to increased levels of state-sponsored violence (see, e.g., Butler et al. 2007; Bohara et al. 2008; Englehart 2009; Conrad and Moore 2010; Cingranelli et al. 2014). While these works explicitly take the preferences of repressive agents into account, these agents are either assumed to act by pure opportunism or to have a stronger preference for violence than their principals.

A relatively straightforward way of identifying the military's preferences regarding the use of domestic repression is to observe how it behaves once in power. Two cross-national comparative studies (Poe et al. 1999; Davenport 2007b) have investigated whether military-led regimes are systematically more prone to repression than civilian ones: both find it to be the case. The explanations offered are somewhat different: Poe et al. state that "political leaders with direct control of the instruments of coercion (...) will face fewer barriers than other leaders if they choose to act repressively"<sup>118</sup> (Poe et al. 1999, 293) while

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<sup>118</sup> The argument is not entirely clear, however. The sentence seems to imply that the most serious "barrier"

Davenport's (2007b) argument relies on the idea of "coercive expertise": in short, actors who are familiar with coercion are more likely to use it out of habit, and less likely to pursue alternative strategies that would decrease their own influence on the decision-making process. In spite of these divergences, both studies directly link the use of repression to the preferences of the military.

As already noted, these works contrast sharply with early research on military coups, but also with more recent qualitative scholarship: a growing body of qualitative, case studies-based research on military behavior during political crises has developed in recent years, based on the finding that revolutions usually do not succeed without the active support (or at least the neutrality) of the military. Indeed, such defections are by no means exceptional: the military's refusal to repress protesters and the withdrawal of its support for the government – if not its active support for the revolution – has been the ultimate cause of the regime's demise in cases as various as Venezuela in 1958 (Karl 1987), Romania in 1989 (Barany 1997; Watts 2001; Hall 2010), Mali in 1991 (Turriffin 1991; Moestrup 1999), or Indonesia in 1998 (Lee 2005, 2009). More recently, the Arab Spring has highlighted the crucial role played by the military for the survival or breakdown of a regime in revolutionary situations. In all of the countries in which mass unrest broke out, the incumbent ordered his army to repress the protests; the outcome of the protests in each country ultimately depended on whether generals accepted or refused to carry out the order (Barany 2011). For instance, in Tunisia and Egypt, the army backed the protesters, and the regime fell; in Libya, the army split and civil war ensued; in Bahrain, where the army supported the regime, the revolution failed (see also Nepstad 2013; Croissant 2013a). D'Anieri (2006) makes a similar point about the role of the security forces (including the military) in determining the outcome of the "colored revolutions" in post-communist countries: revolutions failed wherever the security forces remained loyal to the regime, and succeeded in cases where they not only refused from repressing demonstrations, but also refrained from taking preemptive measures and even sometimes assisted the protesters. For these reasons, qualitative research has become increasingly concerned with the factors that may lead the military to defect from the regime. Possible causes of defections include

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leaders could face if they choose to repress is resistance from the military itself. But if the military is willing to resist orders to repress when it is not in power, why should it repress more than civilians when it directly rules the country?

the military's own grievances (Karl 1987, Barany 2013, Pion-Berlin et al. 2014), its lack of internal cohesion (Lee 2005, 2009; Barany 2011, 2013; Lehrke 2014), its treatment under the current regime and/or the extent to which it would be better off under another regime (Lee 2009; Lutterbeck 2013; Nepstad 2013), or the relative domestic or foreign support for the regime and the opponents (Barany 2011, 2013; Nepstad 2013). Most of these approaches are either related to the military's self-interest in the breakdown of the regime – thus viewing the defection as an elite-driven, top-down process – or to its inability to react to the threat in an effective way because of specific organizational dysfunctions (e.g., Lee 2005, Lehrke 2014). However, some studies also specifically stress the military's reluctance to be used to coerce domestic opponents. In their comparative analysis of cases of military disobedience in face of mass unrest, Pion-Berlin et al. (2014) argue that the military ethos (i.e., the military's perception of its role and duty) had a decisive influence on the decision to follow or disobey orders to repress the protests: militaries that professed a duty to the people rather than to the regime and felt that internal security tasks were not part of their professional obligations were more likely to refuse orders (see also Lutterbeck 2013). Nepstad (2013) similarly stresses the moral conflicts that may arise when troops are ordered to fire at nonviolent protesters, and the way opponents can exploit these moral dilemmas to encourage loyalty shifts. The impact of such factors has been regularly confirmed by individual case studies: for example, Taylor (2001, 2003) attributes the Russian military's repeated refusal to get involved in domestic repression to its deep commitment to the norm of civilian supremacy and its reluctance to fulfill internal security tasks. Hall (2010) provides a thorough account of the Romanian military's behavior during the uprisings that ultimately led to Ceaușescu's downfall in 1989: after soldiers repeatedly refused to carry out the order to suppress demonstrations by force – and even, in some cases, joined the protests – the army's high command rapidly decided to withdraw, and ultimately turned against the regime. In Hall's account, the defection process unfolded bottom-up rather than top-down: because the army's rank-and-file were conscripted, had strong ties with the population and share the same concerns as protesters, they rapidly proved unable to carry out their task and the high command had no choice but to take them off the streets.

To sum up, prior research has either focused on the impact of coercive capacities on the

risk of military *intervention* (Svolik 2012) or on the extent to which the military's unwillingness to repress may lead to defections (e.g., Pion-Berlin et al. 2014) or military *arbitrations*<sup>119</sup> (e.g. Hall 2010, Barany 2011, 2013), whereas the impact of the use of repression on the risk of military intervention has been comparatively neglected.<sup>120</sup> Likewise, coups as a consequence of coercion have been widely overlooked by large-N studies on the repression-dissent nexus. I attempt to fill this gap by providing a theoretical account on why, even in the absence of serious challenges to the regime's authority, the risk of coup increases when the army gets involved in domestic repression.

## **8.2. THE MORAL HAZARD IN STATE REPRESSION REVISITED**

### **8.2.1. Agency loss: Understanding when and why the military resists orders to repress**

In line with prior research (Butler et al. 2007; Conrad and Moore 2010; Acemoglu et al. 2008; Svolik 2012), I conceive the relationship between the ruling elite and the military as a principal-agent relationship. However, I depart from these works to the extent that I do not assume that repressive agents have a systematic preference for more repression, or that they are primarily interested in maximizing their own influence on domestic politics. As discussed above, when the military is ordered to carry out regime maintenance tasks, it can either obey orders, disobey by remaining neutral, or back the opponents and overthrow the government. With regard to repression, the military's preferences can also diverge from those of the ruling elite: when ordered to repress opponents, the military can repress more than asked, but it can also repress less. Conventional wisdom suggests that the military often has a preference for more repression, because bargaining does not belong to its organizational culture and because it enjoys a comparative advantage in repression: because the use of violence is a part of the role of the army, it is often viewed as its default

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<sup>119</sup> I borrow Taylor's (2001) useful distinction between military intervention and military arbitration. Whereas military interventions refer to an independent decision taken by the military to attempt to unseat the executive by force, military arbitration occurs when the state's or the government's authority is seriously challenged and the army is forced to choose whose orders to obey.

<sup>120</sup> A notable exception is Finer (2009 [1962], 27), who views the military's reluctance to be used to repress protests as one of the reasons why, contrary to one commonly held belief, military professionalization may actually lead to interventions.

mode of reaction (see e.g. Davenport 2007b; Fjelde 2010, 200). However, the military's collective identity as an actor specialized in the use of violence says nothing about the content of its preferences when it comes to the actual use of force: the only implication that can be drawn is that, military officers being the ones that are in charge of carrying out repression, their preferences may diverge strongly from those of the principal, because they might have different information about their own coercive capacity or hold different beliefs about the necessity or the feasibility of coercive action. Furthermore, the military itself can be affected by its own internal principal-agent problems: regardless of high-ranking officers' own preferences, their ability to effectively carry out orders to repress ultimately depends on middle-level officers' and recruits' readiness to obey, because it is in their interest to maintain the cohesion of the military and to avoid a complete breakdown of the chain of command.

The workers strike in Poland in 1970 illustrates the internal problems the military may face when it gets involved in domestic repression.<sup>121</sup> In December 1970, workers at the shipyards in the region of Gdansk went on strike to protest against changes in the prices of consumer goods. The government quickly sent the army to support internal security forces and bring back order. However, the protests escalated rapidly and spread throughout the country after the army opened fire at workers in Gdynia. The crisis ultimately resulted in 45 deaths and more than 1000 wounded.

Although the strike was successfully repressed and order was re-established a few days later, the incident had important consequences for both the military and the regime. The violence against the civilian population jeopardized the internal cohesion of the military and profoundly demoralized military units involved in the crisis: even if the military chain of command did not break down during the events, there is evidence that several army units refused to use their weapons despite orders, and that some others had to be withdrawn from the operations because of their unwillingness to fire at strikers. Some officers also publicly protested against the military's involvement in the repression of the strike. In the long term, the events cast doubts on the military's effectiveness in carrying out repression: as the regime faced renewed unrest in 1976, party leaders refrained from sending the army to maintain order and opted for policy concessions instead. The then Defense Minister

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<sup>121</sup> For a thorough discussion of the events and the military's involvement in the crisis, see Fajfer (1993).

Jaruzelski reportedly justified this decision by saying that “Polish soldiers would never fire at Polish workers” (Fajfer 1993, 225). From the perspective of the regime, the events provoked a deep crisis within the party apparatus and ultimately led to First Secretary Gomulka’s resignation in December 1970. Although the military did not directly challenge the authority of the regime or the party during the crisis, the withdrawal of the military’s support for Gomulka was decisive for the leadership change.

The Polish example illustrates why, under certain circumstances, the military high command may be strongly opposed to the military’s involvement in domestic repression. This preference is not necessarily motivated by purely altruistic reasons, however: as stated above, the military high command may have its own preferences regarding the use of repression, but its decision to carry it out is ultimately dependent on whether middle-level officers and rank and files are willing to perform the task, and whether the military’s involvement in domestic repression is susceptible to jeopardize its internal cohesion. The decision to intervene or not is subject to the same type of constraints. High-ranking officers may have a vested interest in the survival of the regime and prefer not to intervene; however, as soon as one part of the military signals its intention to defect, they often have no choice but to go along with the plotters. For example, there is some evidence that the 1960 military coup in Turkey was partly caused by the use made of the army to repress protests (Harris 2011, 4; Finer 2009 [1962], 27; Göktepe 2000, 144-145). The coup was initially staged by junior officers – mostly opposition sympathizers – without active involvement from the military high command. General Gürsel, who later assumed the function of head of state, was subsequently chosen to lead the coup – mainly because he was popular throughout the armed forces – but did not take an active part in its preparation. Other high-ranking officers were recruited on a later stage, in spite of their initial loyalty to the regime, because coup plotters wanted to preserve the façade of a cohesive, united military.<sup>122</sup>

The reasons why even loyal members of the military might want to go along with coup plotters for the sake of the military’s unity are detailed in Geddes’ (1999) analysis of military interventions. First, as stated above, officers are always worse off when the military splits. Second, when a coup is staged, they are also always worse off if they

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<sup>122</sup> For a detailed account of the coup, see Göktepe (2000).

remain passive, regardless of the outcome of the intervention: if the coup fails, the government is likely to respond with coup-proofing measures that would further weaken the military; if the coup succeeds, officers who remained loyal to the regime run the risk to be punished for their defection (Geddes 1999, 126-8; see also Tullock 1987).

### **8.2.2. Some expectations**

To sum up, the argument presented here departs from prior research inasmuch as it concerns military intervention rather than arbitration, and relates to repression itself rather than its causes (e.g., domestic turmoil) or its externalities (e.g., resource endowment or coercive capacities). I hypothesize that the actual use of repression increases the likelihood of a military coup. As noted above, this hypothesis contradicts two commonly held beliefs about the military's preferences about repression and intervention: first, that coup plotters act by greed or opportunism; second, that the military intervenes when it perceives the current government as too weak or unable to maintain order in the face of mass unrest (see, e.g., Nordlinger 1977). The empirical analysis should help decide between those contradictory expectations: if the military has more interest in repression than its principals, the effect of repression on military coups should be negative. If military interventions are motivated by greed or opportunism, this effect should be nonexistent. If, however, the military prefers to avoid being involved in domestic repression, this effect should be positive, even after controlling for coercive capacities and domestic instability.

*Hypothesis 1: Repression increases the risk of a military coup.*

A positive relationship between repression and coups would provide tentative evidence that the military on the whole is not systematically more prone to coercion than civilian leaders. One problem with this idea is that it is flatly contradicted by the finding that regimes led by military officers tend to be more coercive than civilian autocracies (Poe et al. 1999; Davenport 2007b). Yet, the studies by Poe et al. (1999) and Davenport (2007b) both suffer from methodological problems that cast some doubts on the validity of this finding: both are restricted to a relatively short period of time (1976 to 1993 and 1976 to 1996,

respectively) and rely on the same measure of state repression, namely the Political Terror Scale (Poe and Tate 1994; Wood and Gibney 2010). While the Political Terror Scale offers some advantages in terms of scope and coverage, one of its drawbacks is that it also measures abuses perpetrated by non-state actors, which makes its use problematic for the purposes of assessing the impact of domestic institutions on state terror. I thus re-examine the relationship between military rule and coercion with another measure of physical integrity rights violations: I expect no systematic difference between military dictatorships and other types of dictatorships.

*Hypothesis 2: Military regimes do not repress more than other authoritarian regimes.*

Another potential issue is that, even if a relationship between coercion and coups can be identified, there are at least two alternative hypotheses that could explain this empirical association. First, as Acemoglu et al. (2008) and Svobik (2012) have pointed out, heightened repression means higher resource endowment for the military and thus better opportunities to attain power. Second, the primary motivation of coup plotters could be a desire to avoid being prosecuted for participating in coercion in case the regime breaks down. Ruling out these alternative hypotheses is no easy task, since the motivations of coup plotters cannot be gauged directly. Yet, a further, more indirect test could make it possible to decide between the three options: the hypothesis I put forward implies that every ruler runs a higher risk of being overthrown if he overly relies on repression, regardless of the type of regime. By contrast, the first alternative hypothesis – that coercion increases the military’s opportunity to attain power – implies that repression should increase coup risk when the military is not already in power, that is, in regimes led by civilian leaders: if the armed forces are in power, it can be reasonably assumed that they already enjoy full control of the political system and that repression does not change anything about it. Finally, the second alternative hypothesis – that military officers want to avoid prosecution – implies, on the contrary, that repression should increase coup risk only if the country is ruled by the armed forces (the idea being that military members who participated in domestic repression are less likely to be prosecuted if they can plausibly claim that they did not issue the order). While this final test would not settle the question in

a definitive way, it would at least provide some preliminary support for one of these potential mechanisms.

*Hypothesis 3: Repression always increases coup risk, regardless of whether the regime is a military dictatorship or a civilian regime.*

Next sections discuss available measures of state repression and present the data and methods.

### **8.3. MEASURING STATE REPRESSION**

#### **8.3.1. Cross-national data on state repression: The Cingranelli and Richards Physical Integrity Rights index and the Political Terror Scale**

Two comprehensive large-N datasets on physical integrity rights violations are currently available. The first one is the Political Terror Scale (hereafter PTS), which covers more than 180 countries from 1976 to present and measures the levels of state-sponsored political violence that a country experiences in a particular year (Wood and Gibney 2010). “State terror” is defined as “violations of physical or personal integrity rights carried out by a state (...) and includes abuses such as extrajudicial killings, torture or similar physical abuse, disappearances, and political imprisonment” (ibid., 369).<sup>123</sup> These dimensions of personal integrity rights violations are aggregated into a single scale that ranges from 1 (very low number of abuses) to 5 (very high number of abuses).<sup>124</sup> The coding is based on

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<sup>123</sup> Because of the vagueness of this formulation, doubt remain whether state terror is limited to this list of human rights violations or can include other types of violence.

<sup>124</sup> Levels of state terror are defined as follows. Level 1: “Countries . . . under a secure rule of law, people are not imprisoned for their views, and torture is rare or exceptional... Political murders are extremely rare”; Level 2: “There is a limited amount of imprisonment for nonviolent political activity. However, a few persons are affected; torture and beating are exceptional... Political murder is rare”; Level 3: There is extensive political imprisonment... Execution or other political murders and brutality may be common. Unlimited detention, with or without trial, for political views is accepted”; Level 4: “The practices of Level 3 are expanded to larger numbers. Murders, disappearances, and torture are part of life... In spite of its generality, on this level terror affects primarily those who interest themselves in politics or ideas”; Level 5: “The terrors of Level 4 have been extended to the whole population... The leaders of these societies place no limits on the means or thoroughness with which they pursue personal or ideological

two annual sources: the US Department of State *Country Reports on Human Rights Practices* and the Amnesty International *Annual Report*.

The second one is the physical integrity rights index of the Cingranelli and Richards (CIRI) Human Rights Data Project (Cingranelli and Richards 2010), which covers more than 190 countries from 1981 to present. Physical integrity rights are defined as “the rights not to be tortured, extrajudicially killed, disappeared, or imprisoned for political beliefs” (ibid., 397). Each of these types of physical integrity rights violations is first coded on an ordinal scale that ranges from 0 (meaning that this practice occurred frequently in the country-year) to 2 (no instance of this practice). When numeric estimates are available, the coding is based on the frequency of each type of violations (a score of 2 would indicate no violation at all, a score of 1 would indicate up to 49 cases of violations, and 0 would indicate more than 50 cases of violation).<sup>125</sup> The four indicators are then added into an index that ranges from 0 (no respect for any of these rights) to 8 (full respect for all of these rights) (ibid., 407).

### **8.3.2. Physical integrity rights violations and political regime type: Some descriptive patterns**

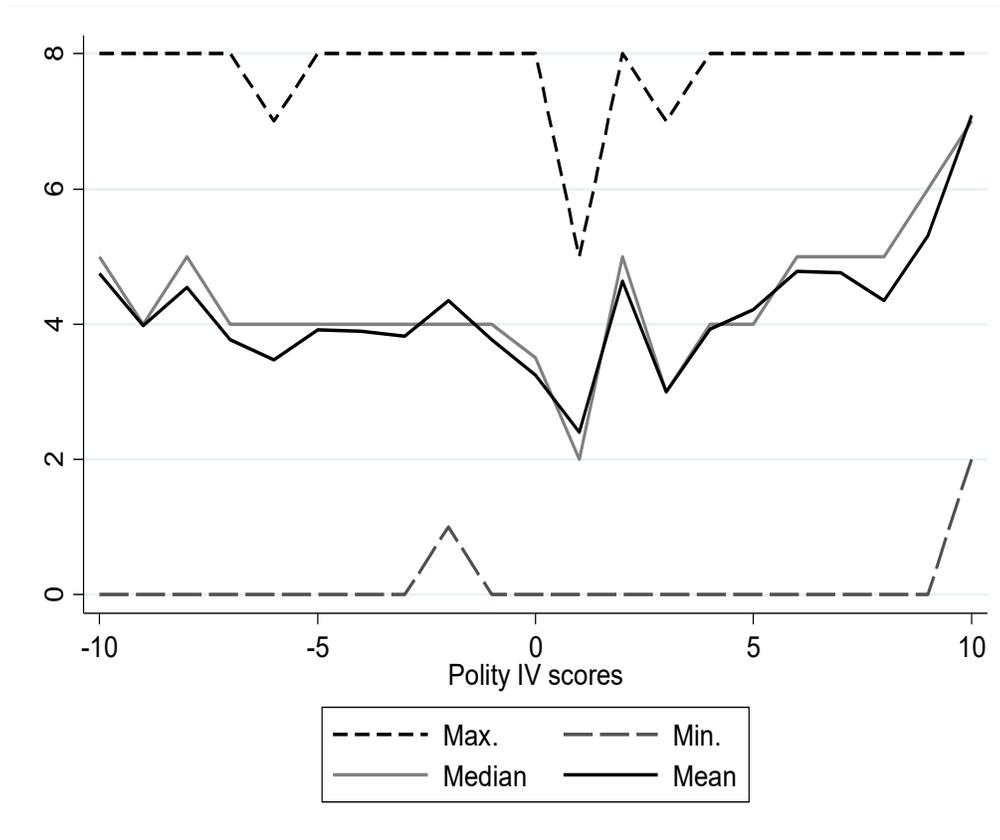
The striking pattern of physical integrity rights data is their volatile character: contrary to most attributes of political regimes, physical integrity rights protection and abuses are inherently dynamic phenomena. Even though democracies tend to make a lesser use of repression, state violence is not a structural feature of political regime type, and, consistently with what Davenport (2007a) labels as “the law of coercive responsiveness” (see also Earl 2011), physical integrity rights performance displays strong within-country variations over time and is only weakly related to regime type: in short, states commit physical integrity rights abuses when confronted to threats, and tend not to commit them otherwise.

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goals” (Woods and Gibney 2010, 373).

<sup>125</sup> It is not very clear, however, whether this coding scheme is systematically applied as long as numeric estimates are available (see Cingranelli and Richards 2010, 401-2).

**Figure 8.1. CIRI scores by Polity score**

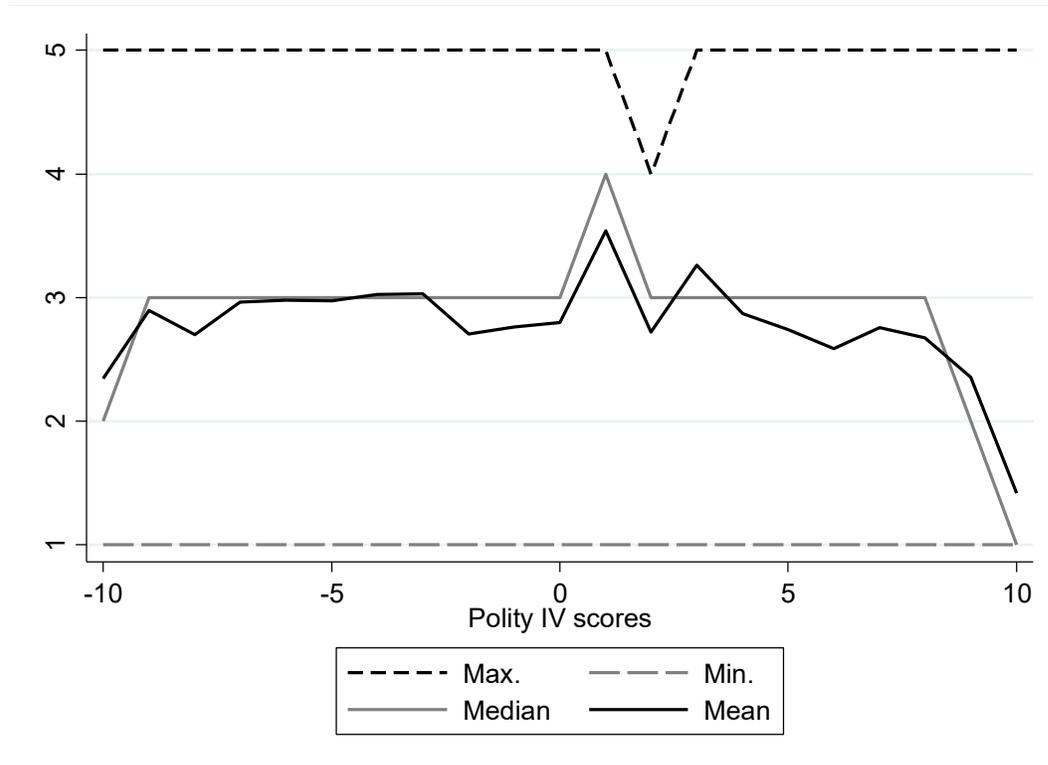


To illustrate this point, Figures 8.1 and 8.2 display the distribution of CIRI and PTS scores according to countries’ scores on the Polity IV democracy index.<sup>126</sup> As can be seen in Figure 8.1, the distribution of CIRI physical integrity rights score is characterized by a high dispersion within each regime category. Furthermore, the mean CIRI score does not seem to increase along with Polity scores: the mean fluctuates randomly around the middle of the CIRI scale until the highest Polity values (9 and 10) are reached. Judging by the distribution of higher and lower CIRI scores by Polity values, highly autocratic regimes are not necessarily the most repressive, and democracies sometimes exhibit a bad physical integrity rights performance: some relatively open systems (e.g., Spain, Brazil or Turkey) consistently obtain lower scores than strongly authoritarian regimes such as Qatar or Oman. This lends some support to the idea that repression is a tool that can be used

<sup>126</sup> To avoid confusing transitioning or occupied countries or failed states with anocracies, I use the first combined Polity index (POLITY) instead of the revised Polity index (POLITY2) in which standardized authority scores (-88, -77 and -66) are coded 0. These values are recoded as missing here.

independently from regime type, depending on the context.

**Figure 8.2. PTS scores by Polity score**



The distribution of PTS scores (Figure 8.2) displays the same patterns. On average, stable autocracies (from -10 to -5) do not perform worse than anocracies (from -5 to 5) or even some democracies. With regard to the latter, the mean PTS score tends to be somewhat lower than in nondemocracies, but the median remains the same until countries reach the score of 9 on the Polity scale. Furthermore, some countries rated 9 or 10 on the Polity index still reach the highest score of 5 on the PTS, whereas some of the least democratic systems get the best possible score of 1. On the whole, PTS scores vary strongly within, but not across categories: the mean remains relatively stable between the Polity scores of -10 and -8,<sup>127</sup> and the median, maximal or minimal value of the PTS do not vary in a

<sup>127</sup> Oddly enough, both indices indicate that physical integrity rights performance tends to worsen by the middle of the Polity scale: countries rated 1 get a median score of 4 on the PTS (indicating widespread abuses) and a quite bad median score of 2 on the CIRI physical integrity rights index.

predictable way as countries move from one Polity score to another.

Furthermore, many country scores display considerable fluctuations over time, which seem to be completely independent from regime type. For example, Zimbabwe's CIRI score changed 13 times between 1990 and 2008, and Turkmenistan's 12 times within the same time span. Within four years, Bhutan's CIRI score increased by 5 points (from 1 in 1992 to 6 in 1996) and Chile improved its score by 6 points (reaching its minimal score of 2 in 1998 and its maximal score of 8 in 2002). None of these countries has undergone a significant regime change in the period under consideration, and in the three nondemocratic countries, no alternation in power has taken place. In sum, these patterns cast some doubt on the idea that physical integrity rights protection is structurally dependent on political regime type.

### **8.3.3. Some problems with the use of the PTS in cross-country analyses of state repression**

The CIRI physical integrity rights index and the PTS are very similar in their content and scope: both are ordinal scales based on annual country reports by the US State Department and Amnesty International; both measure the same specific phenomenon, i.e., actual physical integrity rights violations carried out by state actors (instead of, for example, general political repression including civil liberties restrictions, or general human rights conditions including violence carried out by non-state actors) and include the same components. The main differences arise from their respective coding procedures, i.e., how each project deals with the issue of transforming inherently qualitative source materials – in which accurate numeric estimates of casualties are seldom available – into numerical, single-dimensional scales.

For example, while the CIRI index is exclusively based on the frequency of physical integrity rights violations, the PTS authors attempt to take several dimensions of state violence into account in their coding: first, the type of violence committed (with the implication that some types of violence – e.g., killings – should have more weight in the overall score than others – e.g., imprisonment); second, the frequency with which the state

perpetrates human rights abuses; and third, the share of the population targeted for abuse, i.e., the extent to which violence is selective or indiscriminate (Wood and Gibney 2010, 374). The distinction between the second and the third dimension (which are referred to as “intensity” and “range”, respectively) is especially important because, as the authors argue, the mere frequency of violence says nothing about whether it is targeted at political activists or whether the government is indiscriminately torturing and killing apolitical citizens (ibid., 379). Because the CIRI index does not take such information into account, no distinction can be made between specific patterns of coercive behavior and their ethical<sup>128</sup> as well as practical consequences (given the fact, for instance, that indiscriminate violence is supposedly more likely to backfire than targeted coercion).

Yet, even if such information is per se valuable, the question remains how selectivity is observed and how exactly this criterion affects the overall score of a country on the scale. The same goes for the dimension the PTS authors refer to as “scope” (i.e., the severity of abuses carried out by the state): although it is strongly implied by the descriptions of each level of state terror that instances of specific types of human rights violations would automatically move a country toward the highest values of the scale<sup>129</sup> (see also Wood and Gibney 2010, 377), there is no information about how exactly each type of abuse influences the overall score. Furthermore, if the concept to be measured is inherently multidimensional, conflating these dimensions into a single scale may not be a sound

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<sup>128</sup> An extensive discussion on the normative implications of giving higher scores to states that repress indiscriminately would be far beyond the scope of this analysis. However, it is somewhat regrettable that the authors do not provide a clear theoretical justification for this coding decision. In fact, Wood and Gibney (2010, 379) deny that this decision was based on normative considerations; however, states that selectively use violence against opponents still receive better scores than those that repress indiscriminately, regardless of the frequency or the type of violence carried out. The unfortunate implication is that (assuming that political activity is the criterion that determines whether repression can be considered as “selective”) torturing and killing apolitical citizens seems somehow more “serious” than torturing and killing political activists. Given the fact that the distinction between “apolitical citizens” and “political opponents” is itself very blurry and depends on what kind of political activities are allowed in the first place, it also implies that extremely restrictive states are more likely to get a better score on the PTS because they are more likely to be considered selective in their use of repression. Lastly, it is not clear what “selectivity” exactly means, and according to which criteria it would be more “acceptable” for a state to target its victims: for example, repression of political opponents (based on political engagement) is treated as selective violence, whereas ethnic cleansing or genocide (based on ethnicity) is apparently not (ibid., 374). Even though they do not clearly violate common sense, such decisions should be discussed more extensively.

<sup>129</sup> This would mean, for example, that extensive political imprisonment would dictate a score of 3 whereas widespread extrajudicial killings would dictate a score of 5 (barring other violations, and assuming that the frequency of abuse is the same in both cases).

choice since it violates one requirement of social sciences methodology, that variables should be unidimensional (for a further discussion, see McCormick and Mitchell 1997).

The most serious issue, however, is that, while the CIRI index is explicitly restricted to physical integrity rights violations perpetrated by the state or its agents, the PTS also takes terror from non-state actors into account (Cingranelli and Richards 2010, 405-7). This broader focus is not necessarily problematic per se, but including abuses perpetrated by non-state actors in measures of state violence can lead to serious biases when the question is whether some regime types repress more than others: theories linking regime type to physical integrity rights violations would not only need to address the question of why some states perpetrate more abuses than others, but also why terror from non-state actors is more widespread under some regimes. The two most comprehensive analyses on authoritarian regime type and repression to date (Poe et al. 1999; Davenport 2007b) both rely on the PTS but do not include such a discussion. Both studies find military dictatorships to engage more extensively in physical integrity rights violations and rely on the “coercive expertise” hypothesis to explain this empirical relationship, but this finding could actually simply result from the fact that military dictatorships tend to emerge in countries that are already plagued by domestic turmoil (see, e.g., Acemoglu et al. 2008, Svobik 2012; see also Chapter 4) and in which human rights conditions are generally bad. Thus, the question arises whether there is actually a regime effect in instances of human right abuses.

Since the CIRI index is based on a more reliable coding and more transparent aggregation rules, I will use it as my primary source for the empirical analysis, and check for the robustness of the results with the PTS if the empirical analysis lends preliminary support for my hypotheses (if the tests I perform with the CIRI data do not yield any significant result, I consider the hypothesis to be plainly rejected).

#### **8.3.4. A modified version of the CIRI physical integrity rights index**

Although the CIRI composite index of physical integrity rights is more reliable than the PTS, it is not free from methodological problems either. Most of these problems are due to the aggregation rule: for example, as Wood and Gibney (2010, 377) remark, summing

instances of abuses without taking casualties into account can artificially decrease<sup>130</sup> the overall score (e.g., a state that imprisons, tortures and kills one individual gets the same score as one that imprisons one person, tortures another and kills a third one). The matter is further complicated by data truncation issues, that is, the fact that each subcomponent index only comprises three possible scores, and that in each one of them, the category of the “worst offenders” is probably the most heterogeneous one because there is no upper bound to the number of abuses. Because this is true of each subcomponent, and because the scores are then summed without being weighted, a state in which one single type of abuse (e.g., extrajudicial killing) is reported can possibly get one of the highest (best) scores on the overall scale, even if the number of victims is considerable: thus, the additive rule can also result in artificially high scores on the overall scale (ibid., 387-91). A related concern is that this apparently simple aggregation rule can lead to a misleading picture of the overall physical integrity rights practices because it does not take the mutual dependence relationships between types of abuses (including possible substitution effects)<sup>131</sup> into account (see Fariss and Schnakenberg 2014).

However, since the CIRI project provides data on each subcomponent, problems due to aggregation rules are easy to solve. I thus transform the physical integrity rights index into a multiplicative index ranging from 0 (worst score) to 2 (best score). The underlying principle is that no country can reach the highest score as long as it gets scores lower than 2 in one (or several) of the subcomponents: a score of 1 in at least one of the subcomponents would automatically move the country from the best to the middle category of the overall index, and a score of 0 in one subcomponent would automatically place the country in the category of the worst performers (regardless of how well it performs otherwise). Thus, a score of 2 means that no physical integrity right violation occurred in the country-year; a score of 1 means that abuses occurred but none of those was widespread; a score of 0 means that at least one type of violation was widespread.

Table 8.1. gives an overview of the distribution of the original and the modified physical integrity rights indices.

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<sup>130</sup> Recall that low scores on the CIRI index indicate bad human rights performance.

<sup>131</sup> Substitution means that, if two repressive strategies can be used to achieve the same goal, the extensive use of one tactic may reduce the need to resort to the second one. For example, since extrajudicial killings and political imprisonment can both be used to neutralize opponents, widespread killings may reduce the instances of political imprisonment (Fariss and Schnakenberg 2014).

**Table 8.1. Distribution of original and modified CIRI physical integrity rights scores, 1981-2008**

<b>CIRI physical integrity rights index</b>				<b>Modified physical integrity rights index</b>			
<i>Score</i>	<i>Frequency</i>	<i>Percent</i>	<i>Cum.</i>	<i>Score</i>	<i>Frequency</i>	<i>Percent</i>	<i>Cum.</i>
0	221	5.49	5.49	0	2,201	54.67	54.67
1	241	5.99	11.48	1	1,341	33.31	87.98
2	316	7.85	19.32	2	484	12.02	100
3	382	9.49	28.81				
4	584	14.51	43.32				
5	598	14.85	58.17				
6	562	13.96	72.13				
7	638	15.85	87.98				
8	484	12.02	100				
Total	4,026	100		Total	4,026	100	

(Source: Cingranelli and Richards 2010)

**8.4. EMPIRICAL ANALYSIS**

**8.4.1. State repression and military coups**

Testing Hypothesis 1 requires undertaking a further modification of the physical integrity rights index: I use a variable that measures whether physical integrity rights conditions have improved or worsened compared to the previous year. I first compute the difference between the values of the physical integrity rights index in a given year and its values in the previous year: the resulting variable ranges between -2 (strong decrease in physical integrity rights performance) and 2 (strong increase in physical integrity rights performance). I then dichotomize the variable and code it 1 when the physical integrity rights performance has decreased, 0 otherwise. Positive values represent about 12.5% of the cases.

This transformation is justified by several reasons. First, the original variable is ordinal (i.e., the intervals between the scores are not necessarily equal, and a country that obtains a score of 2 cannot be said to perform twice as good as a country that obtains a score of 1), which means that it cannot be included in a regression model as it is. One possibility would be to dichotomize the original variable. However, deciding whether scores of 1 should be treated as “low” or “high” levels of repression would necessarily be somewhat arbitrary.<sup>132</sup> By contrast, measuring whether human rights conditions improve or worsen provides a clear, theoretically grounded cutoff point.

The second reason is theoretical rather than practical. Contrary to other political institutions – that can be expected to impact the probability of a coup without necessarily impacting its timing – repression cannot be regarded as a structural precondition that facilitates or hinders coups: it should rather be conceived as a triggering factor, not as an underlying prerequisite for military intervention. In other words, coercion is supposed to impact not only the likelihood of coups but also their timing: if coercion really is a proximate cause, coups should be expected to occur immediately after the onset of state terror, not five or twenty years later (the point being that, if repression correlates with coups regardless of how long it has been used, there is something wrong with the theory).

Table 8.2. displays the descriptive statistics on the new variable. Original physical integrity rights scores are displayed in the left column: changes of score from one year to another account for more than 60% of the cases, and dramatic changes of two points and more occurred in roughly 20% of the cases. The right column displays changes in the modified physical integrity rights index: The variance is strongly reduced since the new scale only comprises three points instead of nine. However, the variation of countries’ scores from one year to another is still non-negligible: changes account for almost 25% of the cases. These cases are evenly shared between improvement (about 11.5% of the observations) and deterioration (about 12.5%) of physical integrity rights performance.

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<sup>132</sup> Treating the index as a factor variable would be somewhat tricky given the small proportion of 2 and the distribution of the dependent variables.

**Table 8.2. Change of physical integrity rights scores (1981-2008)**

<i>Variable</i>	<i>Original CIRI score</i>	<i>Modified CIRI score</i>
No change	40.10%	75.77%
Increase in CIRI score:		
One point	19.40	11.43
Two points and more	9.90	0.23
Total	30.30	11.66
Decrease of CIRI score:		
One point	20.05	12.23
Two points and more	10.55	0.34
Total	30.60	12.57

(Source: Cingranelli and Richards 2010)

I test Hypothesis 1 using logistic regression. The independent variable of interest is the variable measuring increases<sup>133</sup> in repression levels, and described in the previous section. The dependent variable is a dummy variable that takes on the value of 1 if at least one coup attempt was reported in the country-year, 0 otherwise (see Chapters 4 and 5).

**Table 8.3. Coup attempts, 1981-2008**

<i>Frequency</i>	<i>Percent</i>	<i>Observations</i>
135	3.14	4,297

(Source: Powell and Thyne 2011)

The model also includes a short list of control variables, all of which are potentially related to both the dependent and independent variables and whose omission might therefore bias the results: I include, first, GDP per capita (see, e.g., Londregan and Poole 1990 for the relationship between economic development and coups; Davenport 2007a for the relationship between economic development and state terror); I also include a Cold War dummy, since both repression and coups appear to be less prevalent since 1990 (see Davenport 2007b). In addition, to ensure that the effects of heightened repression on coups

<sup>133</sup> Note that, while the transformed CIRI physical integrity right index measures physical integrity rights performance (i.e., low scores indicate the worst performers), the binary variable measuring changes in the repression levels is coded 1 when human rights performance *worsens*. Thus, ideally, the original index would have a negative, and the dummy variable a positive effect on coup risk.

are not due to the effects of higher resource endowments (Svolik 2012), I control for change in military expenditures per soldier. Data on military expenditures and military personnel are taken from the Correlates of War CINC subcomponents dataset, version 4.0 (Singer et al. 1972; Singer 1987).

I then add three further controls to address some potential statistical issues. First, since CIRI scores are partly based on a count of instances of physical integrity rights violations, I include the natural log of population in the model to correct for the fact that populous countries are more likely to cross the 50 cases threshold. Second, countries' previous scores on the modified CIRI physical integrity rights index are included in the model to address the data truncation issue: when a country already reached the worst possible score in the previous year, their score cannot decrease any further, hence resulting in a bias in favor of the worst performers. Since countries' scores on the physical integrity rights index are reported one year prior to the change values and the independent variable is lagged one year, the index is itself lagged two years. Note that the variable's coefficient cannot be interpreted in a substantial way.

**Table 8.4. Summary statistics on control variables (1981-2008)**

	<i>Mean</i>	<i>Standard deviation</i>	<i>Observations</i>
(ln) Population	16.03	1.54	4,289
Ln (GDP/capita)	8.087	1.13	4,125
Δ Exp./soldier (USD)	1,418.80	50,526.1	3,773

(Sources: Maddison project database; COW National Material Capabilities dataset; Ross 2013)

Finally, I also include a variable recording the number of years since the last instance of coups, in order to address time-dependence issues. Standard errors are clustered by countries. The results of the logistic regression are reported in Table 8.5. below.

Model 1 displays the basic results. Most variables impact coup risk in the expected way: coup risk decreases along with the level of economic development and the number of years since the last coup instance, and was somewhat higher in the Cold War period. Change in military expenditures per soldier has a slightly positive, marginally significant effect on

coup risk.<sup>134</sup>

**Table 8.5. Impact of increased repression levels on military coups, 1981-2008 (logistic regression, reporting odds ratios)**

	<i>Model 1</i>	<i>Model 2</i>
Peace duration (coups) <sup>a</sup>	0.950*** (0.011)	1.062* (0.036)
Δ Repression (CIRI) <sup>a, d</sup>	3.499*** (1.283)	5.006*** (2.049)
Repression (CIRI) <sup>b</sup>	0.593* (0.168)	0.471** (0.176)
GDP/capita <sup>a, c</sup>	0.615*** (0.097)	0.082*** (0.054)
Population <sup>a, c</sup>	0.917 (0.094)	0.035 (0.104)
Cold War <sup>d</sup>	1.764** (0.420)	0.167 (0.343)
Δ Exp./soldier <sup>a</sup>	1.000* (1.42e-05)	1.000 (2.63e-05)
Constant	11.47 (25.30)	3.059e+32 (1.866e+34)
Country fixed effects		YES
Year fixed effects		YES
Countries	157	49
Observations	3,373	1,096

Robust standard errors in parentheses. \* p < 0.10; \*\* p < 0.05; \*\*\* p < 0.01.

<sup>a</sup>Lagged one year; <sup>b</sup>Lagged two years; <sup>c</sup>Log; <sup>d</sup>Dichotomous

Most importantly, however, estimates displayed in Model 1 lend support to Hypothesis 1: increased repression levels positively and significantly impact the risk of military coup, and the variable is significant at the 1% level. Furthermore, the magnitude of the effect is substantial: military coups are about 3.5 times as likely to occur by increased repression levels. So far, Hypothesis 1 is supported.

Model 2 includes country and year fixed effects. The estimated effect of repression on military coups is even larger, coup risk increasing by a factor of 5 when physical integrity rights conditions have worsened. Moreover, the variable is now significant at the 0.001 level. Judging by the results regarding control variables, these estimates may not be completely reliable. For example, Model 2 predicts a (weak but still significant) positive

<sup>134</sup> Additional tests reveal that the current level of expenditures per soldier does not impact the dependent variable. I also attempted to replace these variables with alternative measures of resource endowment or coercive capacity. Military expenditures (both in absolute terms and standardized with population) do not impact coups either. Including or removing these variables does not impact the findings regarding repression

effect of peace duration on coup risk. GDP per capita displays an astonishingly low odds ratio of approximately 0.08 in the model (which means that for each one-unit increase in GDP, coup risk is divided by more than ten); and the sample is reduced to 49 countries instead of 157 because of the low variance of the dependent variable. However, since the results regarding repression remain relatively stable across estimates, there is still some confidence about their validity: Hypothesis 1 is so far confirmed.

I also perform additional tests (not reported) using rare-event logit (King and Zeng 2001) to correct for the high proportion of zeroes in the dependent variable: the results remain basically unaltered. I then check whether the results are sensitive to the specification of the independent variable: I thus reconstruct the dummy measuring increased repression levels using the additive CIRI physical integrity rights index. As expected, the magnitude of the effect decreases substantially, but the variable remains statistically significant. I also run a test using PTS<sup>135</sup> data, after transforming the index in a similar way:<sup>136</sup> the variable is likewise positively and significantly related to coup risk. Finally, removing statistically insignificant variables from Model 1, or adding controls for interstate and intrastate armed conflict does not impact the main results: on the whole, there is strong support for Hypothesis 1.

#### **8.4.2. Regime type and state repression**

To test Hypothesis 2, I use the modified CIRI index (ranging from 0 to 2) as my dependent variable. To measure my independent variables – regime type – I use Cheibub et al.'s (2010) data: the model thus includes five dummy variables measuring democracy, military dictatorship, civilian autocracy, monarchy, and party dictatorship, respectively. While I do not expect any significant difference to arise between authoritarian regimes, I still expect democracies to be on average less likely to use repression than autocracies (Poe and Tate 1994; Davenport and Armstrong 2004; Davenport 2007a).

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<sup>135</sup> Note that the PTS codes the Amnesty International and the US Department reports on two separate scales. I conduct the empirical analysis using the Amnesty International scale, and fill missing values with scores from the US Department scale.

<sup>136</sup> Positive values (i.e., cases in which one country's rating is worse than in the previous year) comprise about 17.5% of the observations. Since the PTS does not provide disaggregated data on subcomponents, I could not transform the index into a multiplicative one: The independent variable I use for the empirical analysis therefore measures changes in the value of the original index.

I control for three potential confounders. First, GDP per capita is notoriously associated with both regime type (see Przeworski and Limongi 1997; Burkhart and Lewis-Beck 1994; Boix and Stokes 2003) and repression (see Poe and Tate 1994; Davenport 2007a). Second, I add controls for ongoing internal and external conflicts: both types of conflict have been repeatedly found to make state repression more likely (see, e.g., Poe and Tate 1994; Davenport 2007a; Hafner-Burton 2014; Hill and Jones 2014) and, at the same time, the occurrence of such conflicts is not independent from regime type. In particular, countries under military dictatorship are more likely to experience intrastate armed conflict (Fjelde 2010) and to be involved in international conflicts (Sechser 2004; Lai and Slater 2006; Weeks 2012). I thus include a control for ongoing intrastate armed conflicts using data from the UCDP/PRIO Armed Conflict Dataset,<sup>137</sup> and a dummy variable that takes on the value of 1 if a country is involved in a militarized interstate dispute that reaches a hostility level of at least 4 on the Correlates of War coding scheme.<sup>138</sup>

**Table 8.6. Ongoing intrastate and interstate conflicts by regime type, 1981-2008**

		<i>Democracy</i>	<i>Monarchy</i>	<i>Civilian autocracy</i>	<i>Party regime</i>	<i>Military dictatorship</i>	<i>Sample</i>
<i>Intrastate armed conflict</i>	Frequency	278	14	240	42	169	743
	Percent	14.34	4.84	27.15	13.17	22.35	17.75
<i>Interstate armed conflict</i>	Frequency	406	33	240	91	221	991
	Percent	20.65	11.26	26.37	28.00	26.72	22.93

(Sources: UCDP/PRIO Armed Conflict dataset; Correlates of War project; Cheibub et al. 2010)

Additionally, I include a control for regime duration and the dependent variable lagged by one year. Dynamic models with lagged dependent variables are somewhat severe, but the practice is relatively common in the quantitative literature on repression (see, e.g., Poe and Tate 1994; Poe et al. 1999; Davenport 1995).

Since the dependent variable is trichotomous, I use multinomial regression. Standard errors

<sup>137</sup> Coups are excluded from the list (see Chapter 4).

<sup>138</sup> The COW project distinguishes militarized interstate disputes according to their hostility level: Disputes coded 1 involve no militarized action, disputes coded 2 involve the threat of force, and disputes coded 3 involve the display of force. Only disputes coded 4 or 5 involve the actual use of military force: A hostility level of 4 indicates a conflict that result in less than 1,000 battle-related deaths per year, while a hostility level of 5 indicates full-scale war.

are clustered by country. Results – with military regimes as the reference category – are reported in Table 8.7. below. The base outcome is 0, indicating the worst possible scores on the dependent variable: thus, positive relative risk ratios (above 1) indicate that a variable is associated with a better human rights performance, while negative relative risk ratios (below 1) indicate a comparatively worse performance. The first column reports the impact of independent variables on each country’s probability to move from a score of 0 to a score of 1; in the second column, the outcome is a score of 2.

**Table 8.7. The impact of regime type on state repression, 1981-2008 (multinomial regression, reporting relative risk ratios)**

	<i>Reference category: military dictatorships</i>	
	<u>Outcome: CIRI=1</u>	<u>Outcome: CIRI=2</u>
CIRI=1 (t-1)	9.290*** (1.324)	32.82*** (11.75)
CIRI=2 (t-1)	30.83*** (9.696)	869.6*** (455.5)
Democracy <sup>a</sup>	1.787*** (0.325)	2.537** (1.011)
Civilian autocracy <sup>a</sup>	1.064 (0.211)	0.905 (0.442)
Party regime <sup>a</sup>	1.036 (0.276)	1.109 (0.715)
Monarchy <sup>a</sup>	0.723 (0.201)	0.594 (0.350)
Regime duration <sup>a</sup>	1.005 (0.005)	1.005 (0.007)
GDP/capita <sup>a, b</sup>	1.259*** (0.104)	2.363*** (0.411)
Population <sup>a, b</sup>	0.723*** (0.044)	0.631*** (0.053)
Ongoing MID <sup>a</sup>	0.697** (0.108)	0.611* (0.160)
Ongoing intrastate conflict <sup>a</sup>	0.395*** (0.087)	0.303* (0.210)
Constant	5.531 (6.278)	0.008*** (0.014)
Countries	159	159
Observations	3,571	3,571

Robust standard errors in parentheses. \* p < 0.10; \*\* p < 0.05; \*\*\* p < 0.01.

<sup>a</sup>Lagged one year; <sup>b</sup>Log

Unsurprisingly, the most powerful predictor of a country’s score in a given year is the score it reached in the previous year: countries that received a score above 0 in the previous year are on the whole very unlikely to receive a score of 0 in the current year. For example, as can be seen in Column 2, countries that received a score of 2 in the previous year are 870 times less likely to move from a score of 2 to a score of 0 than countries with lower scores in the preceding years. This is by far the highest coefficient (the relative risk ratios in Column 1 are somewhat less impressive), but this indicates that, when the small

fluctuations that appeared on the original CIRI index are put aside and a more conservative measure is adopted, human right performance is actually relatively stable over time.

Results regarding control variables are also in line with expectations: GDP per capita has a positive and non-negligible effect on the dependent variable, the coefficient being significant at the 1% level in both columns. By contrast, large populations and the occurrence of either interstate or intrastate armed conflict reduce the probability that a country reaches a score of 1 or 2 on the modified CIRI index. Regime duration has no discernible impact on the dependent variable.

Turning to the independent variables of interest, it appears that democracies are somewhat more likely than military dictatorships to reach a score of 1 instead of 0, and much more likely to reach a score of 2: the corresponding relative risk ratios are 1.8 and 2.5, respectively. This result is expected and broadly consistent with prior research (see Poe and Tate 1994; Davenport and Armstrong 2004; Davenport 2007a; Hill and Jones 2014). Yet, democracies are the only regime that is consistently correlated with better human rights performance compared to military dictatorships: none of the other regime dummies reach statistical significance. Results reported in both columns indicate that party regimes and civilian autocracies actually behave very similarly to military dictatorships: the associated relative risks ratios are in both cases very close to 1. Monarchies seem to be less prone to repression than military dictatorships, but the variable falls short of statistical significance.

Are these findings due to the specification of the dependent variable? This could be the case, given the fact that the coding scheme I used strongly reduces the variance of the original CIRI index. Moreover, most of this reduction of variance occurred at the lower end of the index – that is: on the autocracies' side – because a score of 0 in any of the subcomponents of the CIRI index results in the country receiving a score of 0 on the overall index. In other words, the resulting index is severely truncated. This could mask important variations within the subgroup of autocracies, because this coding scheme makes it more probable that they all reach the same score: countries that commit some physical integrity rights violations become indistinguishable from those that engage in widespread repression in a systematic manner.

I therefore create a new variable, which is a slight modification of the original CIRI physical integrity rights index. I recode the original variable in the following way (see

Table 8.8. for the descriptive statistics):

- (1) The values of 0, 1, and 2 are recoded 0;
- (2) The values of 3, 4, and 5 are recoded 1;
- (3) The values of 6, 7, and 8 are recoded 2.

**Table 8.8. Modified CIRI index, 1981-2008 (descriptive statistics)**

<i>Score</i>	<i>Frequency</i>	<i>Percent</i>	<i>Cum.</i>
0	778	19.32	19.32
1	1,564	38.85	58.17
2	1,684	41.83	100
Total	4,026	100	

(Source: Cingranelli and Richards 2010)

The resulting index differs widely from the previous one: while only 12% of the observations reached the value of 2 on the first modified CIRI index, this value represents approximately 42% of the cases with the new index. Furthermore, more than half of the observations took on the value of 0 on the previous index, while this score represents less than the fifth of the cases with the new variable. Since the resulting variable reflects variations at the lower end of the CIRI index more precisely, it might be more appropriate for analyzing the effects of authoritarian regime type on physical integrity rights. I thus rerun the previous model while only replacing the dependent variable. Results can be seen in Table 8.9.

Again, the most powerful predictors are the lagged dependent variables. Results regarding control variables do not differ greatly from those of the previous model. With regard to regime type, the only changes worth noting is that democracy loses its statistical significance in the first column (that is, if a country becomes democratic, its probability of increasing its score from 0 to 1 does not systematically change) and that monarchy now has a positive and significant impact on the probability that a country gets a score of 1 or 2 instead of 0. This effect is quite large: monarchies are four times as likely as military dictatorships (the reference category) to increase their score from 0 to 1, and more than twice as likely to receive a score of 2. Interestingly, the relative risk ratios associated with monarchy are substantially higher than those associated with democracy in the first

column, and almost as high in the second column.

**Table 8.9. The impact of regime type on state repression, 1981-2008 (robustness check)**

	<i>Reference category: military dictatorships</i>	
	<u>Outcome: CIRI=1</u>	<u>Outcome: CIRI=2</u>
CIRI=1 (t-1)	7.589*** (1.339)	45.57*** (22.55)
CIRI=2 (t-1)	265.8*** (274.2)	16.205*** (20.979)
Democracy <sup>a</sup>	1.338 (0.280)	2.912*** (0.828)
Civilian autocracy <sup>a</sup>	1.053 (0.196)	1.207 (0.342)
Party regime <sup>a</sup>	1.184 (0.284)	1.400 (0.519)
Monarchy <sup>a</sup>	4.149*** (1.923)	2.449** (1.047)
Regime duration <sup>a</sup>	0.995 (0.008)	1.004 (0.008)
GDP/capita <sup>a, b</sup>	1.184* (0.116)	1.923*** (0.250)
Population <sup>a, b</sup>	0.778*** (0.041)	0.525*** (0.043)
Ongoing MID <sup>a</sup>	0.894 (0.133)	0.639** (0.135)
Ongoing intrastate conflict <sup>a</sup>	0.347*** (0.059)	0.185*** (0.086)
Constant	13.71** (14.22)	4.650 (7.279)
Countries	159	159
Observations	3,571	3,571

Robust standard errors in parentheses. \* p < 0.10; \*\* p < 0.05; \*\*\* p < 0.01.

<sup>a</sup> Lagged one year; <sup>b</sup> Log

These new findings notwithstanding, the main results remain on the whole unaltered: judging by the size of the coefficients and the levels of statistical significance, neither party regimes nor civilian autocracies appear to differ systematically from military dictatorships with regard to their use of repression. Military dictatorships are more likely to violate physical integrity rights than democracies and monarchies, but no more and no less than non-institutionalized regimes.

As a final test, I investigate whether these findings hold with logistic regression. Recall that both dependent variables built from the CIRI scale can take on the values of 0, 1, and 2. I thus dichotomize them in two ways: each variable is coded 1 either when the original variable was coded 1 or 2 (0 otherwise) or only when the original variable was coded 2. In short, these two coding procedures correspond to a more or less encompassing definition of good human rights performance: this allows me to test whether military dictatorships are systematically more likely to be in the group of the worst performers, or systematically less

likely to be in the group of best performers. I then run four models with each of the resulting combinations as the dependent variable. Interestingly, results concerning democracies and monarchies vary strongly according to the specification of the dependent variable: when a less restrictive definition of good human rights performance is adopted, monarchies are the most likely (judging by the odds ratios) to display a good overall performance; however, they are still less likely than democracies to achieve the best scores on the CIRI scale. Putting aside these variations, the estimated effect of authoritarian regime type is generally insensitive to the specification of the dependent variable: neither party regimes nor civilian autocracies perform better than military dictatorships in any of the four models. Finally, none of these results change substantively when country and year fixed effects are included in the models.

### **8.4.3. Repression and coups in military and civilian regimes**

We now turn to Hypothesis 3. To test whether repression has a different impact on coup risk according to whether the military is in power or not, I split my sample into two subgroups, namely military and non-military (civilian) regimes. I then rerun Model 1 (see Section 8.4.1.) for each subsample.

The results (displayed in Table 8.10.) support Hypothesis 3: although the baseline probability of a coup differs largely in both samples, the effect of repression on coups is roughly similar in both models, with odds ratios slightly above 3. Actually, repression is even the only variable that is consistently associated with a higher coup risk in both models: most of the control variables fall short of statistical significance, even though they impact the dependent variable in the expected way. Although consistently negative, the effect of GDP per capita is never significant. Past coups impact the risk of renewed coup attempts, but only in the case of civilian regimes. Population, Cold War, and military expenditures have virtually no effect on the dependent variable. Finally, ongoing armed conflicts have – as expected – a positive impact on coup risk.

**Table 8.10. The impact of repression on military coups by regime type, 1981-2008 (logistic regression, reporting odds ratios)**

	<i>Civilian regimes</i>	<i>Military regimes</i>
Peace duration (coups) <sup>a</sup>	0.953*** (0.014)	0.991 (0.028)
$\Delta$ Repression (CIRI) <sup>a, d</sup>	3.425** (1.851)	3.043** (1.631)
Repression (CIRI) <sup>b</sup>	0.677 (0.383)	1.055 (0.509)
GDP/capita <sup>a, c</sup>	0.738 (0.181)	0.593 (0.257)
Population <sup>a, c</sup>	0.913 (0.116)	0.866 (0.219)
Cold War <sup>d</sup>	1.958 (0.803)	1.052 (0.431)
Exp./soldier <sup>a</sup>	1.000 (2.09e-05)	1.000 (2.90e-05)
Ongoing intrastate conflict <sup>a</sup>	1.149 (0.607)	2.209 (1.156)
Constant	1.683 (5.234)	27.05 (133.1)
Countries	149	57
Observations	2,698	598

Robust standard errors in parentheses. \*  $p < 0.10$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

<sup>a</sup>Lagged one year; <sup>b</sup>Lagged two years; <sup>c</sup>Log; <sup>d</sup>Dichotomous

To address statistical issues arising from the low number of observations left in the sample of military regimes (there are only about 600 valid observations, restricted to 57 countries), I run another model (not reported here) with interaction terms: I interact repression with military regimes first and then with civilian regimes. Neither of the interaction terms attains statistical significance, and in both models, only one of the constitutive terms (namely repression) is significantly and positively related to coups. Thus, the impact of repression on coups does not depend on regime type: Hypothesis 3 is confirmed as well.

## 8.5. CONCLUSION

In this chapter, I examined how the military is likely to react to increased repression levels, whether military dictatorships are more likely to violate physical integrity rights, and whether the influence of state repression on coup risk is constant across regime types. My results all point to a general conclusion: it is simply not true that military officers are more likely to use repression or to push for coercive policies than civilian leaders.

To begin with, I do not find that military regimes are more likely to use repression than

other non-institutionalized regimes. Military regimes behave in the same way as all other regimes in which the chief executive is not constrained by voters: they use repression as a response to specific threats. To put it in technical terms, when threats are held constant, I do not uncover any marginal effect of authoritarian regime type.

This finding is preliminary and still needs further confirmation. Results regarding the impact of regime type on repression appear to be highly data-sensitive: results obtained with the CIRI index are different from those obtained with the PTS data (see Poe et al. 1999; Davenport 2007b), and although there are compelling theoretical justifications for using the former instead of the latter, some statistical properties of the CIRI data could mask important variations within the group of the most repressive regimes. In a nutshell: it could well be that military dictatorships are on average exactly as repressive as civilian-led dictatorships, but that they are more likely to practice the most extreme forms of state terror.

Yet, even if it were the case, the conclusion that widespread repression is a consequence of the military's preferences and beliefs about the legitimacy and effectiveness of violence – because military officers' specific training and organizational culture lead them to favor the use of force as a means of solving political conflicts – would still be unwarranted. One key finding presented in this chapter is that a degradation of physical integrity rights conditions increases the risk that the military defects from the regime and topples the incumbent. While this finding constitutes only indirect evidence that military officers rebel against orders to repress because they fundamentally dislike violence, it is nevertheless consistent with an observation regularly made by early analysts of civil-military relations, that professional soldiers rarely consider domestic repression as being part of their duties and that they are generally reluctant to carry it out themselves. Moreover, the fact that the impact of state terror on coup risk is independent from regime type constitutes some tentative evidence that this empirical association is not due to competing factors, such as increased opportunities to seize power or the desire to avoid reprisals upon regime transition.

Two further findings deserve to be mentioned. First, the empirical analysis confirms the expectation that democracies tend to be internally more peaceful: they are systematically less likely to practice state repression than non-democratic regimes. This result is not really

surprising, but it is not entirely trivial either: recall that Cheibub et al.'s (2010) measure of democracy is based on a minimal and purely procedural definition, and does not include any component directly related to human rights protection. This finding indicates that competitive elections alone, because they allow voters to punish elected officials, generate a strong incentive for leaders not to commit human rights violations.

Second, I find tentative evidence that monarchies are somewhat less likely to use repression than non-institutionalized autocracies. This finding is rather mysterious and would need further confirmation, but could constitute an avenue for future research.

Finally, the findings presented here complement past research on the destabilizing effects of state repression: while the fact that repression can backfire because it leads opposition groups to switch from peaceful to violent tactics is now well-documented (see, e.g., Lichbach 1987; Moore 1998), defections of the armed forces have received only scant attention in cross-national analyses of regime transitions. A task future research will have to confront will be to identify the factors that may explain the variations in the military's behavior in times of crisis – that is, why the military sometimes chooses to support the incumbent and sometimes decides to defect from the regime.

## 9

### **Coup risk, regime type, and international conflicts: Why are military dictatorships more likely to initiate militarized interstate disputes?\***

*“War is too important to be left to the generals.”*

*Georges Clemenceau.*

Idi Amin – Uganda’s president from 1971 to 1979 – managed to stay in office for eight years in spite of intense domestic opposition, only to lose power as a consequence of a lost war against neighboring Tanzania. The war lasted from October 1978 to April 1979 and was quickly won by the Tanzanian military: the Ugandan army, poorly trained, ill-disciplined, plagued by ethnic divisions and weakened by repeated purges (Ravenhill 1974; Gertzel 1980; Brett 1995), proved unable to mount effective resistance. The Ugandan-Tanzanian war represents an interesting case, not only because of its outcome – irregular leadership changes resulting from foreign invasion are rather infrequent but by no means exceptional (see Goemans et al. 2009a; Svobik 2012) – but because it was provoked by Amin’s decision to annex a portion of the Tanzanian territory, thereby prompting Tanzanian

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\* A more recent version of this chapter appeared as: Panel, Sophie. 2017. Regime instability, leader’s affiliation, and organizational culture: Why are military dictatorships more likely to initiate militarized interstate disputes? *Security Studies* 26 (2): 333-358.

president Nyerere's decision to invade Uganda and remove Amin from power. Why, then, did Amin initiate a war he probably knew he could not win?

At first glance, Amin's aggression was motivated by the enduring rivalry between Uganda and Tanzania. Relations between both countries were always tense: Nyerere not only refused to recognize Amin's government following his 1971 coup but also granted political asylum to Uganda's ousted president Obote. Furthermore, Tanzania harbored a large community of Ugandan opponents, some of them organized in guerilla movements that unsuccessfully attempted to invade Uganda and oust Amin in 1972 (Gertzel 1980). The conflict between Uganda and Tanzania thus remained latent for eight years until it finally escalated in 1978. Yet, the timing of the attack on Tanzania tells a different story: it was launched shortly after a mutiny of the Ugandan army, to which Amin reacted by using potentially dissident army units to invade the north-western part of Tanzania (Khadiagala 1993; Brett 1995). Is this timing a mere coincidence, or was the Ugandan-Tanzanian conflict a diversionary war? Although it would be difficult to identify one single cause for the events that ultimately led to Amin's downfall, several facts are consistent with the latter interpretation. First, dissension in the Ugandan army – taking the form of periodic coups and assassination attempts – was a permanent issue, and probably one of the most important threats to Amin's rule (Brett 1995). Second, although Ugandan exiles represented a long-term problem, the threat was less acute in the short run: Ugandan opponents abroad were divided along ethnic, political and ideological lines, did not enjoy military support from Tanzania until 1978, and proved unable to establish an organization capable of overthrowing Amin (Gertzel 1980; Brett 1995). Finally, if the Ugandan-Tanzanian conflict was primarily caused by tensions between the two countries, it is difficult to explain why Amin did not attack Tanzania upon the failed invasion of 1972 but did it six years later for no apparent reason. The mutiny of 1978 constitutes by contrast an immediate cause for the outbreak of the war to the extent that it directly threatened Amin, thus forcing him to resort to extreme measures in order to divert attention and insulate himself from the threat. The strategy was risky and ultimately backfired, but it was probably Amin's only way of getting rid of dissidents within its own military in a definitive manner.

Is Amin's Uganda a unique case, or is it representative of a more general empirical

association between domestic political instability and interstate conflicts? Recent research on international conflicts actually suggests that such strategies are not exceptional, and that many international wars have domestic roots (e.g., Miller and Elgün 2011; Powell 2014b). This chapter builds on this literature and analyzes the relationship between a specific form of political instability – namely coup risk – and the initiation of militarized interstate disputes (MIDs). More specifically, I argue that dictators do not initiate international wars when they lose public support – as elected leaders do – but rather when they face a high risk of being overthrown by their own militaries. I further argue that this causal relationship explains the recurring finding that military dictatorships are more likely to initiate international conflicts than any other regime type (Sechser 2004; Lai and Slater 2006; Weeks 2012).

Next section presents a short overview of current debates on military dictatorships and their conflict behavior, and specifies the general argument developed in this chapter. Section 9.2. reviews the literature on the initiation of international conflicts by dictatorships. Section 9.3. presents the hypotheses. Sections 9.4. and 9.5. are devoted to the empirical analysis. Section 9.6. concludes.

## **9.1. OVERVIEW OF THE PROBLEM**

As shown in Chapter 8, military dictatorships' supposed inability to settle political conflicts in a peaceful way is a recurring theme in the literature on authoritarian regimes (see, e.g., Nordlinger 1977; more recently, Davenport 2007b; Fjelde 2010). The international relations literature is not an exception to this trend, and the idea that military officers have been socialized to favor the use of force over peaceful conflict resolution pervades recent scholarship on international conflicts (see, e.g., Sechser 2004; Weeks 2012). There is probably some truth in this assertion: a large number of cross-national, quantitative studies have found military dictatorships to be more likely to initiate militarized interstate disputes than authoritarian regimes led by civilians (Sechser 2004; Lai and Slater 2006; Weeks 2012).

Table 9.1. presents the results of a simple logit model on the relationship between regime

type and the initiation of militarized interstate disputes, using data from the COW project.<sup>139</sup> The model is kept parsimonious and includes only a small set of control variables, namely a control for time-dependence, a count of each country's land and sea borders (also drawn from COW data), and the COW's National Material Capabilities (CINC) dataset. All independent variables are lagged by one year, and standard errors are clustered by country. Military dictatorships are the reference category.

**Table 9.1. Regime type and the initiation of militarized interstate disputes, 1946-2008 (logistic regression, displaying odds ratios)**

	<i>Reference: military dictatorship</i>
Past MIDs	0.916*** (0.009)
CINC	3,507*** (3,744)
Total borders	1.084*** (0.017)
Democracy	0.566*** (0.102)
Civilian autocracy	0.747** (0.109)
Party regime	0.551*** (0.103)
Monarchy	0.518*** (0.117)
Constant	0.214*** (0.0417)
Countries	169
Observations	7,549

Robust standard errors in parentheses

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

The results are unambiguous: all regime dummies differ significantly from the baseline and exhibit negative coefficients.<sup>140</sup> Odds ratios associated with democracy, party dictatorship

<sup>139</sup> The COW project distinguishes militarized interstate disputes according to their hostility level: disputes coded 1 involve no militarized action, disputes coded 2 involve the threat of force, and disputes coded 3 involve the display of force. Only disputes coded 4 or 5 involve the actual use of military force: A hostility level of 4 indicates a conflict that result in less than 1,000 battle-related deaths per year, while a hostility level of 5 indicates full-scale war (Jones et al. 1996; Kenwick et al. 2013). The dependent variable used here is a dummy variable that takes on the value of 1 if the country initiated a dispute reaching a level of 4 or 5, 0 otherwise. Only the first year of the dispute is reported and ongoing dispute years are coded 0. Multiple conflict initiations within the same calendar year are collapsed into a single value.

<sup>140</sup> After changing the reference category, I do not find any systematic difference between the four other regime types: civilian autocracies appear to be slightly more likely to initiate a MID than democracies and party regimes, but the effect is only marginally significant. I do not find any trace of a monadic

and monarchy indicate that military dictatorships – the reference – are almost twice as likely to initiate a MID than these three regimes. The effect of civilian autocracy is somewhat weaker, but still non-negligible (civilian autocracies being about 75% as likely to initiate a MID as military dictatorships). These findings point to a clear tendency – military dictatorships are more belligerent than any other regime – and confirm results uncovered by prior research.

Most researchers agree on the fact that the source of this variation between regime types is domestic: military regimes tend to respond to internal crises by initiating external conflicts, in a desperate attempt to rally support. Classical examples include the Falklands war of 1982, initiated by the Argentine junta (Miller and Elgün 2011), or the war in Cyprus initiated by the Greek military dictatorship in 1974 (Lai and Slater 2006; Sirin 2011). But besides this relative agreement, the relatively sparse literature on military dictatorships and international conflicts has not yet come to a consensus about what exactly makes these regimes more belligerent than their civilian-led counterparts. For example, Lai and Slater (2006) extend the classical argument about diversionary wars and locate the source of military regimes' conflict behavior in their inherent instability: lacking a strong party infrastructure that would help them enhance social control, enforce policies and secure their tenure, dictators backed by the military often have no choice but to resort to “extreme” measures – such as the initiation of an interstate conflict – to avoid being violently removed from power. By contrast, Weeks (2012) views the military's organizational culture as the primary cause of military regimes' aggressive foreign policy: military officers, she argues, “have been selected for, and socialized to hold, specific beliefs about the utility and the appropriateness of military force as an instrument of politics” (Weeks 2012, 333; see also Sechser 2004). She consistently finds that, while powerful civilian domestic audiences – e.g., party elites – reduce the risk of conflict initiation, any combination of personalism and/or military influence makes states more belligerent.

Why, then, are military dictatorships more likely to initiate MIDs than civilian-led autocracies? The ideal-typical military dictatorship displays three features that are all possibly related to the initiation of MIDs. First, the effective head of government is a

member of the armed forces (Cheibub et al. 2010). Second, the military as an organization either rules directly via a junta or at least “decides who will rule and exercises some influence on policy” (Geddes 1999, 21). This distinction is not trivial: there are numerous examples of leaders (e.g., Idi Amin in Uganda) who have a background in the armed forces and accessed power with the active support of the army, but managed to free themselves from any influence from the military. Depending on whether classifications are based on a broad or restrictive definition of military rule, such dictatorships can either be labeled as military or personalist. This distinction between collective military rule and personal, military-based rule is increasingly acknowledged by researchers, as reflected by the growing popularity of the junta/strongman dichotomy in the recent literature on authoritarian regimes and MID initiation. Yet, to complicate matters, a large share of personalist dictators are former members of the military (Geddes et al. 2014, 158); furthermore, as Lai and Slater (2006) convincingly argue, personalist dictators are always – to some extent – dependent on the armed forces to maintain their rule, and, conversely, military-based dictatorships are systematically more likely to become personalized than party-based ones (p. 119; see also Tullock 1987; Acemoglu et al. 2009). Further confusion arises from the fact that personalist dictatorships – like military regimes – also display a comparatively high propensity to conflict initiation (Peceny and Butler 2004; Weeks 2012): there is still disagreement on whether their conflict behavior is due to their small winning coalition (Peceny and Butler 2004) or the lack of constraints on the chief executive (Weeks 2012), or whether personalization is simply an epiphenomenon of military influence (Lai and Slater 2006).

The third typical feature of military regimes is their mode of accessing and maintaining political power, namely the “actual or threatened use of military force” (Hadenius and Teorell 2007a, 146). In other words, military dictatorships are typically established through military coups, and military dictators – precisely because they primarily depend on the military to stay in office, and cannot easily engage in coup-proofing strategies – run a high risk of being overthrown by members of their own ruling coalition (Svolik 2012). This partly explains the short tenure of military leaders, and the fact that many of them are removed from office through irregular and often violent ways (see descriptive statistics section below) and subsequently replaced by another military officer (Cheibub et al. 2010,

86).

All of these three institutional features are potentially related to the initiation of militarized interstate disputes: first, military leaders – regardless of whether their ruling coalition is made up of other officers or civilian elites – may be more likely to initiate MIDs, either because their prior military background impacts their attitudes toward the use of force (Horowitz and Stam 2014) or because individuals who view the use of force as appropriate are more likely to enter the military in the first place. Second, regimes in which the military has a strong influence on foreign policy can be expected to behave differently from those in which foreign policy decisions are exclusively taken by civilians: except for the most serious conflicts that involve mass conscription, the military has to bear the costs of military intervention; thus, in most conflict situations, its preferences are susceptible to diverge strongly from those of the civilian population and elites. Third, a number of studies find political instability – and particularly coup risk – to be positively related to the initiation of MIDs (Belkin and Schofer 2005; Miller and Elgün 2011; Powell 2014b).

The aim of the chapter is not to assess the validity of previous findings regarding military dictatorships' conflict behavior, but rather to find out which of these three regime features accounts for the empirical regularity uncovered by prior research. To do so, I exploit the distribution of military influence, coup risk, and leader affiliation within and across regime categories: as discussed above, even if these features are typical of military dictatorships, they can be present in regimes that are not military dictatorships. Conversely, some military dictatorships may lack one or two of these three features: for example, the military junta that ruled Brazil from 1964 to 1985 was arguably more stable than, for example, Amin's government. In other words, political instability and military involvement in domestic politics – the two key variables investigated here – often tend to go hand in hand (see Chapter 4) but they correlate only imperfectly: this imperfect correlation makes it possible to disentangle their respective effects.

## **9.2. STATE OF THE ARTS: DOMESTIC SOURCES OF INTERSTATE CONFLICTS**

Theories on domestic causes of international conflict are generally subsumed under the

umbrella concept of “diversion theories,” i.e., theories that predict that leaders who face a high risk of being removed from office are more likely to initiate interstate conflicts. This can happen for several, theoretically distinct reasons: leaders either attempt to distract voters’ attention away from domestic issues, or to shift the blame for domestic problems onto foreign rivals, or to appeal to national identities to promote “rally round the flag” effects and enhance in-group cohesion (Sirin 2011). Somewhat differently, the “gambling for resurrection” hypothesis predicts that leaders get involved in international conflict when a military defeat would not increase their risk of losing office whereas victory would certainly decrease it (Chiozza and Goemans 2003). This incentive increases along with the leader’s risk of being punished after leaving office: not only the risk of being removed from power affects his foreign policy behavior, but also the consequences of losing office (Goemans 2008).

On the whole, cross-national studies on interstate war did not find strong evidence for diversionary behavior.<sup>141</sup> Furthermore, recent research has cast doubt on whether theories of diversionary wars can be applied to dictatorships. First, as Dassel and Reinhardt (1999) note, these theories rely on the rather unrealistic assumption that any leader can unilaterally order its military to fight wars, regardless of the organization’s own preferences. This is probably not true of most dictatorships: since the typical dictator does not rule by consent but rather by force, he is to some extent dependent on his armed forces to secure his rule, which provides the military with some leverage and influence on policies (Svolik 2012). In particular, conflicts of interest between the dictator and the military regarding the use of force abroad can easily lead to the regime’s demise.<sup>142</sup> Second, the incentives to stage a diversionary war to distract voters’ attention away from domestic issues are probably not the same for a dictator as for a democratically elected leader. On the one hand, a dictator’s seat is not contested as often as an elected leader’s seat, since he does not have to periodically run for re-election (Chiozza and Goemans 2003; Goemans 2008); on the other hand, his political survival does not primarily depend on its popularity among the whole public, but rather on the support of a small elite coalition. Thus, a dictator’s foreign policy

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<sup>141</sup> For a recent review and a discussion of the quantitative literature on diversion, see Miller and Elgün (2011).

<sup>142</sup> The 1974 coup that ultimately led to the Carnations Revolution in Portugal is an example of such conflicts of interests.

is probably oriented toward members of this coalition (see Weeks 2012) or potential challengers (see Belkin and Schofer 2005) rather than the whole public. As a consequence, empirical studies on diversion regularly find that, while classical indicators of mass discontent (such as slow economic growth) are related to the initiation of interstate conflict by democratically elected leaders, such incentives are not sufficient for dictators: if anything, autocrats only react to serious challenges to their rule, such as violent protests or intrastate armed conflicts (Davies 2002; Sirin 2011; for an exception, see Miller 1999; for an alternative formulation of the theory, see Dassel and Reinhardt 1999). Yet, even this mechanism is contested: Gleditsch et al. (2008) find that externalization of civil conflict is indeed a pervasive phenomenon, but occurs for reasons mostly unrelated to diversion.

Finally, if democratically elected leaders react to public discontent by engaging in aggressive foreign policy behavior, this is primary because they lack any alternative way to maintain social control. This is not true of dictatorships since they can manage mass dissent with repression. As Gelpi (1997) argues, whereas repression is a costly option for leaders who must run for re-election, autocrats tend to favor it over diversion, because it is both less risky on the short run and more effective, on the long run, than short-lived rally effects. He consistently finds that conflict initiation is positively related to civil unrest in democracies, but negatively in dictatorships (see also Davies 2014).

More recent research on the conflict behavior of authoritarian regimes departs from classical theories of diversion and either switches the analytical focus from the mass public to the dictator's ruling coalition or brings the military back in analyses of potential threats to the dictator's rule. For example, scholars have recently begun to argue that political instability in the form of coup risk may constitute a stronger incentive to divert than declining support for the dictator or mass unrest – mainly because coups represent the most frequent irregular way leaders lose office (Miller and Elgün 2011; Powell 2014b). When confronted with a high risk of being deposed by their own armed forces, autocrats initiate wars abroad either to divide the military by hindering coordination between different branches of the army or exacerbating inter-branch rivalries (Belkin and Schofer 2005), to “gamble for resurrection” (Miller and Elgün 2011, 200), or simply to “keep the generals busy and out of the capital” (Lai and Slater 2006, 121). This is consistent with one emerging finding in the interstate conflict literature, that involvement in external war

enhances civilian control of the military and decreases coup risk (Desch 2001; Arbatli and Arbatli 2014; McMahon and Slantchev 2015). Powell (2014b) demonstrates that political instability increases the risk that a dictatorship initiates a MID unless the dictator can take coup-proofing measures as an alternative. Dassel and Reinhardt (1999) bring a different argument on the relationship between political instability and use of force abroad: according to them, what previous works have interpreted as diversionary violence initiated by the incumbent was in fact often initiated by the military itself. Domestic strife, they contend, does not lead to external conflict unless it directly threatens the military's corporate interests: in those cases, the military has an incentive to use force abroad in the hope of provoking a rally effect, or because a war would leave civilian leaders no choice but to cede greater political autonomy and material resources to the armed forces.

Fewer studies on the conflict behavior of authoritarian regimes have attempted to assess the impact of the military's organizational culture on MID initiation. A notable exception is a study by Sechser (2004), who finds that states lacking strong civilian control of the military are more likely to initiate MIDs than average. He attributes this pattern to the military's parochial interests and to perception biases – such as, e.g., an overestimation of both the probability and the advantage of war – that are widespread within the armed forces. In a somewhat different way, Horowitz and Stam (2014) analyze the impact of leaders' past experience in the military on their propensity to initiate wars abroad. They find that leaders with experience in the military but no combat experience are more prone to use force, whereas leaders with combat experience exhibit a greater aversion to risk. Yet, this is only true of elected leaders: in authoritarian (especially military) regimes, leaders with combat experience tend to display a more aggressive behavior, because in such regimes, individuals who react to experiencing war by becoming more risk acceptant are also more likely to be selected as leaders. Interestingly, the same pattern holds true for regimes led by former guerilla leaders.

A different but related research agenda examines the relationship between authoritarian regime types – based on Geddes' (1999) regimes classification – and interstate conflict. While the majority of works have focused on the personalism dimension and institutional constraints on dictators (e.g. Peceny et al. 2002; Peceny and Beer 2003; Peceny and Butler 2004; Reiter and Stam 2003; Pickering and Kisangani 2010), another emerging consensus

is that, on the whole, military regimes tend to be more belligerent than civilian dictatorships. Lai and Slater (2006) conducted the first cross-national quantitative analysis on military dictatorships' conflict behavior and found these to be more likely to initiate MIDs than civilian autocracies, regardless of their degree of personalization. These findings were mostly corroborated in a subsequent study by Weeks (2012).

Both contributions rely on broadly similar conceptualizations (i.e. the quadripartite classification of dictatorships into "strongmen," "bosses," "machines" and "juntas" regimes) but come to slightly different results: Lai and Slater (2006) find that all types of military regimes display a comparatively high likelihood to initiate MIDs, whereas in Weeks' (2012) contribution, only party-based dictatorships emerge as a relatively peaceful regime type. This difference in findings can partly be explained by the fact that both articles focus their analytic lens on different institutional traits. Weeks contends that military officers' socialization and specific beliefs about the use of force are the primary cause of conflict initiation, and does not devote much attention to military regimes' vulnerability to coups. Neither does she control for coup risk in her empirical analysis. By contrast, military dictatorships' vulnerability to both elite splits and mass unrest is one of the main causes underlying their conflict behavior in Lai and Slater's contribution. Yet, regime classifications are not particularly appropriate measures of political instability. Furthermore, Lai and Slater locate the source of military regime's instability in the absence of political institutions – especially parties – that would help them maintain social control and elite cohesion, thereby ignoring the fact that military "strongmen" frequently create political parties to rally supporters (Geddes et al. 2014, 158). Thus, which causal mechanism underlies military regimes' tendency to provoke interstate conflicts is still subject to discussion.

The major flaw in both contributions is that their respective approaches rely on rigid classifications, thus overlooking variations within and similarities across regime types. For example, political instability, legitimacy issues and coup risk are no defining features of a particular regime, even if these traits happen to add up in military dictatorships. Furthermore, the military's influence on politics is an attribute that can vary both within and across regime types: the military can veto foreign policy decisions even when it is not formally part of the ruling coalition and when the dictator is a civilian (Dassel and

Reinhardt 1999). Similarly, a dictator with a military background can coexist with a relatively strong or autonomous military that is not part of his ruling coalition, but whose support is crucial for his survival and that may turn against him as soon as its interests are threatened (e.g., the Indonesian military under Suharto; see Lee 2005). Thus, more specific measures are needed to assess the relative impact of the leader's affiliation to the military, the vulnerability of the regime, and the military's own preferences. These issues are addressed in the following sections.

### **9.3. POLITICAL INSTABILITY, LEADERS' AFFILIATION, AND MILITARY INFLUENCE ON POLITICS: UNPACKING MILITARY DICTATORSHIPS**

#### **9.3.1. The impact of domestic instability on the initiation of interstate conflicts**

In line with previous research on the conflict behavior of dictatorships (e.g., Belkin and Schofer 2005; Miller and Elgün 2011; Powell 2014b), I expect coup risk to be one of the most powerful domestic factors driving diversionary wars. While economic crises (and other factors triggering mass discontent) may lead democratically elected leaders to initiate interstate conflicts, these incentives are probably not sufficient for dictators, whose political survival does not depend on the approval of the voters. In order to stay in office, dictators must instead adapt their policies to the preferences of their ruling coalition (Weeks 2012) or attempt to divert or weaken their potential challengers. One of the most serious threats to the dictator's rule is the coup d'état: coups are both more frequent and more likely to succeed than, for example, revolutions or civil wars. Indeed, the overwhelming majority of dictators who lose office in an irregular manner are removed by a coup launched by members of the ruling elite, often with the active support of the military (Svolik 2012; Kendall-Taylor and Frantz 2014). I argue that the main objective of dictators who initiate interstate wars is not to distract their potential challengers' attention from domestic issues, but to deprive them of the potential support of the military (or, when the potential challenge comes from the military itself, to insulate themselves from the threat).

Most works on political instability and interstate conflict either focus on mass challenges to the regime, or treat mass unrest and elite unrest as identical phenomena. For example, Mansfield and Snyder (1995, 2002, 2005) argue that states that undergo a transition to democracy (i.e., those that first experience political competition while still lacking strong institutions to regulate mass participation) are more prone to war, because competing elite groups have an incentive to mobilize mass support through nationalist appeals. Davies (2002) and Sirin (2011) find that violent domestic strife leads incumbents to provoke interstate conflicts in the hope of rallying mass support. Lai and Slater (2006) make a similar point by arguing that instability is the primary cause of military regimes' higher propensity to initiate MIDs: they posit that military regimes are vulnerable to challenges from both masses and members of the ruling elite, without yet undertaking a clear-cut distinction between both mechanisms. However, in spite of their popularity, theories linking mass unrest to the initiation of MIDs lack empirical support (see Gleditsch et al. 2008) and suffer from a number of flaws that have already been extensively discussed in the literature. First, since initiating a war abroad is a risky strategy, the dictator must face a serious threat of losing office (Powell 2014b); yet, few dictators are removed from office as a result of a revolt (Svolik 2012). Second, as Dassel and Reinhardt (1999) convincingly argue, states plagued by civil strife are precisely the ones in which the military has the most influence on politics (and is therefore able to veto any foreign policy decision), because of the dictator's reliance on his armed forces to maintain order. Third, since the military can hardly be used simultaneously for internal repression and external wars, mass unrest should lower rather than increase the dictator's incentives to allocate his military resources abroad (Andreski 1980).

By contrast, the mechanisms driving the diversionary use of force become clear when the main threat to the dictator's rule comes either from dissident factions of the ruling elite or from the military itself. First, by withdrawing potentially dissident factions of the armed forces from the country, the dictator can effectively prevent them from overthrowing him or backing up his challengers (Tullock 1987). Second, planning an intervention abroad diverts military officers' attention away from domestic issues (Lai and Slater 2006, 121). Third, by promoting rally effects, the dictator can increase his own popularity and deter the military from staging a coup (Arbatli and Arbatli 2014). Fourth, as Belkin and Schofer

(2005) have demonstrated, interstate conflicts tend to weaken the military by exacerbating internal divisions and rivalries and by impeding coordination between different branches. Fifth, involvement in an interstate war enables the dictators to credibly commit to making money transfers and political concessions to the military: in the absence of serious security issues, the risk that the dictator reneges on his promises increases, which creates incentives for the military to take preemptive measures (Arbatli and Arbatli 2014). Finally, a military defeat can deprive the military from public support, thus preventing it from taking over the state. In sum, interstate conflicts reduce both the military's willingness and its capacity to stage coups, hence helping vulnerable dictators insulate themselves from threats coming from their own armed forces. Therefore, we should observe that, as coup risk increases, so does the dictator's incentive to initiate a MID.

*H1: Vulnerable dictators are more likely to initiate a militarized interstate dispute.*

### **9.3.2. Ruling out alternative explanations**

As already discussed, high coup risk is not the only specific feature of military dictatorships, and there are at least two other factors that may account for their aggressive foreign policy: military regimes' propensity to initiate interstate conflicts might either result from the central position of the military within the political system, or from the fact that they are ruled by a leader with a background in the armed forces.

Let us first examine the first potential confounder, namely the influence of the military on politics: can the military's specific preferences regarding the use of armed force account for military regimes' aggressive foreign policy? While common sense suggests that military and civilian elites think differently about the costs and benefits of violence, the literature is divided between theories of "military conservatism," which hold that professional soldiers are generally more reluctant to initiate wars than civilian elites, and theories of "militarism," which hold that military officers have a comparative advantage in the use of force and hold specific beliefs that lead them to favor open conflict over negotiation (for a review of the different arguments, see Sechser 2004). On the whole, the empirical literature provides support for both approaches, although recent scholarship on

conflict initiation by dictatorships rather tends to emphasize the second one (Sechser 2004; Weeks 2012). Both theories are not fundamentally incompatible, though: military officers may favor open conflict over negotiation as long as their organizational interests are not directly threatened (e.g., when they perceive the potential costs of the conflict as low or the probability of winning as high). By contrast, they should be more hesitant than civilians to initiate conflicts they evaluate as costly or impossible to win: in most cases, it is the military that has to bear the costs of war.

Precisely because the stakes of international war are higher for the military than for civilian elites, I adopt the widespread assumption that the military's preferences regarding the use of force diverge strongly from those of dictators. Yet, I depart from previous research in the sense that I expect military officers to have a more cautious attitude toward armed conflict than authoritarian elites in general. While I do not contest the argument that civilian control over the military reduces states' propensity to initiate conflicts abroad *as long as the regime is democratic* – that is, if leaders are otherwise constrained by both their electorate and horizontal accountability institutions, and cannot unilaterally provoke international wars – I argue that this argument is flawed when applied to dictatorships. In most nondemocratic regimes, the military is one of the few actors who can effectively constrain the leader: in those regimes, the fact that the military is submitted to strong civilian control and lacks autonomy or influence on policies is not necessarily good news. Although I do not contend that the military has a systematic preference for peace, I argue that the consequences of military influence (i.e., whether military influence increases or decreases the risk of conflict initiation) should be evaluated relative to regimes' baseline risk of initiating a conflict. In other words, while military influence may increase the risk of MID initiation when the leader is moderate or constrained by other actors (i.e. in democracies), it should reduce dictators' propensity to engage in external conflicts, especially when those display an otherwise high conflict risk. While democracies tend to avoid initiating wars they cannot win, that could result in too many casualties or that may drag on for too long (see Reiter and Stam 2002), dictators are notoriously less risk averse: when they have nothing to lose, leaders may initiate conflicts regardless of the costs or probability of winning, as the Ugandan-Tanzanian war discussed in the introduction exemplifies. In such cases, a powerful military should reduce the risk that a dictatorship initiates an interstate conflict.

*H2: The influence of the military on domestic politics reduces dictators' propensity to initiate interstate armed conflicts.*

A second potential confounding factor is the military background of the dictator himself. Most existing works on the conflict behavior of military leaders do not operate a systematic distinction between the perceptions and preferences of the military as a whole and those of military dictators. Yet, while the military as an organization and leaders with a background in the armed forces may share the same beliefs about the effectiveness and desirability of armed conflict, the risk aversion hypothesis should only apply to military officers who are directly exposed to the costs and consequences of war, which is not the case of the leader. Thus, the preferences of the military regarding wars can diverge from those of the dictator even if he is himself a member of the armed forces. In empirical terms, this implies that, when the influence of the whole military on policies is held constant, the marginal effect of the leader's identity on MID initiation should be positive.

There is another, slightly different reason why we should expect military dictators to be more likely to favor aggressive foreign policies. Horowitz and Stam (2014) argue that the process through which leaders are selected into office can vary considerably from one regime to another and consequently affect the likelihood that incumbents generally favor violence over peaceful conflict settlement: in military regimes, individuals who self-select into leadership positions are generally more likely to display extremely aggressive attitudes, because risk acceptance and propensity to violence are requisites to access political power (Horowitz and Stam 2014, 535). While this holds true for most dictatorships – in which leadership is a risky business in general – selection biases in favor of aggressive individuals should be especially strong in military regimes, given that these regimes are frequently established through violent means: while a non-negligible proportion of civilian leaders access power through regular means (typical examples include elections or hereditary succession), most military leaders either access power via a coup or are selected for succession by a group of military officers (see descriptive statistics section below), who are themselves more likely to favor risk-acceptant behaviors. Thus, even if the dictator himself accessed power through nonviolent means, a military leader is

more likely to display a comparatively high propensity to violence because such attitudes are what got him co-opted by the junta in the first place.

*H3: Leaders with a background in the armed forces are more likely to initiate militarized interstate disputes.*

#### **9.4. RESEARCH DESIGN AND VARIABLES MEASUREMENT**

I test my hypotheses on a time-series cross-sectional dataset that covers more than 100 nondemocratic countries. For reasons related to data availability, democracies are excluded from the analysis (see section 9.4.2. below). Data on the dependent and the independent variables are available from 1946 to 2010 and from 1975 to 2008, respectively. Since my hypotheses are monadic, I use the country-year as my unit of analysis.

I use logistic regression on a dummy variable that measures whether each state initiated a MID in a given year. To address time-dependence issues, I add a variable that records the years since the last time the country initiated a MID, and use robust standard errors clustered by country.

Detailed information on variables measurement and descriptive statistics are provided below.

##### **9.4.1. Dependent variable**

To measure my dependent variable, I rely on the Militarized Interstate Disputes dataset from the Correlates of War (COW) project (Palmer et al. forthcoming). MIDs are classified into five categories according to their intensity level: only Levels 4 and 5 involve the actual use of military force. Since my hypotheses relate to actual militarized action, I thus exclude the lower levels – that is, Level 1 (no military action), Level 2 (threat to use force) and Level 3 (display of military force): for diversion to be effective (Hypothesis 1), the military has to be effectively (i.e., physically) deployed abroad. Hypothesis 2 states that the military has an incentive to oppose wars, but says nothing about the military's preferences about

lower-scale conflicts. Only the “military leader” variable can be to some extent expected to have a constant impact across all hostility levels.

Thus, the dependent variable takes on the value of 1 if the MID reaches a hostility level of at least 4 (use of force) and the country is listed on the initiators side (Side A), 0 otherwise. Only the first year of the dispute is reported: ongoing dispute years are coded 0. If the country initiated several MIDs within the same calendar year, I collapse these into a single value. For the period from 1981 to 2006, 471 MIDs initiations, including 16 wars (Level 5) were reported, for a total of 4,322 observations.

#### **9.4.2. Independent variables**

The first independent variable of interest simply measures whether the country is ruled by a military officer or a civilian dictator. The dummy variable takes on the value of 1 if the chief executive is a military officer, 0 otherwise, and is drawn from the Database of Political Institutions (Beck et al. 2001). I expect this variable to affect positively states’ propensity to initiate MIDs.

Measuring coup risk is somewhat trickier. One of the few consensual measures of coup risk is the one developed by Belkin and Schofer (2003), which has been subsequently used in quantitative studies on coup risk and conflict initiation (Belkin and Schofer 2005; Powell 2014b). However, their assessment of coup risk relies on an index that includes components only loosely related to coups, such as INGOs membership or the degree of competitiveness and regulation of participation in the political system. I therefore turn to an alternative measure, based on Svolik’s (2012) data on ruling coalitions and leadership change in dictatorships. The dataset includes a variable (“entry”) that records the manner each leader assumed office.<sup>143</sup> To measure coup risk, I create a dummy variable that takes on the value of 1 if the leader accessed power by regular means (i.e. elections, consensus, or hereditary succession), 0 otherwise.<sup>144</sup> I expect the variable to have a negative effect on

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<sup>143</sup> Svolik’s original data are in leader-year format. Since I have a country-year dataset, for each country-year in which several leaders were reported, I selected only the leader in office on the First January in order to avoid reverse causation issues. The DPI similarly reports leaders who were in office on the First January.

<sup>144</sup> Irregular entry includes entry types labeled as “civil war”, “coup”, “foreign”, “independence”, “revolt”, “interim”, and “other”. The results are not sensitive to the removal of “interim” and “other” from the category of irregular entries.

conflict initiation. This measure presents two advantages. First, it is a more precise measure of a dictator's risk of being removed from office in an irregular way, and makes it possible to test directly whether conflict initiation results from the preferences of authoritarian elites coalitions (Weeks 2012), from unilateral decisions of the military (Dassel and Reinhardt 1999), or – as I expect – from the leader's own sense of insecurity. Second, the measure does not systematically overlap with the military's veto power (which would be an alternative explanation for unstable regimes' propensity to initiate MIDs), since a non-negligible part of military leaders accessed power through regular ways (see descriptive statistics section below) and, conversely, the military is not systematically involved in irregular entries.

To assess whether the military is a powerful actor and can exert some influence on foreign policy decisions, I use a variable – also drawn from Svoboda's (2012) leaders and ruling coalitions data – that measures whether the military was actively involved in the leader's entry into office. This variable is intended to reflect ex ante constraints from the military on the dictator: irrespective of the leader's affiliation and regardless of whether the military is part of the ruling coalition or not, it can at least be expected that the dictator is to some extent sensitive to the military's preferences, and that the military has a say in foreign policy decisions. Thus, this measure of military influence is broader than standard measures based on regime type or civilian control over the military (see, e.g., Sechser 2004; Lai and Slater 2006; Weeks 2012), while still allowing me to distinguish dictators who can compel their militaries to fight wars from those who cannot.<sup>145</sup> Note also that military influence is conceptually as well as empirically distinct from coup risk (see descriptive statistics section below): military influence means that the military can obtain some policy concessions from the leader, without necessarily representing a direct threat to his rule.

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<sup>145</sup> As noted above, one of the strongest cases against diversion theories in Dassel and Reinhardt's (1999) study is that previous research in this field is mostly based on the untenable assumption that every dictator can unilaterally order its military to fight wars. Yet, their own article is itself based on a premise – that no dictator has this capacity – that is also somewhat unrealistic.

### 9.4.3. Control variables

I use control variables that are standard in the interstate conflict literature. First, I include a count of the total number of contiguous borders of each state by land, based on the Direct Continuity dataset from the COW project (Stinnett et al. 2002). I then control for military capabilities using countries' scores on the Composite Index of National Capabilities (CINC) from the COW project (Singer et al. 1972), and for country size using World Bank data. I further control for GDP per capita, since it can affect both regime instability, regime type and conflict behavior. Data are taken from the Maddison Project Database (Bolt and van Zanden 2013).

I then add several control variables that are potentially correlated with both regime type and foreign policy behavior. I first include a variable that records the number of years the leader has already been in office: past research suggests that the risk of losing office decreases throughout the leader's tenure (Bienen and van de Walle 1989; 1992) and that the average tenure of the chief executive is shorter in military regimes than in civilian regimes (McKinlay and Cohan 1975). Second, I control for intrastate armed conflicts,<sup>146</sup> using data from the UCDP/PRIO Armed Conflict Dataset (Gleditsch et al. 2002; Harbom and Wallensteen 2010). The variable takes on the value of 1 when an armed conflict resulting in at least 25 battle-related deaths is reported in the country-year, 0 otherwise.<sup>147</sup> Domestic strife has regularly been found to be a robust predictor of the initiation of interstate conflicts (Dassel and Reinhardt 1999; Davies 2002; Cirin 2011); thus, the relationship between regime type and external conflict behavior could possibly be driven by the fact that military dictatorships tend to experience more intrastate armed conflicts (see Fjelde 2010). Furthermore, controlling for intrastate armed conflict makes it possible to undertake a distinction between two different mechanisms that can both explain why unstable dictatorships initiate more MIDs: if diversionary conflicts are initiated in response to mass unrest and coup risk is only an epiphenomenon of instability, the effect of coup risk should vanish once armed conflicts are controlled for. By contrast, if coup risk remained a

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<sup>146</sup> I do not include classical indicators of mass discontent (e.g., slow growth): as argued above, dictatorships are not bound to their electorate as democracies are, and I do not expect dictators to run the risk of losing a war unless the risk of losing office becomes really serious.

<sup>147</sup> To avoid confusing armed conflicts and military coups, I drop every country-year in which both a coup and an ongoing armed conflict are reported (see Chapter 4).

significant predictor of MID initiation after controlling for armed conflict, this would indicate that the incumbent's foreign policy behavior is not necessarily oriented toward the voters.

I then control for coup-proofing measures, measured as the annual change in military expenditures per soldier (see Powell 2014b). Data on military personnel and military expenditures are taken from the National Material Capabilities dataset from the COW project (Singer et al. 1972). The omission of this variable could bias the results to the extent that the financial endowment of the military is related both to military influence on policies and to conflict initiation: on the one hand, Powell (2014b) argues that coup-proofing and diversion are substitutable strategies in dictatorships, and consistently finds that autocrats' propensity to initiate MIDs decrease as the financial endowment of their military increases. On the other hand, research also suggests that the leader's ability to implement coup-proofing measures partially depends on regime type (Pilster and Böhmelt 2012): even though there is to date no clear evidence that military-backed regimes spend more on their militaries than other regime types (see, e.g., McKinlay and Cohan 1975; 1976), it can be assumed that – unlike civilian autocracies – they are at least limited in their ability to decrease the armed forces' budget. Therefore, to the extent that military influence actually reduces the risk that a state initiates interstate conflicts, this relationship could simply result from the fact that the military's financial endowment is higher when military influence on policies is high, resulting in a lower coup risk. Since countries with weak militaries are *ceteris paribus* less likely to initiate MIDs, I also add a measure of military expenditures per soldier.

For a similar reason, I also control for repression: military dictatorships have been found to be internally more repressive (Poe et al. 1999; Davenport 2007b), and there is some evidence that repression – as an alternative to diversion – negatively affects the risk that a state initiates a MID (Gelpi 1997; Davies 2014). If military dictatorships tend to use both internal and external violence to deal with instability, omitting this variable could mask important statistical trends. To measure state repression, I use the physical integrity rights index from the Cingranelli and Richards (CIRI) data project (Cingranelli and Richards 2010). The index ranges from 0 to 8, low values indicating widespread physical integrity rights violations (such as political imprisonment, torture or extrajudicial killings) and high

values good human rights performance.

**Table 9.2. Summary statistics on control variables, 1975-2006**

	<i>Mean</i>	<i>Standard deviation</i>	<i>Observations</i>
CINC	0.01	0.02	4,842
Leader's tenure	11.19	9.29	2,758
Land borders	3.56	2.42	4,842
(ln) Country size	12.18	1.86	4,796
(ln) GDP/capita	8.04	1.11	4,617
Phys. integrity rights	4.75	2.33	3,706
Exp./soldier (USD)	25,613.98	66,771.45	4,517
$\Delta$ exp./soldier	1,334.02	46,845.54	4,404

#### 9.4.4. Descriptive statistics

This section presents an overview of the distribution of the independent variables (military leader, military influence, and coup risk).

From 1975 to 2008, 90 military dictators and 165 civilian dictators assumed office, representing respectively about 35% and 65% of the country-years. Roughly 50% of all dictators from the sample accessed office in a regular way, i.e. either by elections, hereditary succession or consensus. The military participated in the entry of 98 out of 255 dictators. Although the sample is evenly shared between regular and irregular entries, the distribution of the variable varies greatly between the categories of military and civilian leaders. Entry modes of civilian and military leaders are detailed in Table 9.3.

The modal entry type for military dictators is the coup d'état (60% of the sample), followed by consensus (about one quarter of the sample). By contrast, only 20% of civilian dictators accessed power via a coup. The most common entry type is consensus (roughly one third of the sample), followed by elections (25% of the sample). On the whole, military leaders are much more likely to have accessed power by force than civilian leaders, although a substantial part of the latter (about 35%) entered office through irregular means.

**Table 9.3. Entry mode of military and civilian dictators, 1975-2008**

	<i>Military</i>		<i>Civilian</i>	
	<i>Frequency</i>	<i>Percent</i>	<i>Frequency</i>	<i>Percent</i>
Election	5	5.56	42	25.45
Consensus	21	23.33	54	32.73
Succession			10	6.06
<i>Total: regular entry</i>	<i>26</i>	<i>28.89</i>	<i>106</i>	<i>64.24</i>
Civil war	5	5.56	5	3.03
Coup	54	60.00	33	20.00
Revolt			1	0.61
Foreign	1	1.11	4	2.42
Interim	3	3.33	11	6.67
Other	1	1.11	5	3.03
<i>Total: irregular entry</i>	<i>64</i>	<i>71.11</i>	<i>59</i>	<i>35.76</i>
Total	90	100	165	100

(Sources: Svolik 2012; Database of Political Institutions)

As can be seen in Table 9.4, irregular entry strongly increases the risk of an irregular exit (see also Goemans et al. 2009a).

**Table 9.4. Entry type and exit type in dictatorships, 1975-2008**

	<i>Regular exit</i>		<i>Irregular exit</i>		<i>Other</i>		<i>Total</i>	
	<i>Freq.</i>	<i>Percent</i>	<i>Freq.</i>	<i>Percent</i>	<i>Freq.</i>	<i>Percent</i>	<i>Freq.</i>	<i>Percent</i>
Regular entry	68	31.19	39	17.89	1	0.46	108	49.54
Irregular entry	24	11.01	70	32.11	16	7.34	110	50.46
Total	96	42.20	109	50.00	17	7.80	218	100

(Source: Svolik 2012)

On the whole, roughly 50% of the dictators accessed power by force, and exactly 50% of the sample is comprised of dictators who lost office by force. Yet, the two modal categories are made up of dictators who either accessed office by force and lost it by force (70 cases)

or accessed power in a regular way and lost office in a regular way (68 cases). Taken together, both categories represent about two thirds of the sample. Of the 110 dictators who accessed power in an irregular way, only 24 managed to exit office without violence; conversely, from the 108 dictators who accessed power by regular means, only 39 lost office irregularly.

Thus, previous findings on military dictatorships and the initiation of MIDs may result from the fact that many military dictators, having accessed power in an irregular way, fear a violent removal from office: compared to civilian dictators, military leaders are more likely to have entered power irregularly, and irregular entry is strongly related to irregular exit. Yet, since entry mode and military leader are only imperfectly correlated, their respective effects can be empirically parsed out: if Hypothesis 1 is valid, the way the leader assumed office should impact the risk of conflict initiation regardless of the leader's affiliation (i.e., all dictators who fear for their seat should be more likely to initiate a MID, regardless of whether they are military or civilian).

Table 9.5. displays a comparison of the military's involvement in the entry of military and civilian leaders, respectively.

**Table 9.5. Military participation in civilian leaders' and military leaders' entry, 1975-2008**

	<i>Military leader</i>	<i>Civilian leader</i>	<i>Total</i>
Military involvement:			
Yes	68	30	98
No	22	135	157
<i>Total</i>	<i>90</i>	<i>165</i>	<i>255</i>

(Sources: Svobik 2012; Database of Political Institutions)

The military was overtly involved in the entry of 98 out of 255 dictators, most of whom were military officers. Yet, 30 out of the 98 military-backed dictators are civilian leaders. Furthermore, 22 out of 90 military leaders accessed office without active support from the military. Again, since the leader's affiliation and the support from the military are only imperfectly correlated, the effects of both factors can be distinguished: if Hypothesis 3 is valid, military leaders should be more likely to initiate MIDs, but for reasons unrelated to

the preferences of the whole military. If Hypothesis 2 is valid, the military's influence on policies should be negatively related to conflict initiation, regardless of whether the dictator is military or civilian.

## 9.5. EMPIRICAL ANALYSIS

I begin with a simple model that includes only the independent variables of interest<sup>148</sup> and a variable that reports the number of years passed since the last time the country initiated a MID, in order to minimize temporal dependence issues. Results are displayed in Table 9.6, Model 1.

Consistently with Hypothesis 1, leaders who accessed office in a regular way are substantively less likely to initiate a MID. Their odds of initiating a MID is about 59% as high as those of leaders who seized power irregularly. The variable is significant at the 1% level. Regimes led by a military officer appear to be more likely to initiate a MID. However, the effect is not statistically significant: so far, Hypothesis 3 is not supported. Finally, the influence of the military seems to reduce the risk that the leader initiates a MID: leaders supported by the military are about 65% as likely to initiate a MID as other dictators. The variable is significant at the 10% level. In sum, preliminary estimations lend some support to Hypotheses 1 and 2.

Model 2 includes all appropriate controls.<sup>149</sup> The effect of coup risk remains large and strongly significant: the regular entry dummy reduces the estimated risk of conflict initiation by more than half.<sup>150</sup> Military influence also strongly decreases the likelihood that a country initiates a MID, military-backed autocrats being only 40% as likely to get involved in a MID as autocrats who are free from military control. The variable is

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<sup>148</sup> VIF tests as well as correlations matrices indicate that multicollinearity is not a concern.

<sup>149</sup> Since the CRI data are only available from 1981 onward, the sample size shrinks substantially. Nevertheless, when the variable is removed from the model, results concerning military influence and irregular entry hold up, both in terms of magnitude and statistical significance. The only notable change is that the military leader variable becomes marginally significant when physical integrity rights are not controlled for. Yet, the effect is not robust to alternative specifications.

<sup>150</sup> Results regarding coup risk are robust to alternative measurements of the variable. Including "interim" and "other" in the category of regular entries or replacing the variable with a dummy indicating whether the leader entered office via a coup do not substantially alter the results. When the impact of each entry mode is analyzed, it appears that elections and hereditary succession strongly decrease the risk of MID initiation, whereas leaders who seized power through coup or a rebellion are especially likely to initiate interstate conflicts.

significant at the 1% level. So far, Hypotheses 1 and 2 are supported. Yet, the military leader dummy still fails to attain conventional levels of statistical significance: Hypothesis 3 can be rejected.

**Table 9.6. Determinants of militarized interstate disputes, 1975-2006 (logistic regression, reporting odds ratios)**

	(1) <i>Basic model</i>	(2) <i>Full model</i>	(3) <i>Coup risk</i>	(4) <i>Military leader</i>	(5) <i>Military influence</i>	(6) <i>Coup risk + mil. influence</i>
Peace duration <sup>a</sup>	0.896*** (0.013)	0.950*** (0.0150)	0.944*** (0.0152)	0.943*** (0.0155)	0.942*** (0.0153)	0.948*** (0.0148)
Military leader	1.415 (0.327)	1.533 (0.436)		1.336 (0.290)		
Military influence	0.653* (0.152)	0.417*** (0.104)			0.906 (0.166)	0.519*** (0.124)
Regular entry	0.588*** (0.118)	0.473*** (0.108)	0.653** (0.110)			0.437*** (0.0923)
Country size <sup>b</sup>		1.144 (0.0959)	1.100 (0.0960)	1.076 (0.0924)	1.093 (0.0915)	1.151* (0.0958)
Exp./soldier <sup>a</sup>		1.000** (3.03e-07)	1.000*** (2.61e-07)	1.000*** (2.46e-07)	1.000*** (2.40e-07)	1.000** (2.76e-07)
Δ Exp./soldier <sup>a</sup>		.999 (2.68e-07)	.999 (2.55e-07)	.999 (2.42e-07)	.999 (2.59e-07)	.999 (2.75e-07)
Armed conflict <sup>a</sup>		2.344*** (0.496)	2.345*** (0.509)	2.421*** (0.516)	2.389*** (0.516)	2.291*** (0.488)
CINC <sup>a</sup>		0.596 (1.754)	5.974 (16.82)	18.65 (53.27)	8.931 (24.56)	0.442 (1.282)
Leader's tenure		0.984 (0.0104)	0.988 (0.0119)	0.989 (0.0118)	0.991 (0.0121)	0.985 (0.0109)
Contiguity (land)		1.054 (0.0434)	1.046 (0.0431)	1.049 (0.0435)	1.039 (0.0422)	1.045 (0.0427)
GDP/capita <sup>a, b</sup>		1.205 (0.172)	1.164 (0.162)	1.124 (0.171)	1.072 (0.156)	1.177 (0.160)
Phys. integrity rights <sup>a</sup>		0.813*** (0.0457)	0.836*** (0.0463)	0.833*** (0.0463)	0.830*** (0.0454)	0.819*** (0.0446)
Constant	0.440*** (0.096)	0.0234** (0.0390)	0.0361* (0.0613)	0.0428* (0.0764)	0.0622 (0.106)	0.0310** (0.0493)
Countries	124	109	109	109	109	109
Observations	2,856	1,535	1,536	1,535	1,536	1,536

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

<sup>a</sup>Lagged one year <sup>b</sup>Log

On the whole, control variables have the expected sign. Peace years have a consistently

negative and strongly significant effect on the dependent variable (Models 1 and 2). As can be seen in Model 2, larger countries tend to initiate more MID, but the effect is neither strong nor statistically significant. Military expenditures are positively correlated with MID initiation. As expected, change of expenditures per soldier decreases the country's probability of initiating a MID, but the variable fails to pass the 10% significance threshold. Leader duration also has the expected sign but falls short of statistical significance.

Two interesting results can be observed. First, interstate armed conflict strongly and significantly increases the risk of MID initiation, regardless of whether coup risk is controlled for. Conversely, coup risk remains statistically significant even after armed conflict is included in the model. Thus, both types of contestation impact external conflict independently from each other: this suggests that there are several causal relationships between domestic political instability and interstate conflicts, and that both mechanisms should be distinguished and investigated separately.

Second, physical integrity rights have a strong, negative and statistically significant effect on the likelihood that a country initiates a MID in the following year. In other words: repression is positively related to the risk of conflict initiation. Although this finding could result from reverse causation issues,<sup>151</sup> it indicates at least that both strategies are neither incompatible (Andreski 1980) nor substitutable (Gelpi 1997).

I then assess the impact of each independent variable (military leader, military influence, and coup risk) separately. Results reported in Model 3 show the influence of coup risk on MID initiation: consistently with Hypothesis 1, leaders who accessed power by regular means are substantively less likely to initiate a MID. Their odds of initiating a MID is about 65% as high as those of leaders who accessed office by irregular means. The variable is significant at the 5% level.

As can be seen in Model 4, regimes led by a military officer appear to be more likely to initiate a MID. However, the effect is not statistically significant: so far, Hypothesis 3 is not supported.

The influence of the military seems to reduce the risk that a leader initiates a MID: this lends support to Hypothesis 2. The effect appears to be weak and statistically insignificant

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<sup>151</sup> Note, however, that the effect of the variable remains significant when it is lagged up to seven years.

in Model 5; however, the variable attains significance and grows in magnitude as soon as the leader's entry mode is controlled for. In Model 6, military entry appears to reduce the risk of interstate conflict by half.

On the whole, control variables have the expected sign and their effects remain stable across models. Peace years have a consistently negative and strongly significant effect on the dependent variable. Larger countries tend to initiate more MIDs, but the effect is rather weak and the variable is only marginally significant in Model 3. Military expenditures are positively correlated with MID initiation. As expected, change of expenditures per soldier decreases the country's probability of initiating a MID, but the variable systematically fails to pass the 10% significance threshold. Leader duration also has the expected sign but falls short of statistical significance in all models.

I then perform several robustness checks. First, to check whether the main results are sensitive to the inclusion or removal of insignificant controls, I run a more parsimonious version of the full model, that only includes the control variables that were significant in Model 2. Results are displayed in Table 9.7., Model 7.

Regular entry and military influence still have a strong, negative and statistically significant effect on the dependent variable, although the magnitude of these effects decreases slightly. With regard to military leaders, the variable is again positively correlated with the initiation of MIDs but statistically insignificant. The estimations also show that results regarding control variables are consistent across models: intrastate armed conflict, repression, and expenditures per soldier still increase the risk that a country initiates a MID, while peace years remain negatively correlated to the dependent variable. All variables in the model are statistically significant.

Second, I check for the robustness of the findings by including country fixed effects (Model 8) and country and year fixed effects (Model 9). In order to maximize the number of observations, I only include control variables that were significant in previous models. Although fixed effects models are conservative, results concerning military influence and coup risk remain basically unaltered, dictators being about three times as likely to initiate MIDs when they are not constrained by their militaries, and more than twice as likely to engage in interstate conflict when coup risk is high.

**Table 9.7. Determinants of militarized interstate disputes, 1981-2006 (fixed effects logit, reporting odds ratios)**

	(7)	(8)	(9)
Peace duration <sup>a</sup>	0.938*** (0.0128)	1.089*** (0.0211)	1.084*** (0.0214)
Military leader	1.231 (0.309)	2.140* (0.952)	2.082 (0.936)
Military influence	0.494*** (0.122)	0.328** (0.143)	0.336** (0.151)
Regular entry	0.586** (0.134)	0.420** (0.155)	0.482* (0.189)
Exp./soldier <sup>a</sup>	1.000** (4.32e-07)	1.000 (9.77e-07)	1.000 (1.01e-06)
Armed conflict <sup>a</sup>	2.348*** (0.423)	3.902*** (1.182)	3.933*** (1.227)
Phys. integrity rights <sup>a</sup>	0.810*** (0.0408)	0.893 (0.0641)	0.858** (0.0653)
Constant	0.585* (0.170)	0.0277*** (0.0296)	0.0363*** (0.0424)
Country FE	.	YES	YES
Year FE	.	.	YES
Countries	115	64	64
Observations	1,751	1,065	1,065

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

<sup>a</sup>Lagged one year

Results of fixed effects models have to be interpreted cautiously when the dependent variable entails a large proportion of zeroes: since the dependent variable does not vary in several countries and years, a large number of observations (almost 700) are lost and the sample is now restricted to 64 countries. Furthermore, some results cast doubt on the overall reliability of the estimates: peace years are now positively and significantly related to MID initiation. The physical integrity rights variable loses significance in Model 8, and in the same model, the military leader dummy passes the 10% level significance threshold; however, these results vanish in Model 9. In spite of these anomalies, results regarding Hypotheses 1 and 2 remain consistent across several specifications, which lends credence to the validity of the findings.

## 9.6. CONCLUSION

The results of the empirical analysis strongly confirm previous findings on coup risk and MID initiation (Belkin and Schofer 2005; Miller and Elgün 2011; Powell 2014b): dictators'

propensity to initiate interstate conflicts increases along with their risk of being violently removed from power. This is probably the main causal mechanism that underlies the empirical relationship between military regimes and interstate conflict: military leaders display a higher propensity to initiate interstate conflicts than their civilian counterparts, but the variable regularly fails to attain statistical significance; moreover, once leader identity and coup risk are controlled for, military influence on policies has a strong, negative effect on the probability of initiating a MID. These results are robust across several model specifications.

Thus, the empirical analysis discussed here solves a paradox while contributing to a persistent debate on the military's preferences about the use of force: the "military conservatism" thesis is perfectly compatible with the recurring finding that military dictatorships tend to initiate wars more often than authoritarian regimes led by civilians. While I do not contest the finding itself, I argue that it may be wrong to use this observation to draw inferences about the military's preferences and beliefs: military regimes are more belligerent *because* they run a higher coup risk, and *in spite of* the whole military's preferences. To the extent that previous research has uncovered a relationship between military influence and conflict initiation, my findings indicate that it is probably spurious: for example, neither Sechser (2004) nor Weeks (2012) did control for coup risk. My results are also at odds with Dassel and Reinhardt's (1999) argument, that diversionary wars are initiated by the military itself: states' propensity to initiate MIDs actually decreases as the military's influence on policies increases.

My findings also have broader implications for theories of diversionary wars in general. First, domestic factors matter: internal instability is a determinant of the use of violence abroad. Second, while I find evidence for diversion, my results suggest that the mechanisms driving the initiation of interstate conflicts differ across political regimes. Unlike democratically elected leaders, dictators are not bound to the voters: thus, they have to orient their strategies toward potential challengers from their inner circle. This is consistent with some patterns uncovered by the empirical analysis: the fact that repression does not reduce the likelihood that a state initiates a MID indicates that both strategies are not substitutable (i.e., that they are targeted at different groups). Similarly, the fact that civil unrest and coup risk both lead to MID initiation suggests that both factors impact foreign

policy through independent, theoretically distinct paths.

Finally, these findings highlight the need for precise measures of political variables: while regime classifications are useful shortcuts, they are not always appropriate to test hypotheses about specific causal relationships. Conflating distinct institutional features into a single measure can result in misleading findings, particularly when the phenomena under investigation are different analytically and in their consequences.

## 10

### **Conclusion: Out of the violence trap?**

The aim of this dissertation was to analyze the relationships between political regime type and violent conflicts. Considering the results as a whole, there are some good news and some bad news.

The bad news is that, in a nutshell, violence tends to breed violence: violent conflicts are not only enduring but also tend to beget other forms of conflicts through spillover effects. This general observation is not exactly new: for example, the impact of domestic turmoil on interstate conflicts (e.g., Dassel and Reinhardt 1999; Gleditsch et al. 2008) and the effect of state violence on rebellion (Moore 1998; Bell et al. 2013) have already been extensively discussed in the literature, and the relationship between coup risk and interstate conflicts has recently gained attention as well (Miller and Elgün 2011; Powell 2014b). Yet, the present work contributes to this research in two ways. From an empirical point of view, to the general observation that violent conflicts fuel each other, I add two new findings: first, Chapter 4 shows that a high risk of civil war and a moderate risk of interstate war make military coups more likely; second, as discussed in Chapter 8, widespread violations of physical integrity rights also increase coup risk. I also confirm one of the emerging findings of the international relations literature: coup risk increases leaders' propensity to initiate interstate conflicts (Chapter 9). Now, from an analytical point of view, the theoretical framework developed here makes it possible to organize this heterogeneous set of empirical findings – which have been treated so far in relative isolation from each other and remain to date fragmented – in a coherent way. One of the key themes developed in the

third part of this dissertation is that states do not react to threats coming from opposition groups and to those originating from the military in a similar manner: the typical response to mass dissent is repression, while the initiation of diversionary wars can be thought of as the incumbent's reaction to coup risk. Likewise, as shown in Chapter 4, internal and external security threats do not impact coup risk in the same way.

The good news is that specific institutional arrangements can alleviate at least some of the conflicts mentioned above. I find that two regimes are consistently less prone to violence. First, democracies are less likely to experience civil war (Chapter 5) and less likely to repress human rights to physical integrity (Chapter 8). This latter result is not trivial, since the measure of democracy I use does not include any indicator related to human rights protection: democracies achieve this better human rights performance through electoral accountability alone. Electoral competition also explains why democracies are less likely to experience civil war: because of procedures that allow for regular alternations in power, and because large groups – that is, those that could pose the most serious challenges to the state's authority – are precisely the ones that benefit the most from electoral competition, democracy generates strong incentives for everyone to abide by the rules instead of resorting to violence (Przeworski 1999). Chapter 7 brings a further confirmation to these results, as it demonstrates that civil war sometimes breaks out as a response to a mismatch between the numerical strength and the political influence of the main political groups.

Second, because hereditary succession represents a durable solution to succession problems and reduces the uncertainty that characterizes the relationships between the leader and his ruling coalition, monarchies are able to moderate power struggles and avoid coups (Chapter 5). Moreover, although they do not protect physical integrity rights as well as democracies do, monarchies are less likely to engage in widespread repression than other non-democratic regimes (Chapter 8) and they also experience fewer civil wars than non-institutionalized regimes (Chapter 5). Finally, I find tentative evidence that leaders who attained power by rule of hereditary succession are less likely to initiate international conflicts (Chapter 9). Given the small number of monarchies in the post-World War II period and the peculiarities of these countries, these results have to be considered with caution. Yet, the fact that the effect of monarchy on civil war also holds for the 19<sup>th</sup> century lends some credibility to the overall validity of the findings: the “monarchic peace” effect

at least seems to go beyond mere regional effects.

Thus, consistently with theoretical expectations, regimes that rely on formal, binding procedures for selecting the effective leader (that is, competitive elections in the case of democracies, and hereditary succession in the case of monarchies) are more peaceful in two respects: they experience less violence and they commit less violence. But what about regimes that lack such institutions? One key expectation of the dissertation was that, in non-institutionalized regimes, the identity of the leader and the type of organization on which he relies to maintain his rule would affect the regime's propensity to initiate or experience violent conflicts – since prior research indicates that authoritarian regimes differ systematically in their conflict proneness, civilian regimes (especially party-based ones) being both internally and externally more peaceful than military dictatorships (see, e.g., Peceny et al. 2002; Lai and Slater 2006; Davenport 2007b; Fjelde 2010; Weeks 2012). Are these expectations borne out?

The answer is a (very) partial “yes.” To begin, I find strong evidence that military dictatorships are more vulnerable to coups than any other non-institutionalized regime, especially party-based regimes (Chapter 5). This high coup risk certainly results from the distinctive characteristics of the military – contrary to civilian party cadres, military officers rarely share common policy objectives and often disagree about the nature and objectives of the regime, which makes military governments especially prone to internal splits and policy disputes – but might also be partly driven by the particularly toxic environment in which military dictatorships emerge (see Chapter 4).

Is that all? Prior research suggests that the effects of military government go well beyond internal factionalism: the idea that military officers are more inclined to use force than civilians is a recurring theme in the literature. A military career, so the argument goes, generates specific perception biases, military officers becoming more likely to view the use of force as both effective and legitimate, and to favor it over more peaceful ways of solving conflicts (see, e.g., Nordlinger 1977; Sechser 2004; Davenport 2007b; Fjelde 2010; Weeks 2012). A first consequence is that military dictatorships have been found to experience more civil wars (Fjelde 2010) and to commit more physical integrity rights violations (Poe et al. 1999; Davenport 2007b) than other types of dictatorship. Yet, I was not able to replicate any of these results. In fact, the discussions in Chapters 5 and 8 indicate that these

findings were mainly due to measurement errors in the dependent variables: after correcting for these errors, I do not uncover any empirical association between military dictatorship, on the one hand, and civil war and repression, on the other hand. Moreover, one of the key findings in Chapter 8 is that state repression actually increases the risk that the military turns against the incumbent and launches a coup. This result is consistent with early research on military coups – many authors such as Finer (1962), Needler (1975), and Nordlinger (1977) have consistently stressed military officers' deep reluctance to carry out domestic repression – but at odds with the increasingly widespread assumption that military officers systematically favor overt terror over peaceful conflict resolution.

Thus, on the whole, I do not find that civilian-led and military-led dictatorships differ systematically in their way of dealing with domestic conflicts. I do find, however, that military dictatorships are more likely to initiate militarized interstate disputes than any other regime (Chapter 9). Yet, this correlation is essentially a by-product of coup risk: after controlling for coup risk, I find that the influence of the military on domestic politics has a *negative* effect on the probability that a country initiates an interstate conflict. The fact that the effective leader is himself a military officer has no effect at all. Again, these results speak clearly against the widespread assumption that the use of violence is the military's natural modus operandi.

Thus, on the whole, authoritarian regime type has less explanatory power than previously assumed. But then again, this does not mean that political institutions cannot alleviate political conflicts: on the contrary, my findings point to a clear divide between political regimes in which institutions are binding and self-enforcing and those that lack such institutions. If anything, what the present work demonstrates is that the rules of the game matter more than the identity of those who play it.

Several qualifications must be added to these general conclusions. First, the present work suffers from some limitations from a theoretical point of view: although institutionalization has a strong explanatory power and explains much of the variation in political regimes' behavior, its effects are sometimes unexpected and rather surprising. For example, while the fact that monarchies are relatively immune to coups is perfectly consistent with the theoretical framework developed in Chapter 3, it is not so clear why they experience so few civil wars, and why they are systematically less repressive than non-institutionalized

dictatorships. One possible explanation is that stability and instability at the elite level have repercussions on the state's ability to deal with conflicts between elites and citizens, but understanding how coup risk and conflict risk reinforce each other would require further research. Conversely, one of the key expectations formulated in Chapter 3 is not borne out: in spite of the institutionalization of leadership selection, democracies are not immune to military coups. This expectation was the result of an attempt to endogenize coup risk: the fact that democracies do not experience fewer coups than non-institutionalized regimes demonstrates that the military's decision to overthrow a government is not necessarily related to system instability, and that other, exogenous factors might better account for the cross-national variations in coup risk.

A related issue is that the empirical category of non-institutionalized regimes possibly includes some institutionalized ones. The hypotheses as well as the definition and measurement of regime variables are based on the assumption that only competitive elections and hereditary succession can be viewed as binding, self-enforcing institutions. But this assumption may be incorrect: theoretically speaking, competitive elections and hereditary succession do not exhaust all possibilities, and there might well be other regimes that are based on self-enforcing rules for selecting and replacing the effective leader, that my measures of regime type did not adequately capture. For example, the central finding of Chapter 6 is that gerontocracy brings about peace: aging dictators experience fewer rebellions than younger ones, regardless of their respective tenure in office. Due to the lack of data, I could not investigate all possible implications of this finding, but it may constitute an indication that the seniority principle – as applied in many bureaucratic organizations such as the military – is a self-enforcing rule, because it creates natural limits to incumbency but also tends to produce leaders who have already accumulated effective power within the organization. Thus, one possible direction for future research would be to compare party or military regimes that implement this principle consistently with regimes that rely on a similar type of organization but lack such a rule, and test whether they differ systematically in their stability and their behavior.

The remaining limitations are rather practical than theoretical: institutionalization brings about peace, but not every institution is self-enforcing, and rules that are self-enforcing are typically difficult to establish (see Chapter 3). With regard to democracy, holding the first

competitive election is a risky gamble for incumbents, even if peaceful alternation in power becomes self-stabilizing on the long run (Przeworski 2015). The same is true of monarchies: although their institutional setup makes them more stable on the long run, establishing hereditary succession as a rule requires that the current leader is powerful enough to force his will upon his allies (Brownlee 2007). Finally, no policy recommendation can be drawn from the findings sketched above: as Hume (2000 [1738]) reminded us centuries ago, prescriptive statements cannot be logically derived from descriptive ones. The fact that monarchies are more peaceful than other autocratic regimes does not necessarily make them more desirable.

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