"Causes and Consequences of Democracy and Terror"

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Chapter 1

Introduction

Democracy and terror are two important concepts of political economy. Explaining the emergence of both has been at the center of the literature for an extended period of time. Especially the literature dealing with terror has taken a flight since the tragic attacks of September 11, 2001. This thesis sheds new light on various aspects of democracy and terror, reviewing what has been found and extending existing theories. A major contribution of the thesis is the rigorous and thorough empirical evaluation of various proposed determinants of both democracy and terror while also contributing new empirical findings to extend the research agenda.

Several key features will be reappearing throughout the whole work. One is the empirical method of extreme bounds analysis (Sala-i-Martin, 1997). In some chapters it will play a central role, e.g., chapter 2 while in some it will be used as a test of robustness. A rigorous discussion of the method is presented in the chapters that follow. Briefly, this method permutates covariates in order to examine the dependence of a particular variable on the conditioning information set.

A second reappearing theme is the measurement of key variables, in particular, the measurement of democracy. Numerous measures have been proposed as quantifications for democracy. The three most popular are the Polity IV measure developed by Marshall and Jaggers (2002), the annually published Freedom House indicators (civil liberties and political rights), and finally the dichotomous measure developed by Przeworski et

al. (2000). In each chapter we explain why a particular measure has been employed, or we show the independence of the results with respect to the measure chosen. The difference between the three indicators is in particular addressed in chapter 3.

The thesis is structured as follows. In chapter 2 we address the determinants of democracy. Efforts to test the empirical implications of stories of democracy have produced a long list of variables, and there is little consensus over which variables have a robust effect. We apply Sala-i-Martin's (1997) extreme bounds analysis (EBA) to test the robustness of more than 55 various proposed factors. We assess both the emergence and the survival of democracy. Our approach is extreme. In total, we evaluate over three million regressions. A striking finding is that most variables do not survive the EBA. We do not suggest that these factors are unimportant. Some variables, however, do survive and are perhaps the most important determinants. Regarding the emergence of democracy, we find that economic growth, fuel exports, and the share of the population that is Muslim have robust negative effects. GDP per capita does not have a robust relationship with the emergence of democracy. Regarding the survival of democracy, GDP per capita has a positive significant effect. We also find that being surrounded by democracies has a positive effect, while having an executive who comes from the military has a negative effect. Finally, a past history of transitions cuts both ways, making democracies and dictatorships less sturdy.

After exploring the determinants of democracies we continue by linking democracy to an economic outcome. In particular we focus on the influence of the political regime on international trade in chapter 3. Firstly, we develop a theoretical model that predicts that autocracies trade less than democracies. Secondly, we test the predictions of the model empirically using a panel of more than 130 countries for the years 1962 to 2000. In contrast to the existing literature, we use data on individual importing and exporting countries, rather than a dyadic set-up. In line with the model, we find that autocracies import substantially less than democracies, even after controlling for official trade policies. This finding is very stable and does not depend on a particular set-up or estimation technique.

The next chapter turns to a topic which is currently in the focus of both politicians and the public in general. We analyze what shapes environmental policies. Deindustrialization, stagnant real incomes of production workers and increasing inequality are latter-day features of many economies. It is common to assume that such developments pressure policy-makers to relax environmental standards. However, when heavily polluting industries become less important economically, their political importance also tends to diminish. Consequently, a regulator may increase the stringency of environmental policies. Like some other studies, we find that declining industrial employment translates into stricter environmental standards. In contrast to previous studies, but consistent with our argument, we find that greater income inequality is associated with policies that promote a cleaner environment.

Chapter 5 is the first chapter in which terror plays a central role. The chapter presents a political economy model linking terror and governments' respect for human rights. Using panel data for 111 countries over the period 1973-2002, we then empirically analyze whether and to what extent terror affects human rights – measured by three indices covering a wide variety of human rights aspects. According to our results, terror significantly diminishes governments' respect for basic human rights such as absence of extrajudicial killings, political imprisonment, and torture. To some extent, civil rights are also restricted as a consequence of terrorism, while we find no effect of terrorism on empowerment rights.

We continue to explore the interrelationship between democracy and terror in chapter 6. We examine whether cabinet duration is affected by the presence of terror. The literature on cabinet duration argues that critical events may trigger cabinet dissolution. In this chapter we empirically examine whether one specific critical event, i.e., terrorism, can explain why some cabinets endure longer than others. Using different duration models for a panel of more than 150 countries for the period between 1968 and 2002, we find some evidence that terrorism affects the duration of governments. In particular, we find that terrorists influence cabinet dissolution in countries that have unstable polities.

The following chapter examines a more specific question dealing with a very similar topic. The role of terror for election outcomes. Put more explicitly, we examine the

relationship between terrorism and electoral accountability. Using conditional fixed effects logit regressions and a dataset containing over 800 elections in 115 countries, we find that terror has a robust positive effect on the probability that the incumbent government is replaced. The magnitude of the effect increases with the severity of the terrorist attack.

Chapter 8 deals with the determinants of terror. We analyze the impact of political proximity to the United States on the occurrence and severity of terror. Employing panel data for 116 countries over the period 1975-2001 we find that countries voting in line with the U.S. are victims of more and deadlier attacks.

The final chapter of this work briefly summarizes our findings.

Chapter 2

Extreme Bounds of Democracy

2.1 Introduction

There are many stories of democracy. Efforts to test the empirical implications of various theories have produced a long list of variables that purportedly explain democracy. There is, however, little consensus over which variables robustly determine democracy. We address this issue by applying Sala-i-Martin's (1997) extreme bounds analysis (EBA) to test the robustness of more than 55 various proposed factors. We assess both the factors leading to the emergence of democracy as well as the factors explaining the persistence of democracy. To be clear from the outset, our approach is extreme. We evaluate over 1.7 million regressions of the emergence of democracy, and over 1.4 million regressions for the survival of democracy.

The most striking of our findings is that most of the variables suggested in the literature do not survive the rigorous EBA robustness test. While many of the factors we test have been shown elsewhere to produce significant effects in plausible and well-specified models, when put to the rigors of being tested along with many other plausible variables, the significance of their effects simply do not survive.

This chapter is an adapted version of Gassebner et al. (2007e).

We do not suggest that this implies these factors are unimportant. Many of the findings we build off of are valid within the confines of the original statistical model proposed in the literature. In particular one has to bear in mind that our empirical approach focuses on reduced form models. Moreover, to the extent that some variables fail our test, this could be because they are poor proxies for otherwise strong theories of democracy. The standard of surviving the test of EBA is just a very a high one, and only the strongest of relationships survive it.

Some variables, however, do indeed survive. We suggest that these variables may be the most important factors determining democracy. Regarding the emergence of democracy, we find that economic growth has a robust negative effect. This finding, standing in stark contrast to Modernization Theory, suggests that autocracies with strong economic performance are unlikely to see democracy emerge. Instead, economic contraction causes dictatorships to break down. Also in contrast to Modernization Theory, but consistent with the argument of Przeworski et al. (2000), the level of GDP per capita does not have a robust relationship with the emergence of democracy. The only other variables that have a robust significant effect are fuel exports and the share of the population that is Muslim. We suspect that the latter two findings are largely driven by the Middle East and seek to pursue this possibility further.

Regarding the survival of democracy, GDP per capita has a positive significant effect. The finding confirms Przeworski et al. (2000) and Przeworski (2005). We also find that being surrounded by democracies increases the likelihood of staying a democracy, consistent with Gleditsch (2002). In line with Cheibub (2006), we find that democracies with executives who have a military background are more likely to experience a democratic breakdown. Finally, we find that past transitions also reduce the survival probability of democracies. It turns out that a past history of transitions cuts both ways, making both democracies and dictatorships less sturdy, as Przeworski et al. (2000) argue.

The chapter proceeds as follows. We begin with a brief review of the literature. We then introduce the EBA method in detail and then present the results. We conclude by summarizing our findings and deriving policy conclusions.

2.2 Background

In an early large-n study of democracy, Almond and Verba (1963) propose a cultural explanation of democracy. Using survey-based research in five countries, they argue that a "participant" culture (as opposed to a "subject" or "parochial" culture) is required for democracy. The "civic culture" argument is tested cross-nationally in the work of Inglehart (1988), who finds that democracy is correlated with the percentage of people reporting high levels of interpersonal trust, low levels of support for revolutionary change, and high levels of life satisfaction. His findings are disputed by Seligson (2002), who shows that the correlation disappears when one controls for level of economic development. Przeworski et al. (2000) test a full range of other cultural variables, finding that none has a robust relationship with democracy once one accounts for level of economic development.

Economic explanations of democracy date back to Lipset (1959) who is often cited as the first "modernization theorist." Modernization Theory argues that as countries develop economically, social structures become too complex for authoritarian regimes to manage – technological change endows owners of capital with some autonomy and private information, complex labor processes require active cooperation rather than coercion, and civil society emerges. At some point in this process, dictatorship collapses and democracy emerges as the alternative.

Huntington (1968) adds that sustainable democracy requires political development along with economic development, but basically agrees that as a dictatorship experiences economic development democratization becomes more likely. Without political development, however, rapid economic development can also destabilize democracies. Thus he proposes a "bell-shaped" pattern of stability of regimes with respect to economic development.

In their expansive large-n study of democracy and development, Przeworski et al. (2000) thoroughly explore the relationship. They begin with the observation that the correlation between level of economic development and democracy is strong. They question, however, the process by which this correlation is driven. They suggest, in contrast

to modernization theorists, that this correlation is possible even if the emergence of democracy is completely random with respect to economic development. The correlation may be driven instead by a relationship between economic development and the survival of democracy.

This is in fact what their book argues. The emergence of democracy has no relationship with level of economic development; the correlation instead is entirely driven by the survival of democracy. In other work, Przeworski (2005, p.253) argues that "democracy prevails in developed societies because too much is at stake in turning against it." Conversely, in poor democracies, "the value of becoming a dictator is greater and the accumulated cost of destroying capital stock is lower" (Przeworski and Limongi, 1997, p.166 fn.1).

It should be noted, however, that while Przeworski et al. (2000) show that transitions to democracy are not well predicted by economic development and survival of democracy is, the estimated effect of economic development on the transition to democracy is statistically significant in their specification.[1] We suspect (and show below) that it is not a robust relationship.

Since the Przeworski et al. (2000) study, many large-n studies of democracy have been pursued – too many to adequately review here. We present a summary of 14 published empirical studies on the determinants of democracy in Table 2.8 in the Appendix.

The Przeworski et al. (2000) study ignores the oil rich countries of the Middle East. As these scholars were originally interested in estimating the effect of regime on economic growth, they chose not to include oil rich countries, whose process of augmenting GDP per capita is much different from that of other countries. Nevertheless, these countries present a real challenge to the Modernization Theory argument that should be considered.

The argument of Boix (2003) provides a compelling answer.[2] He argues that level of economic development, income distribution, and – importantly – asset specificity

[1]The insignificant coefficient reported indicates that the difference between the coefficients for the emergence and survival of democracy is not significant.

[2]See also Boix and Stokes (2003) on this matter.

together impact the probability of the emergence of democracy. Where asset specificity is high and the income distribution is highly skewed, such as in many oil-rich countries, the rich face severe redistributional consequences for allowing popular sovereignty, and they have no credible threat to flee the country taking their productive capacity with them. Thus, it is in their interest to pay high costs of repressing democracy, maintaining dictatorial rule. If assets are not highly specific, however, the rich have a credible exit threat. If the rich flee the country, taking the productive capacity along with them, they can severely harm the national economy. The credible threat restrains the redistributional demands of the poor and may make democracy possible even in countries with relatively low levels of economic development, such as India. Asset specificity aside, if redistributional demands diminish at higher levels of economic development, Boix argues that economic development should make democracy more likely both to emerge and to survive.

Acemoglu and Robinson (2006) also propose a theory of democracy where elites may prefer dictatorship but must pay the costs of repression as the masses threaten disorder. Where repression costs are high and elites cannot credibly promise concessions otherwise, democracy can offer a compelling alternative under specific conditions. Factors identified by their theory as determinants of democracy include measures of civil society, political institutions, economic crises, income distribution, the structure of the economy, and the forces of globalization.

Another story that addresses a regional pattern of regime such as found in the Middle East is suggested by Gleditsch (2002). His thesis, summarized nicely by the title of his book, is that all (international) politics are local. Diffusion theorists suggest that through various forces that spill over borders – political, cultural, and economic – regime in one country is likely to be correlated with regime in a neighboring country. Thus we have solidly democratic regions, such as Europe, dictatorial regions, such as the Middle East, and regions where countries tend to transition in waves, such as Latin America.

A related story is suggested by Pevehouse (2002a,b), who argues that participation in international organizations that are dominated by one regime or another influences

both the emergence and survival of democracy. He develops an innovative mechanism by which diffusion may operate – participation in regional organizations provides incentives for countries to encourage democratic standards amongst the membership.

After briefly highlighting and describing the main variables we now turn to the empirical section of the chapter. The motivation for the test presented in the following section is as follows:

There is a vigorous debate on the determinants of democracy. Most papers, however, present but a handful of potential specifications, controlling for very few of the possible combinations of different variables. Of course, each paper presents valid theoretical justifications for how the specifications are chosen. But looking across the vast literature, there appears to be little consensus on the theory. Findings that are presented as statistically significant in the presence of some variables may or may not be significant in the presence (or absence) of other variables that other scholars have proposed. We, therefore, suggest testing the bounds of the significance of previously proposed variables.

As a measure of democracy, we begin with the Przeworski et al. (2000) dichotomous variable (which follows Schumpeter's (1942) conception of regime): democracy is the political system in which key government offices are filled through contested elections. The definition has two parts: "key government office," which they define as the executive and the legislature; and "contested," which implies that more than one party has some probability of winning office through election.[3] Elections must be associated with some *ex ante* uncertainty, and be subject to *ex post* irreversibility. Put succinctly, "democracy is a system in which incumbents lose elections and leave office when the rules so dictate" (Przeworski et al., 2000, p.54).

[3]Sometimes this is obvious, such as when incumbents lose elections and relinquish power (Przeworski 1991). Sometimes it is not, such as when incumbents successively win contested elections. Also see Vreeland (2003).

2.3 Empirical Method

Since there are many studies that investigate the determinants of democracy, there is a long list of potential explanatory variables.[4] Studies often restrict their analysis to certain subsets of these variables and often ignore the effects of any omitted variable bias when other variables are not included. In addition to any model uncertainty, the limited number of observations often restricts the power of statistical tests that rule out irrelevant explanatory variables.

To address these issues we use extreme bounds analysis (EBA), as proposed by Leamer (1983) and Levine and Renelt (1992). EBA enables us to examine which explanatory variables are robustly related to our democracy measure and is a relatively neutral way of coping with the problem of selecting variables for an empirical model in situations where there are conflicting or inconclusive suggestions in the literature.

We begin with a basic model of democracy which assumes that the probability of observing democracy at time t (measured in years in our data) follows a first order Markov process. Let D be a dummy variable coded 1 if a country is a democracy, and 0 otherwise. Then,

$$\Pr\left(D_t|D_{t-1}\right) = \left(1 - D_{t-1}\right) \cdot \Pr\left(D_t|D_{t-1} = 0\right) + \left(D_{t-1}\right) \cdot \Pr\left(D_t|D_{t-1} = 1\right). \tag{2.1}$$

As the likelihood function for this model is additively separable, it can be easily estimated as two logistic functions, where the transition probabilities are defined as follows:

$$\Pr\left(D_t|D_{t-1} = 0\right) = \Lambda\left(\beta^{AD'} x_{t-1}\right) \tag{2.2}$$

$$\Pr\left(D_t|D_{t-1} = 1\right) = \Lambda\left(\beta^{DD'} x_{t-1}\right), \tag{2.3}$$

where Λ is the cumulative distribution function of the logistic distribution, x_{t-1} is the vector of (lagged) variables that determine democracy, β^{AD} is a vector of coef-

[4] See Table 2.8 in the Appendix for a summary of 14 recent empirical studies.

ficients capturing the effects of these variables on the probability of transition from ***A***uthoritarianism to ***D***emocracy, and β^{DD} is a vector of coefficients capturing the effects of these variables on the survival of democracy ("transitioning" from ***D***emocracy to ***D***emocracy). While it is of course not necessary to assume that the same variables determine both the emergence and survival of democracy, most scholars in practice do, and we will be testing all variables in both setups.

To conduct an EBA, we define:

$$\beta^{AD'} x_{t-1} = \beta_M^{AD'} M_{t-1} + \beta_F^{AD'} F_{t-1} + \beta_Z^{AD'} Z_{t-1} \quad (2.4)$$

$$\beta^{DD'} x_{t-1} = \beta_M^{DD'} M_{t-1} + \beta_F^{DD'} F_{t-1} + \beta_Z^{DD'} Z_{t-1}, \quad (2.5)$$

where M is a vector of "commonly accepted" explanatory variables for the emergence of democracy; and F is a vector containing the variables of interest; and Z is a vector containing up to three possible additional explanatory variables (as in Levine and Renelt, 1992) which, according to the broader literature, are related to the dependent variable.

The EBA test for a variable in F states that if the lower extreme bound for β_F – i.e., the lowest value for β_F minus two standard deviations – is negative, while the upper extreme bound for β_F – i.e., the highest value for β_F plus two standard deviations – is positive, the variable F is not robustly related to our democracy measure.

As it is common in the literature we use pooled logit regressions. We include all time-invariant variables previously proposed in the literature to proxy for fixed country effects. Including fixed country effects directly, reduces the sample size to only 200 observations which makes inference unreliable.

Sala-i-Martin (1997) argues that this testing criterion is far too strong for hardly any variable to ever pass it. If the distribution of the parameter of interest has both positive and negative support, then a researcher is bound to find at least one regression model for which the estimated coefficient changes sign if enough regressions are run. Consequently, we report the percentage of the regressions in which the coefficient of the variable F is statistically different from zero at the five percent significance level. Moreover, instead of only analyzing the extreme bounds of the estimates of the coefficient of a particular

variable, we follow Sala-i-Martin's (1997) recommended procedure and analyze the entire distribution. Accordingly, we also report the unweighted parameter estimate of β_F and its standard error, as well as the unweighted cumulative distribution function, CDF(0). The latter represents the proportion of the cumulative distribution function lying on each side of zero. CDF(0) indicates the larger of the areas under the density function either above or below zero, i.e., whether this happens to be CDF(0) or 1 - CDF(0). So CDF(0) always lies between 0.5 and 1.0. However, in contrast to Sala-i-Martin, we use the unweighted, instead of the weighted, CDF(0).[5]

Another objection to EBA is that the initial partition of variables in the M and in the Z vector is likely to be arbitrary. However, as pointed out by Temple (2000), there is no reason why standard model selection procedures (such as testing down from a general specification) cannot be used in advance to identify variables that are particularly relevant. Furthermore, some variables are included in the large majority of studies and are by now common in this branch of the literature.

In our view, the inclusion of GDP per capita (measured in purchasing power parities) in the M vector is the only non-contentious inclusion as a regressor. We are conscious of not prejudging the importance of other explanatory variables for the outcome of the EBA. Thus, we sort all other variables in the F vector and test their relevance individually. The list of all variables, their definitions and sources is given in Table 2.1. All variables (except the time-invariant) are included in the model with a lag of one year. On the one hand, this mitigates potential endogeneity problems and on the other hand this also allows us to interpret the relationships as being (Granger-)causal.

The basic idea of the EBA is to run many regressions continuously permutating explanatory variables and to test how the variable in the center of attention "behaves" (e.g., how often it is significant) with respect to the conditioning set. In the basic model regression estimation equations include GDP per capita as well as combinations of up to

[5]Sala-i-Martin (1997) proposes using the integrated likelihood to construct a weighted CDF(0). However, missing observations for some of the variables poses a problem. Sturm and de Haan (2002) show that the goodness-of-fit measure may not be a good indicator of the probability that a model is the true model and that the weights constructed in this way are not invariant to linear transformations of the dependent variable. Hence, changing scales could result in different outcomes and conclusions. We therefore employ the unweighted version.

three further variables. In the F vector regressions, GDP per capita is included as well the variable in focus plus up to three additional variables out of the F vector. Overall, we estimate a total of 1,776,379 specifications for the emergence of democracy and 1,492,029 specifications for the survival of democracy. As we use logit regressions we exclude estimations with convergence problems as well as estimations where the optimizing algorithm breaks down. Finally, we control our output variables by calculating medians. However, these values are very similar to the calculated means.

Table 2.1: Variables – definitions, sources and previous studies

Variable	**Definition**	**Source**	**Proposed by**
Africa	Dummy variable for African Countries	Easterly and Sewadeh (2001)	Li and Reuveny (2003); López-Córdova and Meissner (2005)
Arable land	Arable land (hectares)	World Bank (2006)	Crenshaw (1995)
Bonds investment	Portfolio investment, bonds (PPG + PNG) (NFL, current US$)	World Bank (2006)	Li and Reuveny (2003)
Colony	Dummy variable if ever in a colonial relationship	CEPII (2006)	Barro (1999); Boix and Stokes (2003)
Employment in Agriculture	Employment in Agriculture (% of total employment)	World Bank (2006)	Clague et (2001); Crenshaw (1995)
Equity investment	Portfolio investment, equity (DRS, current US$)	World Bank (2006)	Li and Reuveny (2003)
English speaking	1 if English first language of at least 9% of the population	CEPII (2006)	Clague et al. (2001)
European settlers 1900	Share of European settlers in the country in 1900	Acemoglu and Robinson (2006)	Acemoglu and Robinson (2006)
FDI net inflows	Foreign direct investment, net inflows (% of GDP)	World Bank (2006)	Li and Reuveny (2003)
French colony	1 if ever in colonial relationship with France	CEPII (2006)	Barro (1999)
French speaking	1 if French first language of at least 9% of the population	CEPII (2006)	Clague et al. (2001)

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Variable	Definition	Source	Proposed by
Fuel exports	Fuel exports (% of merchandise exports)	World Bank (2006)	Ross (2001)
GDP growth	GDP growth (annual %)	World Bank (2006)	Boix and Stokes (2003); Fidrmuc (2003); Li and Reuveny (2003); Muller (1995)
GDP p.c. growth	GDP per capita growth (annual %)	World Bank (2006)	Pevehouse (2002a,b)
GDP p.c., PPP	GDP per capita, PPP (current international $)	World Bank (2006)	Acemoglu et al.(2005); Boix and Stokes (2003); Crenshaw (1995); Gleditsch and Ward (2006); Muller (1995); Nieswiadomy and Strazcich (2004)
Globalization	KOF Index of Globalization	Dreher (2006a)	This chapter
Income taxes	Taxes on income, profits and capital gains (% of total taxes)	World Bank (2006)	Ross (2001)
Industry employment, female	Employees, industry, female (% of female employment)	World Bank (2006)	Ross (2001)
Industry employment, male	Employees, industry, male (% of male employment)	World Bank (2006)	Ross (2001)
Inequality	Industrial pay-inequality based on UNIDO's database of payments	UTIP (2001)	Barro (1999); Crenshaw (1995)
Infant mortality	Mortality rate, infant (per 1,000 live births)	World Bank (2006)	Barro (1999); Nieswiadomy and Strazcich (2004)
Inflation	Inflation, GDP deflator (annual %)	World Bank (2006)	Li and Reuveny (2003)
IO score	Highest democracy score of participated International Organization, calculated as average across all members' Polity IV score excluding the own value	Pevehouse (2002a,b)	Pevehouse (2002a,b)

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Variable	Definition	Source	Proposed by
IO score, change	First difference of IO score (see above)	Pevehouse (2002a,b)	Pevehouse (2002a,b)
Land area	Land area (hectares)	World Bank (2006)	López-Córdova and Meissner (2005)
Latin America	Dummy variable for Latin American countries	Easterly and Sewadeh (2001)	López-Córdova and Meissner (2005)
Life expectancy	Life expectancy at birth, total (years)	World Bank (2006)	Barro (1999); Clague et al. (2001); Nieswiadomy and Strazcich (2004); Ross (2001)
Literacy	Literacy rate, adult total (ages 15 and above)	World Bank (2006)	Clague et al. (2001)
Metal exports	Ores and metals exports (% of merchandise exports)	World Bank (2006)	Crenshaw (1995); Ross (2001)
Middle East	Dummy for Countries from the Middle East	Easterly and Sewadeh (2001)	Li and Reuveny (2003)
Military expenditure	Military expenditure (% of GDP)	World Bank (2006)	Ross (2001)
Military leader	Executive leader is a former military officer	Gandhi and Przeworski (2006)	Cheibub (2006)
Military personnel	Military personnel, total	World Bank (2006)	Crenshaw (1995); Ross (2001)
Muslim share	Share of Muslim population	Przeworski et al. (2000)	Barro (1999); Boix and Stokes (2003); Clague et al. (2001); Muller (1995) ; Ross (2001)
Neighboring democracies	Share of surrounding democracies	Own calculations	Gleditsch and Ward (2006); Pevehouse (2002a)
Number of past transitions	Number of previous transitions between autocracy/democracy	Przeworski et al. (2000)	Boix and Stokes (2003)
OECD member	Dummy variable for OECD membership	OECD (2007)	Ross (2001)
Oil exporter	Dummy variable for exporters of fuel/oil	Easterly and Sewadeh (2001)	Barro (1999); López-Córdova and Meissner (2005)

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Variable	**Definition**	**Source**	**Proposed by**
OPEC member	Dummy variable for OPEC membership	OPEC (2007)	Nieswiadomy and Strazcich (2004)
Openness	Trade (% of GDP)	World Bank (2006)	Li and Reuveny (2003); López-Córdova and Meissner (2005)
Population (log)	log of total population	World Bank (2006)	Acemoglu et al. (2005); Barro (1999); López-Córdova and Meissner (2005); Nieswiadomy and Strazcich (2004)
Population share 0-14	Population ages 0-14 (% of total)	World Bank (2006)	Acemoglu et al. (2005)
Population share 15-64	Population ages 15-64 (% of total)	World Bank (2006)	Acemoglu et al. (2005)
Population share 65+	Population ages 65 and above (% of total)	World Bank (2006)	Acemoglu et al. (2005)
Portfolio investment	Portfolio investment, excluding LCFAR (BoP, current US$)	World Bank (2006)	Li and Reuveny (2003)
Portuguese colony	1 if ever in colonial relationship with Portugal	CEPII (2006)	Barro (1999)
Portuguese speaking	1 if Portuguese first language of at least 9% of the population	CEPII (2006)	Clague et al. (2001)
Service employment, female	Employees, services, female (% of female employment)	World Bank (2006)	Ross (2001)
Service employment, male	Employees, services, male (% of male employment)	World Bank (2006)	Ross (2001)
Settler mortality (log)	log of historical mortality rates of potential European settlers	Acemoglu and Robinson (2006)	Acemoglu and Robinson (2006)
Socialist	1 if present or former socialist country	Easterly and Sewadeh (2001)	Muller (1995)
Spanish colony	1 if ever in colonial relationship with Spain	CEPII (2006)	Barro (1999)
Spanish speaking	1 if Spanish first language of at least 9% of the population	CEPII (2006)	Clague et al. (2001)
Tax revenue	Tax revenue (% of GDP)	World Bank (2006)	Ross (2001)

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Variable	Definition	Source	Proposed by
Telephone mainlines	Telephone mainlines (per 1,000 people)	World Bank (2006)	Ross (2001)
TV sets	Television sets (per 1,000 people)	World Bank (2006)	Ross (2001)
U.K. colony	1 if ever in colonial relationship with United Kingdom	CEPII (2006)	Barro (1999); Boix and Stokes (2003); Clague et al. (2001); Crenshaw (1995)
Urban population	Urban population (% of total)	World Bank (2006)	Barro (1999); Nieswiadomy and Strazcich (2004); Ross (2001)
World democracy	Global share of democracies	Own calculations	Boix and Stokes (2003); Gleditsch and Ward (2006)

2.4 Results

The results of our empirical analysis are summarized in Tables 2.2 to 2.5. They read as follows: *Avg. Beta* and *Avg. S.E.* give the unweighted averages over all regressions of the coefficient and the standard error, respectively. *%Sign.* gives the percentage of regressions in which the respective coefficient is statistically significant at the 5 percent level. *CDF(0)* is the unweighted cumulative distribution function which reports the larger of the areas under the density function either above or below zero (as described above). All variables are sorted according to this criterion and the cutoff point for a variable to be considered robustly linked to our dependent variable is a CDF(0) value of 0.9 or higher following Sala-i-Martin (1997). *Regres.* represents the number of regressions run for each variable tested, and *Avg. Obs.* reports the average number of observations for these regressions.

Table 2.2 contains the results for the transitions from autocracies to democracies – the *emergence* of democracy. The first result to note is that GDP per capita does not explain democratic transitions. This confirms the Przeworski et al. (2000) critique of the Modernization Theory literature. Furthermore, the very low value of the CDF(0) indicates that this result is not even a "borderline" variable.

Table 2.2: Results EBA – transition from autocracy to democracy (robust variables)

Variable	Avg. Beta	Avg. S.E.	%Sign.	CDF(0)	Regres.	Avg. Obs.
Base Model						
GDP p.c., PPP (log)	-0.166	0.360	12.8	0.6041	32,558	894
Extended Model						
Number of past transitions	0.535	0.207	87.5	0.9750	30,708	887
OECD member	2.359	0.986	82.1	0.9673	28,263	939
Muslim share	-2.112	1.235	68.9	0.9494	30,860	820
Fuel exports	-0.058	0.041	67.5	0.9472	30,758	543
GDP growth	-0.064	0.041	65.0	0.9267	30,900	878

Notes: 'Avg. Beta' and 'Avg. S.E.' give the unweighted averages over all regressions of the coefficient and the standard error, respectively. '%Sign.' gives the percentage of regressions in which the respective coefficient is statistically significant at the five percent level. 'CDF(0)' yields the result of the CDF criterion as described in the previous section. All variables are sorted according to this criterion. The cut-off value for a variable to be considered robustly linked to our dependent variable is 0.9. Finally, 'Regres.' and 'Avg. Obs.' report the number of regressions run for testing each variable and the average number of observations for each regression. The results are derived using logistic regressions conditional on being autocratic the year before.

Turning to the other variables we see that the variable with the highest score on the CDF criterion is the number of previous transitions. Having had experience with regime changes increases the probability of becoming a democracy. One could describe this as having obtained the knowledge of how to change a system. Being an OECD member also increases the likelihood of a change towards a democracy. We included this variable following Ross (2001) and confirm his finding.

We find that democratic transitions are less likely in Muslim countries, as measured by the percentage of the population that is Muslim. We are not convinced from this finding that Islam is incompatible with democracy – the result calls for further investigation. Our variable is time-invariant, so this could be picking up on other country-specific characteristics. It would be good to get a more nuanced measure.

One possibility is that several Muslim countries are fuel exporters. We find the larger the share of fuel exports the less likely a country is to become a democracy. This is in part driven by the Arabic oil-producing countries which all have a long non-democratic

tradition. The finding is consistent with the resource-curse literature (e.g., Ross, 2001; Jensen and Wantchekon, 2004).

The final variable fulfilling the CDF criterion is annual GDP growth. Countries that are doing better are less likely to engage in a political transformation as in good time the "need" for a change might not be felt in the population and autocratic rulers can justify their position. This may seem intuitive, but it defies a basic idea in Modernization Theory that as a country develops, democracy should become more likely. All other variables tested do not pass the CDF criterion (see Table 2.3).

Table 2.3: Results EBA – transition from autocracy to democracy (non-robust variables)

Variable	**Avg. Beta**	**Avg. S.E.**	**%Sign.**	**CDF(0)**	**Regres.**	**Avg. Obs.**
Openness	-0.014	0.012	57.1	0.8969	30,889	852
Military expenditure	-0.371	0.232	47.5	0.8859	30,847	377
European settlers 1900	0.021	0.016	64.1	0.8854	30,872	837
Military leader	0.913	0.589	61.4	0.8833	30,601	754
French colony	-0.877	0.677	33.2	0.8778	28,782	934
GDP p.c. growth	-0.041	0.043	55.5	0.8706	30,900	877
Spanish colony	0.976	0.720	52.7	0.8602	29,761	909
FDI net inflows	-0.109	0.098	37.0	0.8574	30,893	850
Spanish speaking	1.014	0.729	51.8	0.8570	29,801	909
Latin America	0.912	0.725	54.6	0.8505	29,758	849
Population (log)	0.194	0.172	37.6	0.8399	30,895	882
Employment in agriculture	0.026	0.035	25.6	0.8394	30,790	233
Service employment, male	-0.036	0.070	9.1	0.8232	30,640	181
Neighboring democracies	0.922	0.849	48.6	0.8204	30,890	823
Portuguese speaking	1.487	0.950	18.0	0.8188	22,758	1,088
Colony	-0.922	0.711	38.5	0.8055	29,956	845
Population share 65+	0.131	0.232	48.3	0.8034	30,888	877
Oil exporter	-1.119	0.919	27.9	0.8014	26,309	920
Literacy	0.015	0.016	30.1	0.7968	30,882	756
World democracy	2.931	3.345	44.3	0.7900	30,864	883
IO score	0.045	0.064	42.0	0.7858	30,773	768
French speaking	-0.622	0.676	13.4	0.7822	29,142	925
Military personnel	0.000	0.000	4.2	0.7765	30,909	435
Settler mortality (log)	0.365	0.403	19.4	0.7759	30,712	628
Population share 0-14	-0.032	0.115	31.8	0.7661	30,889	877
Tax revenue	0.042	0.085	16.7	0.7468	30,350	212

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Variable	Avg. Beta	Avg. S.E.	%Sign.	CDF(0)	Regres.	Avg. Obs.
IO score, change	0.019	0.222	27.3	0.7411	30,589	770
Bonds investment	0.000	0.000	8.9	0.7225	30,903	786
Socialist	-0.366	0.757	13.0	0.7112	26,998	870
Life expectancy	0.071	0.085	12.0	0.7021	30,489	262
Service employment, female	-0.013	0.036	3.9	0.7012	30,728	181
Population share 15-64	0.034	0.130	18.4	0.6968	30,890	877
Inequality	0.048	0.082	9.0	0.6782	30,632	475
Telephone mainlines	-0.005	0.010	3.4	0.6634	30,900	857
Africa	-0.269	0.684	13.0	0.6615	30,727	827
Land area	0.000	0.000	3.7	0.6579	30,913	878
Metal exports	0.009	0.032	3.5	0.6534	30,615	572
Portuguese colony	-0.060	0.938	3.7	0.6279	22,121	1,095
U.K. colony	-0.153	0.516	7.0	0.6253	30,801	884
Infant mortality	-0.009	0.274	0.4	0.6141	30,301	170
OPEC member	0.042	0.831	10.1	0.6131	26,452	986
Portfolio investment	0.000	0.000	0.0	0.6104	30,910	725
Arable land	0.000	0.000	17.3	0.5989	30,913	878
TV sets	-0.001	0.013	3.7	0.5975	30,898	845
Income taxes	0.010	0.055	4.0	0.5882	30,395	210
Industry employment, female	-0.018	0.058	0.1	0.5819	30,769	180
Middle East	-0.164	0.699	1.8	0.5805	28,785	870
Globalization	-0.004	0.034	7.0	0.5772	30,845	722
Urban population	0.004	0.019	3.8	0.5741	30,886	871
Industry employment, male	-0.025	0.066	3.6	0.5377	30,619	181
English speaking	0.016	0.517	2.9	0.5254	30,743	886
Inflation	0.004	0.016	14.0	0.5220	30,874	865
Equity investment	0.000	0.000	4.3	0.5026	30,885	816

Notes: See notes to Table 2.2 for the explanation of the abbreviations used. The results are derived using logistic regressions conditional on being autocratic the year before.

Table 2.4 presents the results for the probability of staying a democracy – democracy's *survival*. GDP per capita plays a central role. Richer countries are more likely to remain democracies. This confirms the findings in the previous literature.

Of the remaining variables we find having a (former) military leader to score highest on the CDF criterion. The result implies that countries which have a leader with a military background have a lower probability of sustaining a democracy.

Table 2.4: Results EBA – remaining a democracy (robust variables)

Variable	Avg. Beta	Avg. S.E.	%Sign.	CDF(0)	Regres.	Avg. Obs.
Base Model						
GDP p.c., PPP (log)	1.504	0.953	68.6	0.9285	31,316	1,014
Extended Model						
Military Leader	-2.590	1.657	77.1	0.9558	22,181	836
Neighboring democracies	2.903	1.999	49.4	0.9294	27,681	821
Number of past transitions	-0.630	0.497	59.2	0.9140	29,012	1,020

Notes: See notes to Table 2.2 for the explanation of the abbreviations used. The results are derived using logistic regressions conditional on being democratic the year before.

Our finding with respect to the neighboring democracy variable indicates that there are positive spillover effects from democracies as the probability of remaining a democracy increases in the number of democratic neighbors.

Very interesting is the result for the previous transitions variable, which is the only variable that passes the CDF criterion in both models. Previous transitions increase the chances of democratic failure. Taking the result of Table 2.2 also into account, we can summarize that previous transitions increase the instability of the political system. Again, all remaining variables fail to pass the CDF criterion (see Table 2.5).

Table 2.5: Results EBA – remaining a democracy (non-robust variables)

Variable	Avg. Beta	Avg. S.E.	%Sign.	CDF(0)	Regres.	Avg. Obs.
Colony	1.824	1.238	52.5	0.8884	27,970	965
GDP p.c. growth	0.094	0.133	51.3	0.8159	29,278	1,009
Openness	0.022	0.178	38.5	0.8114	29,361	1,004
English speaking	0.864	1.000	25.1	0.8090	27,983	1,039
Telephone mainlines	0.031	0.513	0.8	0.8065	29,178	1,007
Muslim share	-0.718	2.730	33.7	0.7912	28,901	935
Industry employment, male	0.267	0.298	0.3	0.7904	23,129	602
U.K. colony	0.738	1.051	14.6	0.7846	27,980	1,039
Spanish colony	-0.876	1.079	15.0	0.7759	23,693	1,140
Bonds investment	0.000	0.000	0.1	0.7702	30,360	579
Industry employment, female	0.191	0.300	0.1	0.7701	26,406	568
GDP growth	0.022	0.143	47.5	0.7693	29,244	1,010
Population share 65+	0.353	0.697	0.3	0.7558	29,420	982
Infant mortality	-0.007	0.305	0.4	0.7447	21,627	413
Service employment, female	0.081	0.274	0.2	0.7419	24,304	594
Spanish speaking	-0.731	1.141	7.5	0.7406	23,692	1,140
IO score, change	0.390	0.608	0.6	0.7384	26,107	752
French colony	0.619	0.986	7.0	0.7371	20,309	1,150
Tax revenue	0.256	0.442	0.2	0.7342	21,504	311
World democracy	0.741	7.494	38.3	0.7294	29,324	1,012
Inequality	-0.163	0.209	3.3	0.7220	26,598	685
Military expenditure	-0.172	0.421	1.2	0.7157	28,761	559
IO score	-0.007	0.282	2.5	0.7056	27,054	744
Middle East	-0.976	1.314	5.8	0.6972	26,138	1,008
Inflation	0.085	0.173	20.3	0.6967	29,599	996
Arable land	0.000	0.000	5.0	0.6902	30,489	960
Population (log)	-0.354	0.477	4.7	0.6867	29,182	1,015
OECD member	-0.664	1.249	2.9	0.6786	15,225	1,352
Income taxes	0.017	0.386	1.3	0.6783	21,947	300
FDI net inflows	0.116	0.231	0.2	0.6654	29,419	978
Urban population	0.042	0.087	5.2	0.6622	29,174	1,013
Fuel exports	0.461	0.533	0.1	0.6608	28,233	898
Military personnel	0.000	0.007	3.3	0.6437	30,401	633
Employment in agriculture	-0.049	0.369	2.8	0.6431	23,979	683
Globalization	0.003	0.079	0.7	0.6414	29,197	954
Metal exports	0.714	0.795	11.2	0.6401	28,240	936
TV sets	0.009	0.782	0.5	0.6393	29,737	989

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Variable	Avg. Beta	Avg. S.E.	%Sign.	CDF(0)	Regres.	Avg. Obs.
French speaking	-0.486	0.903	1.2	0.6316	20,550	1,144
Life expectancy	-0.153	0.579	0.0	0.6145	20,474	494
Population share 0-14	-0.158	0.262	0.5	0.5929	29,484	981
Literacy	0.012	0.144	2.6	0.5888	29,100	616
Land area	0.000	0.000	0.8	0.5879	30,407	962
Equity investment	0.000	0.000	0.0	0.5842	30,210	586
OPEC member	0.737	1.286	10.1	0.5755	15,115	1,290
Settler mortality (log)	-0.105	0.924	1.6	0.5752	28,785	589
Oil exporter	-1.466	1.346	10.7	0.5672	15,124	1,202
Portfolio investment	0.000	0.000	0.0	0.5585	30,497	902
Socialist	0.062	1.192	0.1	0.5580	16,248	1,051
European settlers 1900	0.006	0.026	0.4	0.5566	23,701	1,090
Service employment, male	0.013	0.153	0.9	0.5509	23,479	603
Population share 15-64	-0.169	0.350	0.7	0.5331	29,431	982
Africa	-0.233	1.020	2.8	0.5083	22,400	1,038
Latin America	0.112	1.131	1.1	0.5027	23,691	1,059

Notes: See notes to Table 2.2 for the explanation of the abbreviations used. The results are derived using logistic regressions conditional on being autocratic the year before.

To test whether the results of the EBA itself are robust and in order to get a point estimate of the magnitude for each variable we estimate "final" models including all variables which fulfilled the CDF criterion. The results are presented in Tables 2.6 and 2.7. Our model predicts that the probability of a democratic transition taking place is 2 percent if all our explanatory variables are assigned their mean value.[6] Given this low probability, it is quite remarkable that each additional prior transition increases this chance by roughly 1%. While each percentage point of GDP growth reduces this probability by roughly 0.1%.

The probability of remaining a democracy is (at the mean of our variables) 99.8 percent.[7] This also explains why basically all marginal effects are insignificant, except for GDP. Note, however, that in the estimation the number of past transitions is significant at the 5% level.

[6]The unconditional transition probability in our sample is 2.1 percent.

[7]The unconditional survival probability in our sample is 98.2 percent.

Table 2.6: Final models – transition from autocracy to democracy

Variable	**Coefficient**	**Marginal Effect**	**Variable Mean**
Number of past transitions	0.5542	0.0111	0.3921
	(5.05)***	(4.10)***	
OECD member	1.1230	0.0386	0.0307
	(2.19)**	(1.40)	
Muslim share	-1.1606	-0.0232	0.3247
	(1.91)*	(2.06)**	
Fuel exports	-0.0066	-0.0001	20.3009
	(0.86)	(0.87)	
GDP growth	-0.0711	-0.0014	4.4324
	(2.69)***	(2.70)***	
Constant	-3.2960		
	(13.28)***		
Observations	1,464		
Pseudo R-squared	0.1204		
Predicted Probability	0.0204		

Notes: The table shows the logistic regressions including the variables which passed the CDF criterion. The Marginal Effect is given in the respective column. The marginal effects were calculated at the sample means of each variable, given in the respective column. The predicted probability gives the probability predicted by the model for a transition from an autocracy to a democracy if all variables are assigned their mean value.
*/**/*** indicates significance at the 10/5/1-% level; absolute t-values are given in parentheses.

Table 2.7: Final models – remaining a democracy

Variable	**Coefficient**	**Marginal Effect**	**Variable Mean**
GDP p.c., PPP (log)	1.8440	0.0033	8.5822
	(3.78)***	(1.61)*	
Military leader	-0.8219	-0.0022	0.0414
	(1.04)	(0.65)	
Neighboring democracies	1.3768	0.0025	0.6718
	(1.17)	(0.99)	
Number of past transitions	-0.7277	-0.0013	0.7029
	(2.39)**	(1.54)	
Constant	-9.8800		
	(3.07)***		
Observations	966		
Pseudo R-squared	0.3060		
Predicted probability	0.9982		

Notes: The table shows the logistic regressions including the variables which passed the CDF criterion. The Marginal Effect is given in the respective column. The marginal effects were calculated at the sample means of each variable, given in the respective column. The predicted probability gives the probability predicted by the model for a democracy to remain a democracy if all variables are assigned their mean value.
*/**/*** indicates significance at the 10/5/1-% level; absolute t-values are given in parentheses.

2.5 Conclusions

In this chapter we summarize the vast literature on the determinants of democracy. From our literature overview, we gather 55 variables that have been previously proposed as determinants of democracy. Using dynamic logit regressions in combination with extreme bounds analysis, we test the robustness of these factors as determinants of both the emergence and survival of democracy, which we define, following Schumpeter (1942) as the political system that determines leadership through contested elections.

We find a humbling result: only five variables robustly determine the emergence of democracy while just four are reliable predictors of the survival of democracy. One central variable proposed is GDP per capita. In contrast to Modernization Theory we find that richer countries are not more likely to become democratic. They are, however, more likely to remain democracies. Both findings are in line with Przeworski et al. (2000). The other key finding is that previous political transitions destabilize the political system and facilitate future changes. This finding might be able to explain the frequent changes in the political landscape of Latin America. We find that previous transitions on the one hand increase the likelihood of a move towards democratization but on the other hand also reduce the survival probability. Learning cuts both ways: people learn how to set up democracy, but also how to subvert it. We have an interesting finding for economic growth, which makes dictatorships more likely to survive and lower the chances for democracy to emerge. This stands in stark contrast to Modernization Theory. As for the survival of democracy, we have one additional finding. The "democratic neighborhood," that is, the number of other countries in the region that are democracies increase the probability of a country's democracy surviving. The additional variables which help to explain democratic transitions are mainly country specific and beyond the control of parties involved in the political process (Muslim share, OECD membership and fuel export share). Furthermore, leaders with a military background reduce the survival probability of a democracy.

Our standard of robustness is a strong one: Sala-i-Martin's (1997) version of the extreme bounds analysis. Other variables that we test may be significant in specific

theoretical models, but they do not survive the rigors of our tests. This does not imply that they are unimportant, but it does imply that the variables that do survive may be the most important factors on which to base policy, since we can be most certain of their effects.

So what are the policy conclusions we can draw from our analysis? International organizations and Western countries often want to support democratization processes around the world. Our results imply that giving aid or other forms of transfers in order to increase a country's wealth might not be a good idea prior to a democratic transition. Only after a country democratizes from the inside such monetary help might be beneficial for ensuring the survival of a newly founded democracy. Money may be best spent targeting poor democracies. Transfers should be designed to help make the payoffs for complying with the rules of democracy more lucrative than the payoffs from subverting the regime, even in the face of poor economic conditions.

2.6 Appendix

Table 2.8: Summary of previous empirical studies on the determinants of democracy

Author	Period	Cntr.	Obs.	Democracy Measure	Explanatory Variables	Effect	Sign.	Method
Crenshaw (1995)	1980	83	83	Bolen	Democracy index, 1965	∼	-	OLS
					Real GDP p.c., log	+	++	
					Secondary school enrollment	+	+	
					British colony	+	+	
					Military personnel	-	∼	
					Agricultural density, log	+	+	
					Demographic inheritance	+	++	
					Agricultural inequality	+	-	
					Income inequality, 1970	-	-	
					Semiperiphery dummy	-	+	
					Periphery dummy	-	-	
					Commodity concentration	-	-	
					Foreign capital penetration, log	+	-	
Muller (1995)	1980	58	58	Bolen	Democracy index, 1965	+	∼	OLS
					Real GDP p.c., log	+	++	reject non-linear
					GDP growth	+	-	relationship of
					Income inequality	-	+	GDP
					Top 20% income share	-	++	
					Communist dummy	-	+	
					British colony	+	++	
					Protestant share	-	-	
					Muslim share	-	-	
					Years of continuous popular elections, log	+	-	

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Author	Period	Cntr.	Obs.	Democracy Measure	Explanatory Variables	Effect	Sign.	Method
Barro (1999)	1960-1995	103	103	Freedom	5-year lag of dependent variable	+	++	SUR
				House	10-year lag of dependent variable	+	+	
					GDP, log	+	++	
				Bollen	Years of primary schooling	+	+	
					Gap between male and female primary	-	++	
					Urban population	-	+	
					Population, log	+	∼	
					Oil country dummy	-	++	
					Life expectancy at birth, log	+	-	
					Infant mortality rate	-	-	
					Years of upper schooling	-	-	
					Income inequality	-	-	
					Share of middle class in income	+	-	
					Educational inequality	-	-	
					Ethnolinguistic fractionalization	-	+	
					Rule-of-law index	+	-	
					Former colony	-	-	
					British colony	-	-	
					French colony	-	-	
					Spanish colony	+	-	
					Portuguese colony	+	-	
					Dummy for other colony	-	-	
					Muslim share	-	-	
					Protestant share	+	-	
					Hindu share	+	-	
					Buddhist share	+	-	
					Miscellaneous eastern religion share	-	-	
					Jewish share	+	-	
					Nonreligion share	-	+	
					Other religion share	-	-	

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Author	Period	Cntr.	Obs.	Democracy Measure	Explanatory Variables	Effect	Sign.	Method
Ross (2001)	1971-1997	113	2,183	Polity IV	Oil (export value)	-	++	pooled OLS
					Minerals (export value)	-	++	
					GDP	+	++	
					Muslim share	-	++	
					OECD dummy	+	++	
					Food (export value)	+	+	
					Agriculture (export value)	+	-	
					Large states	+	+	
					Mideast	-	++	
					Sub-Saharan Africa	-	++	
					Arabian Peninsula	-	++	
					Taxes	+	++	
					Government consumption	-	++	
					Government/GDP	-	++	
					Military expenditure	-	-	
					Military personnel	-	+	
					Ethnic tensions	-	-	
					Industry employment, male	+	++	
					Industry employment, female	+	++	
					Services employment, male	+	++	
					Services employment, female	-	++	
					Secondary enrollment, male	+	-	
					Secondary enrollment, female	+	-	
					Tertiary enrollment	-	-	
					Telephone mainlines	-	++	
					TV sets	-	-	
					Life expectancy	+	-	
					Urban population	-	-	

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Author	Period	Cntr.	Obs.	Democracy Measure	Explanatory Variables	Effect	Sign.	Method
Clague et al. (2001)	1960-1994	146	146	Clague	Former British colony	+	++	two-sided tobit
					Island dummy	+	∼	period averages
				Freedom	Muslim share	-	++	
				House	Labor force in agriculture	-	∼	
					European ancestry population share	+	++	
					Share of native speakers of Colonizer	+	+	
					Language penetration by British colonizer	+	+	
					Language penetration by democratic colonizer	+	+	
					Ethnic homogeneity	+	+	
					Autocratic minority rule dummy	-	+	
					Literacy rate	+	+	
					Life expectancy, 1962 log	+	++	
Pevehouse (2002a)	1950-1992	76	1,552	Polity IV > 6,	GDP p.c.	+	+	Cox hazard
		52	805	duration	GDP p.c., change	+	∼	model
					IO score	+	-	Weibull hazard
				Gasiorowski,	IO score, change	+	+	model
				duration	Share of neighboring democracies	+	-	(here all signs are
					Previous democratic breakdown	-	-	multiplied by -1)
					Disputes in region	+	-	
					Dummy for political violence	∼	-	
					Dummy for presidential or mixed system	∼	-	
					Dummy for established democracies	∼	-	

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Author	Period	Cntr.	Obs.	Democracy Measure	Explanatory Variables	Effect	Sign.	Method
Pevehouse	1950-1992	106	2,776	Polity IV > 6	GDP p.c.	-	-	logit
(2002b)		86	2,299		GDP p.c., change	∼	-	
				Gasiorowski	IO score	+	++	
					IO score, change	∼	-	
					Number of democracies in region	+	+	
					Previously democratic	+	++	
					Disputes in region	-	∼	
					Dummy for political violence	+	++	
					Dummy for military control	-	++	
					Years of independence	+	-	
Boix and	1950-1990	135	3,991	Przeworski	GDP p.c.	+	++	dynamic probit
Stokes (2003)	1850-1990		6,143		GDP growth	+	-	
					Turnover rate of chief executives	-	++	
					Religious fragmentation	-	++	
					Share Catholic	+	-	
					Share Protestant	+	-	
					Share Muslim	-	-	
					Former colony	+	-	
					Previous democratic breakdowns	-	++	
					British colony	+	++	
					World democracy	+	+	
					Education index	+	∼	
					Percentage of family farms	+	-	
					Occupational diversification	-	-	

continued on next page...

Author	Period	Cntr.	Obs.	Democracy Measure	Explanatory Variables	Effect	Sign.	Method
Fidrmuc (2003)	1990-2000	25	250	Freedom	Economic liberalization, t-1	-	-	pooled OLS
				House	Democracy, t-1	+	++	Granger causality
					GDP growth	+	+	
					GNP per capita, log	+	+	
					War dummy	-	-	
					Distance from Brussels	-	++	
					Central planning	-	+	
Li and Reuveny (2003)	1970-1996	127	2,021	Polity IV	Openness	-	++	pooled OLS
					Net inflows of FDI	+	+	fixed effects
				Freedom	Net inflows of portfolio investment	+	+	random effects
				House	Democracy, t-1	+	++	
					Democratic countries in region	+	+	
					Inflation (GDP deflator)	+	+	
					Real GDP p.c., log	+	++	
					Real GDP growth	+	-	
					Time trend	+	++	
					Time trend · FDI	-	+	
					Time trend · portfolio	-	+	
					Time trend · inflation	-	+	
					Time trend · GDP pc	-	++	
					Memberships in international NGOs	+	++	
					Semiperiphery · GDP pc	≀	≀	
					Periphery · GDP pc	-	-	
					Europe	+	-	
					Middle East	-	+	
					Africa	-	+	
					Asia	-	-	

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Author	Period	Cntr.	Obs.	Democracy Measure	Explanatory Variables	Effect	Sign.	Method
Nieswiadomy and Strazcich (2004)	1972-2001	136	136	Freedom House	Common law	+	∼	Tobit
					Muslim law	-	++	period averages
					Resources	-	++	
					Education	+	+	
					Economic freedom	+	+	
					Ethno linguistic diversity	-	-	
					GDP p.c.	+	-	
					Infant mortality rate	+	-	
					Life expectancy	-	-	
					Population	+	-	
					Urban population	-	-	
					OPEC	-	-	
Acemoglu et al. (2005)	1960-2000	150	3,701	Freedom House	Democracy, t-1	+	+	pooled OLS
	1840-2000	27	662		Real GDP p.c., log	∼	∼	fixed effects
					Population, log	∼	∼	Anderson-Hsiao
				Bollen	Education	∼	∼	Arellano-Bond
				Polity IV	Labor share of value added	+	-	2SLS
					Trade-weighted world democracy	-	-	annual, 5-year and
					Age structure (0 to 15, 15 to 30, 30 to 45, 45 to 60, and 60 and above)	∼	∼	10-year intervals
					Crisis dummy (growth rate drop exceeding 3%; 4%; 5%, respectively)	+	++	

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Author	Period	Cntr.	Obs.	Democracy Measure	Explanatory Variables	Effect	Sign.	Method
López-Córdova and Meissner (2005)	1870-2000	?	4,184	Polity IV, change	Openness	+	++	OLS
					Population	+	+	2SLS
					Land area	∼	∼	
					Landlocked	∼	∼	
					No boarders	-	+	
					Same language	∼	+	
					Democracy, t-1	+	++	
				instruments	Distance Equator	+	++	
					Ethnolinguistic fractionalization	+	++	
					Primary commodity exporter	-	++	
					Petroleum exporter	∼	∼	
					East Asia	-	++	
					Eastern Europe/CIS states	-	++	
					Middle East/North Africa	-	++	
					South Asia	-	++	
					Western Europe	-	+	
					Sub-Saharan Africa	-	++	
					Latin America/Caribbean	-	+	

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Author	Period	Cntr.	Obs.	Democracy Measure	Explanatory Variables	Effect	Sign.	Method
Gleditsch and Ward (2006)	1951-1998	?	6,159	Polity IV > 6	GDP p.c., log	+	++	dynamic probit
					Neighboring democracies	+	++	
					Civil war	$\sim$	-	
					Years of peace	$\sim$	$\sim$	
					GDP growth	$\sim$	$\sim$	
					World democracy	+	$\sim$	
					Neighboring transition to democracy	+	++	
					Years of democracy	+	++	
					Years of autocracy	-	-	

Notes: 'Cntr.' and 'Obs.' report the number of countries and observations, respectively. A '?' identifies that the respective number is not given in the study. 'Effect' yields the sign of the coefficient: $\sim$ indicates changing signs. 'Sign.' identifies the significance of each coefficient: ++ is significant at the 1% level, + significant at the 10% level, $\sim$ indicates changing significance level, i.e., sometimes significant sometimes not. 'Polity IV' stands for the democracy measure developed by Marshall and Jaggers (2000); 'Bollen' is taken from Bollen (1993), 'Freedom House' represents Freedom House (2006), 'Gasiorowski' refers to Gasiorowski (1996) and 'Przeworski' comes from Przeworski et al. (2000).

Chapter 3

Do Autocratic States Trade Less?

3.1 Introduction

After addressing the determinants of democracy we turn our focus to its economic consequences. Is there a systematic relationship between economic and political liberalization? Does the political regime of a country systematically affect how involved that country is in international trade? The first question has received a lot of recent attention in the economic literature with studies of the determinants of democracy (as documented in the previous chapter) and economic freedom (Boockmann and Dreher, 2003; Bjørnskov, 2006; Dreher and Rupprecht, 2007) as well as studies of the relationship between democracy and economic freedom (Sturm and de Haan, 2003; Giavazzi and Tabellini, 2005).[1] The second, more specific question, is, in contrast, much less well researched and the purpose of this chapter is to provide some new answers to the question.

This chapter is an adapted version of Aidt and Gassebner (2007).

[1]Recently convergence and contagion trends of the two variables have also been studied (Nieswiadomy and Strazicich, 2004; Gassebner et al., 2007a). Furthermore, the influence of both measures on macroeconomic variables is attracting great interest (e.g., de Haan and Siermann, 1996; de Haan and Sturm, 2000; Sturm and de Haan, 2001; Bjørnskov, 2005).

Existing knowledge about how political regimes may influence international trade comes from the political science literature. Two seminal papers in this literature find that democracy encourages trade. Mansfield et al. (2000) stress the importance of the congruence between the political regime of pairs of trading countries. They show that pairs of democratic countries trade more than pairs consisting of a democracy and an autocracy.[2] Milner and Kubota (2005) test whether democratization leads to trade policy liberalizations in a sample of developing countries and show that democratic political institutions are one of the main determinants of trade policy in these countries.

We add to this literature in two related ways. First, we argue that the theoretical foundations of the previous studies (discussed in more detail in the next section) overlook the importance of differences in *political accountability* and how these differences induce societies to build more or less effective bureaucratic control mechanisms. The lack of political accountability makes it possible for political leaders to extract rents by imposing restrictions on international trade. Moreover, within a hierarchical government structure, the lack of effective control and monitoring mechanisms makes it less likely that political leaders *choose* to build a bureaucratic structure that reduces trade-distorting red tape and other unofficial trade barriers. Our theoretical contribution, therefore, predicts that autocracies – societies with weak political accountability and control structures – trade less with the rest of the world than democracies – societies with strong political accountability and well-developed control structures – for two reasons: democracy limits the scope for rent extraction via trade restrictions *and* encourages institutional reforms that reduce bureaucratic inefficiencies.

Second, the existing empirical literature focuses on dyads of countries, and for this reason it does not elude on how the political regime affects the trade performance of particular countries. Do autocracies trade less than democracies? We answer in the affirmative. By doing so we move the focus away from dyads of countries to individual countries. Furthermore, we use a much larger data set, with a longer time horizon and a deeper country coverage than previous studies. Finally, our empirical design allows

[2]Especially with their theoretical considerations and the inclusion of mixed and autocratic pairs they enhance previous work by Morrow et al. (1998) who only include pairs of democracies in their empirical analysis. Morrow et al. also find that democracies trade more with each other.

us to demonstrate that regime differences in trade policy, while playing a role, cannot fully account for the observed differences in trade flows. Both the observation that autocracies trade less and the observation that they trade less conditional on trade policy are consistent with our theoretical model.

Some authors have argued that international trade encourages democratization (e.g., Rigobon and Rodrik, 2004, López-Córdova and Meisner, 2005; O'Rourke and Taylor, 2007). This possibility obviously is a concern when trying to estimate the impact of regime type on trade flows: countries that are not involved in international trade could be autocracies for *that* reason. We attempt to deal with this issues partly by allowing for unobserved country and time fixed effects in our empirical specification, partly by lagging the empirical indicators used to capture institutional differences between countries and partly by using instrumental variables.

The rest of the chapter is organized as follows. Section 3.2 presents the model, contrasts it to existing models and develops the two hypotheses that govern the empirical investigation. Next, we develop our empirical strategy. In section 3.4, we present our main result. After that, an extensive set of tests of robustness, including IV estimates, is presented. In section 3.6, we provide some concluding remarks.

3.2 A Model of Political Regimes and Trade Flows

In this section, we present a model that illuminates two new channels through which regime types affect trade flows. One channel is the accountability channel: it is harder for citizens in autocratic countries to hold their rulers accountable because of deficient political institutions and, as a consequence, rulers are relatively free to use trade taxes to extract rents. The other channel is the bureaucracy channel: as we will show, the monitoring technology is weak in autocratic societies. As a consequence it is not in the interest of the ruler to build bureaucratic structures that weed out red tape and other distortionary unofficial trade obstructions introduced by the customs services. Both of these channels suggest that, ceteris paribus, autocracies trade less than democracies and that this continues to be true conditional on similar official trade policies.

Both of these channels are novel. The existing theoretical work on the link between political regime types and trade flows or policy have either focused on the role of international agreements or on the effect of an extension of the voting franchise.[3] The first approach is taken by Mansfield et al. (2000). They study how the incentives to enter a trade agreement differ between pairs of countries with different political regimes. The difference between democracy and autocracy is that the executive in a democracy is constrained by the fact that any trade agreement must be ratified by the legislature while the executive in an autocracy is free of such constraints. With the additional assumptions that the legislature is more protectionist than the executive and that trade negotiations take place sequentially as suggested by Putnam (1988), it follows that pairs of democracies agree on a more lenient trade policy than mixed pairs of autocracies and democracies. The reason is that a trade war is worse for a pair of democracies. While this prediction is robust to a range of different bargaining structures, the model is mute on how much pairs of autocracies trade relative to pairs democracies.[4] Our model shares the notion that the critical difference between autocracies and democracies is the lack of effective constraints on the executive in the former, but departs in three important ways. Firstly, we focus on a single country and thus on unilateral trade policy. This allows us to make predictions about how democracy/autocracy – the regime type – affects trade flows and trade policy. Secondly, we focus explicitly on the incentives that the threat of replacement provides for rulers and politicians in different types of political regimes. Moreover, our model has the advantage that democracy and autocracy can be conceptualized along a continuum controlled by three simple parameters. Thirdly, we combine an explicit economic structure with a stylized political structure.

[3]There is, of course, also a large literature on the political economy of trade protection (e.g., Hillman, 1982; Mayer, 1984; Hillman, 1989; Grossman and Helpman, 1994; Aidt, 1997). The aim of this literature is to explain trade protection within the context of competitive political systems often embodied in some form of democratic institutions rather than to explain differences between broad regimes types such as autocracy and democracy.

[4]Dai (2002) criticizes the theoretical findings of Mansfield et al. (2000) and argues that their main proposition depends on the preferences of the executives and that it is therefore not generally true that democratic pairs trade more than mixed pairs. However, as pointed out by Mansfield et al. (2002) this critique is only valid if the two-level game structure of international negotiations is replaced by a structure in which the legislature of a democracy negotiates directly with its counterpart or with the dictator if paired with an autocracy.

The other approach is taken by Milner and Kubota (2005). In particular, they maintain that the link between democratization and freer trade is an enlargement of the constituency of government that yields a shift of the median voter/supporter. Under autocracy the constituency of government is typically a small group of individuals who are well-endowed with capital. Under democracy with universal suffrage, the median voter is a worker with a low capital endowment. In countries with an advantage in the production of labor-intensive goods (e.g., in developing countries), the Stolper-Samuelson Theorem implies that the median voter benefits from trade liberalization both as a consumer and as a laborer. Our model is complementary to this. While we ignore the effect that political transitions may have on the constituency of government and the role that special interests may play both in an autocracy and in a democracy, we highlight that the degree to which rulers/politicians can be held accountable for their actions and their incentives to invest in "good" institutions varies systematically across regime types.

3.2.1 The Economy

We consider a small open economy that produces two goods and has an infinite time horizon. Good 0 is a numeraire good produced with constant returns to scale with labor as the only input and with an input-output coefficient of 1. Good 1 is produced by labor and sector-specific capital. The profit function is $\pi(p)$ where p is the domestic price of the good; p^* is the international price. Domestic supply is $\frac{\partial \pi}{\partial p} = y(p)$. Labor can move freely between sectors and consequently the wage rate in the private sector is $w^p = 1$.

The economy has two types of private agents. A tiny fraction of the population owns the sector-specific capital and they spend all profit income on good 0. A continuum of workers with measure 1 earns wage income as each supplies one unit of labor inelastically to the labor market. Workers consume both goods. Their utility function is $x_0 + u(x_1)$. Optimization subject to the budget constraint yields individual demands, $x_1 = d(p)$ and $x_0 = w^p - pd(p)$, and the indirect utility $v(p, w^p)$. All utilities are discounted with

the factor $\beta \in (0,1)$. Good 1 is traded internationally and net imports are $m(.) = d(p) - y(p)$.[5]

Trade flows are distorted by two types of policy interventions. Firstly, the ruler of the country (the government) can levy a trade tax τ on good 1. If $\tau > 0$ and good 1 is imported (exported) then τ is a tariff (export subsidy) and if $\tau < 0$ and good 1 is imported (exported) then τ is an import subsidy (export tax). To be concrete, we shall refer to τ as a tariff and thus assume that the country imports good 1.[6] Secondly, the bureaucracy in charge of regulating international trade can introduce various unofficial trade barriers, which we shall refer to as *red tape*. The per-unit cost of this is denoted by θ and we can, therefore, define the effective trade distortion, $\tau + \theta$, as the difference between the domestic and the foreign price, i.e., $\tau + \theta = p - p^*$. The revenues from the trade tax are

$$r(\tau, \theta) = \tau m(\tau, \theta) \tag{3.1}$$

where $\frac{\partial m(\tau,\theta)}{\partial \tau} < 0$, i.e., an increase in τ pushes up the domestic price which reduces domestic demand and increases domestic production. This means that $r(\tau, \theta)$ is a Laffer curve. Moreover, since $\frac{\partial m(\tau,\theta)}{\partial \theta} < 0$, red tape reduces the tax revenues raised for each value of τ.

3.2.2 Politics

The society is governed by a ruler. The ruler can either be a dictator or a democratically elected politician. We assume that the objective of the ruler is to extract rents from the

[5]We note that individual and aggregate demand for good 1 is identical.

[6]It will be clear from the objective function of the ruler that imports or exports are never subsidized. If good 1 is exported, the ruler will impose an export tax and if it is imported he will protect domestic production with a tariff. From the point of view of workers either is equally bad, so it is without loss of generality that we focus on tariffs.

economy which are spend on the numeraire good and that his utility is $u_R = r(\tau, \theta)$.[7] The ruler must, however, employ a bureaucracy to run the customs services. The task of the appointed bureaucrat is to collect tariffs, which are handed over to the ruler, but in the process he might create red tape θ. We assume that the bureaucrat benefits from red tape, e.g., because it allows him to collect bribes or because it gives the customs service more power. Red tape can either be low (absent) or high, i.e., $\theta \in \left\{0, \overline{\theta}\right\}$ where $\overline{\theta} > 0$. The rent that the bureaucrat gets from introducing red tape is θB where B is a positive constant. For simplicity, we assume that the bureaucrat only holds office for one period and that he consumes good 0 only.[8] Red tape is not in the interest of the ruler as it reduces trade flows (and hence tariff revenues), so he might want to design incentives for the bureaucrat to avoid red tape. We focus on two control instruments: monitoring and efficiency wages.

While we take monitoring to be an exogenous feature of the institutional environment, the public sector wage is endogenous. The monitoring technology discovers malfeasance with probability $1 - z$ in case of which the bureaucrat is immediately fired and he loses his wage income from the public sector and the rent from red tape and returns to the private sector where he receives w^p starting from next period onwards. With probability z, he is not discovered and he keeps the public sector wage in the current period and any rent from creating red tape and returns to the private sector in the subsequent period. We can write the expected utility of a bureaucrat who introduces red tape in period t as

$$z\left(w_t + \overline{\theta}B\right) + \frac{\beta w^p}{1-\beta} \tag{3.2}$$

[7]We could extend the model to include a public good. In this case, the ruler can only keep the difference between what is spend on public goods and total tax revenues. All our results are essentially unaffected and we prefer, for simplicity, to stick to Leviathan assumption as in Brennan and Buchanan (1980). More importantly, we do not want to assume that the intrinsic objectives of "rulers" in autocracies and democracies are different; rather we want to argue that it is the quality of institutions that forces democratically elected rulers to behave differently from dictators. There is ample evidence that rulers in particular in societies with weak institutions can extract significant rents (see, e.g., Olken, 2006).

[8]This is not important for the results and it is straight forward to extend the model to allow bureaucrats to hold office forever.

and that of a bureaucrat who refrains from doing so as $w_t + \frac{\beta w^p}{1-\beta}$ where w is the public sector wage.[9] The second control instrument is the public sector wage which is endogenous and designed by the ruler and financed out of tariff revenues. As suggested by Becker and Stigler (1974), the ruler may offer an efficiency wage that effectively ensures that no red tape is introduced:

$$w^e = \frac{z}{1-z}\overline{\theta}B. \tag{3.3}$$

Faced with the public sector wage w, the optimal choice of the bureaucrat in office in any period t can be summarized as follows:

$$\theta_t(w_t) = \begin{cases} 0 & \text{if } w_t \geq w^e \\ \overline{\theta} & \text{if } w_t < w^e \end{cases}. \tag{3.4}$$

In the absence of moderating incentives, rulers design a trade policy (τ) and a wage structure for the bureaucracy (w_t) to extract the maximum rent each period. To avoid this, societies develop political institutions that moderate the behavior of rulers. These institutions partly allow citizens to hold their rulers accountable and to replace the incumbent if he extracts too much rent and partly improves the monitoring capacity of the government (z). In a fully democratic society, elections serve the first purpose (Ferejohn, 1986; Persson and Tabellini, 2000, chapter 3), but even in autocracies and dictatorships, rulers may be constrained by the threat of a coup or a popular revolt (Acemoglu and Robinson, 2001). Formally, at the beginning of each period, workers announce a performance standard that the ruler has to satisfy to get "reappointed" at the end of the period. Workers base their performance standard on the level of utility they get from the policies implemented by the ruler and the bureaucrat. We denote the performance standard announced at the beginning of period t by $\widehat{v}_t$. The standard requires the ruler to introduce a policy package (τ_t, w_t) that yields at least the utility level $\widehat{v}_t$ in order to be considered for reappointment. We assume that only workers have political voice (set standards). This assumption is made for simplicity. We can think

[9] To ensure a positive supply of bureaucrats we assume that $\overline{\theta}B > 1$.

of it as a situation in which the ruler needs to please the masses; an assumption that makes sense in a democracy, but also in many cases in autocracies.[10]

In a well-functioning democracy, a ruler (politician) who complies with the standard is guaranteed reappointment while a ruler (politician) who does not comply is certain of dismissal. These promises are, however, not equally credible in all societies, and in autocracies or dysfunctional democracies intimidation of the opposition, electoral fraud etc. can significantly reduce the degree of accountability. We make a distinction between two types of governance failures:

Definition 3.1. *(p-failure) Workers can only promise to reappoint a ruler who satisfies* $\widehat{v}_t$ *in period* t *with probability* $p \in [0, 1]$.

Definition 3.2. *(q-failure) Workers can only promise to dismiss a ruler who does not satisfy* $\widehat{v}_t$ *in period* t *with probability* $1 - q \in [0, 1]$.

A p-failure arises when workers cannot promise for sure to reward good behavior with reappointment. This type of problem, typically, arises in situations with volatile voter turnout or general apathy among the electorate, but otherwise strong democratic institutions. A q-failure arises when citizens cannot, in all cases, dismiss under-performing rulers, and a society with q close to 1 can be interpreted as a dictatorship. Together with the monitoring technology z, these failures capture *exogenous* variations in the quality of institutions and, as we shall see, allow us to define the difference between autocracies with weak institutions (low p, high q, high z) and democracies with strong institutions (high p, low q, low z).

The interaction between rulers, bureaucrats and workers can be summarized as follows. At the beginning of each period, a new bureaucrat enters office and workers announce a performance standard. Next, the ruler decides on the tariff and the public wage for the period. After that the bureaucrat decides how much red tape to introduce and the monitoring technology determines if he is fired prematurely. At the end of the period, workers observe their utility levels, judge the performance of the ruler against

[10]The model could be extended to allow the owners of specific capital to lobby (offer bribes to) the ruler. This could be done along the lines of Aidt and Dutta (2004), but a formal analysis would distract from our current purposes.

the utility standard and decide if they want to reappoint the incumbent ruler or not. This together with random events, as captured by p and q, determines whether the incumbent is, in fact, replaced by another ruler. After this the sequence of events is repeated.

3.2.3 Analysis and Results

Given a sequence of standards $\{\widehat{v}_t\}_{t=0}^{\infty}$, the ruler faces the choice between complying and hoping to stay in power (which allows him to collect rents in the future) or not complying and collecting the maximum rent now.

If the ruler decides not to comply at time t (i.e., to deviate (D)), he sets

$$\left\{\tau_t^D, w_t^D\right\} = \arg\max_{\tau_t, w_t} r(\tau_t, \theta\left(w_t\right)) - E(w_t). \tag{3.5}$$

In doing so, he anticipates how the public wage affects the choices of the bureaucrat. It is costly to provide wage incentives and the expected wage bill is

$$E(w_t) = \begin{cases} zw_t & \text{if } w_t < w^e \\ w_t & \text{if } w_t \geq w^e \end{cases}. \tag{3.6}$$

The bureaucrat acknowledges that he only has to pay the wage if the bureaucrat is not discovered adding red tape. Clearly, either $w_t^D = 0$ or $w_t^D = w^e$ is optimal. In the former case, the optimal tariff is

$$\tau^{D1} = \arg\max_{\tau_t} r(\tau_t, \overline{\theta}) \tag{3.7}$$

and the rent is $r\left(\tau^{D1}, \overline{\theta}\right)$ for all t, and in the later case, it is

$$\tau^{D2} = \arg\max_{\tau_t} r(\tau_t, 0) - w^e \tag{3.8}$$

and the rent is $r(\tau^{D2}, 0) - w^e$ for all t. In either case, the workers attempt to replace the ruler at time $t+1$ but with probability q fail to do so. The ruler's expected payoff is

$$V_t(D) = \max\left\{r(\tau^{D1}, \overline{\theta}), r(\tau^{D2}, 0) - w^e\right\} + \beta q V^*_{t+1}, \tag{3.9}$$

where V^*_{t+1} is the continuation value of holding office at the beginning of period $t+1$. The optimal deviation policy depends on the quality of the monitoring institutions as described by Lemma 3.1.

Lemma 3.1. *(The Optimal Deviation Policy) Let*

$$\Delta R^D \equiv \frac{r(\tau^{D2}, 0) - r(\tau^{D1}, \overline{\theta})}{\overline{\theta} B} > 0$$

Then

1. *If* $\frac{z}{1-z} \geq \Delta R^D$, *then* $\left(\tau^{D1}, 0\right)$ *is optimal.*

2. *If* $\frac{z}{1-z} < \Delta R^D$, *then* $\left(\tau^{D2}, w^e\right)$ *is optimal.*

Proof. The Lemma follows from a straight forward comparison of the net rents collected by the ruler in each case using equation (3.3). ■

We note that the quality of monitoring (z) effectively determines if it is in the interest of the ruler to maintain strong wage incentives for the bureaucrat or not. If the monitoring technology is effective (z is low), it is cheap to pay efficiency wages and optimal to weed out red tape even for a ruler that has decided to disregard the demands of his citizens. If, on the other hand, the monitoring technology is ineffective (z is high), then it is better for the ruler to accept red tape and focus on maximizing tariff revenues subject to that.

If the ruler decides to comply (C) in period t, he selects the policy package

$$\left\{\tau^C_t, w^C_t\right\} = \arg\max_{\tau, w} r(\tau_t, \theta(w_t)) - E(w_t) \tag{3.10}$$

subject to $v(\tau_t, \theta_t) \geq \widehat{v}_t$. Again, the ruler either sets $w_t^C = 0$ or $w_t^C = w^e$ and we note that

$$\tau^{C1}(\widehat{v}_t) = \arg\max_{\tau_t} r(\tau_t, \overline{\theta}) \tag{3.11}$$

subject to $v(\tau_t, \overline{\theta}) \geq \widehat{v}_t$ is optimal in the former case and that

$$\tau^{C2}(\widehat{v}_t) = \arg\max_{\tau_t} r(\tau_t, 0) - w^e \tag{3.12}$$

subject to $v(\tau_t, 0) \geq \widehat{v}_t$ is optimal in the later. Since $v(\tau, \theta)$ is decreasing in τ, the ruler must reduce the tariff below the respective rent maximizing levels to satisfy the constraints. The expected payoff is

$$V_t(C) = \max\left\{r(\tau^{C1}(\widehat{v}_t), \overline{\theta}), r(\tau^{C2}(\widehat{v}_t), 0) - w^e\right\} + \beta p V_{t+1}^*. \tag{3.13}$$

As shown by the next Lemma, the quality of the monitoring institutions also plays a key role for the choice between the two possible compliance strategies.

Lemma 3.2. *(Within Period Optimal Compliance). Suppose that* $\widehat{v}_t \geq \max\left\{v\left(\tau^{D1}, \overline{\theta}\right), v(\tau^{D2}, 0)\right\}$ *and let*

$$\Delta R_t^C \equiv \frac{r\left(\tau^{C2}\left(\widehat{v}_t\right), 0\right) - r\left(\tau^{C1}\left(\widehat{v}_t\right), \overline{\theta}\right)}{\overline{\theta} B} > 0.$$

Then

1. *If* $\frac{z}{1-z} \geq \Delta R_t^C$, *then the optimal compliance policy is* $\left(\tau^{C1}\left(\widehat{v}_t\right), 0\right)$.

2. *If* $\frac{z}{1-z} < \Delta R_t^C$, *then the optimal compliance policy is* $\left(\tau^{C2}\left(\widehat{v}_t\right), w^e\right)$.

Proof. The Lemma follows from a straight forward comparison of the net rents collected by the ruler in each case using equation (3.3). ■

The sequence of performance standards is incentive compatible if and only if at all t

$$V_t(C) \geq V_t(D). \tag{3.14}$$

Workers select the sequence of standards that yields the highest lifetime utility subject to incentive compatibility. The structure of the model implies that the optimal choice is stationary; that is, $\widehat{v}_t = \widehat{v}^*$ for all t where $\widehat{v}^*$ is defined by

$$\begin{aligned} & \max\left\{r(\tau^{C1}(\widehat{v}^*),\overline{\theta}), r(\tau^{C2}(\widehat{v}^*),0) - w^e\right\} \\ = \quad & \frac{1-\beta p}{1-\beta q}\max\left\{r(\tau^{D1},\overline{\theta}), r(\tau^{D2},0) - w^e\right\}. \end{aligned} \tag{3.15}$$

Incentive compatibility requires that $p > q$; otherwise, institutions are so bad that no ruler would ever comply with any standard other than the rent maximizing one. It is also clear from equation (3.15) that workers' welfare is increasing in the quality of institutions, i.e., that $\frac{\partial \widehat{v}^*}{\partial p} > 0$ and $\frac{\partial \widehat{v}^*}{\partial q} < 0$.

We are interested in why the volume of international trade is different in autocracies and democracies. To study this, we shall make a comparison between two extremes. At one end of the spectrum, we have a society with well-functioning democratic institutions and an effective monitoring technology: $p = 1$, $q = 0$ and $z = 0$. At the other end, we have a society with seriously dysfunctional institutions: $p \leq q$ and $z = 1$. We shall refer to the former as a "democracy" and to the later as an "autocracy" acknowledging that in the real world most societies fall somewhere in between these extremes. The following Proposition states the main implications of the model.

Proposition 3.1. *(Regime Type and the Volume of Trade).*

1. *The effective trade distortion is higher in autocracies than in democracies and as a consequence, autocracies trade less with the rest of the world than democracies.*

2. *For given official trade policy (τ), autocracies trade less with the rest of the world than democracies because of differences in red tape and other unofficial trade distortions.*

Proof. Consider an autocracy with $p \leq q$ and $z = 1$. Lemma 3.1 implies that the optimal deviation entails $w^D = 0$ and $\tau^D = \tau^{D1}$, while Lemma 3.2 implies that the optimal compliance policy is $w^C = 0$ and $\tau^C = \tau^{C1}$ for all t. However, since $p \leq q$,

equation (3.15) implies that incentive compatibility fails and so, the ruler implements $\tau = \tau^{D1} = \tau^{C1}(v\left(\tau^{D1}, \overline{\theta}\right))$ and $w = 0$ each period until he is replaced by a new ruler who behaves likewise. Workers get $v\left(\tau^{D1}, \overline{\theta}\right)$ and the effective trade distortion is $\tau^{D1} + \overline{\theta}$. Consider, next, a democracy with $p = 1$, $q = 0$ and $z = 0$. Lemmas 3.1 and 3.2 imply that $w^D = w^C = w^e$ and that $\tau^D = \tau^{D2}$ and $\tau^C = \tau^{C2}$ at all t. The effective trade distortion is τ^{C2}. Let v^{**} denote equilibrium utility of a worker. Equation (3.15) implies that the best incentive compatible standard under democracy v^{**} entails higher utility than $v\left(\tau^{D1}, \overline{\theta}\right)$. It follows that $\tau_1^D + \overline{\theta} > \tau^{C2}(v^{**})$ and thus, as stated in part 1 of the Proposition, that autocracies trade less. Part 2 follows from the observation that autocracies allow red tape while democracies do not. ■

The first part of the Proposition shows that autocracies trade less than democracies. The source of this result is differences in the quality of institutions. These differences affect trade flows through two channels. First, autocracies have weak political institutions as captured by p and q. This allows autocratic rulers to extract more rents than politicians in a democracy. The implication is higher trade taxes under autocratic rule and consequently less imports (or exports). An improvement in accountability (better institutions) reduces trade taxes and encourages more trade. Second, autocracies also have weak monitoring institutions (as captured by z). As a consequence of this, autocratic rulers have little incentive to weed out red tape and other distortionary unofficial trade obstructions introduced by the bureaucrats in the customs services.[11] In contrast, in a democracy with a more effective monitoring system, it is cheap to pay efficiency wages. It is optimal for rulers to enhance institutional quality of the bureaucracy and this reduces red tape and encourages trade flows. The second part of the Proposition shows that precisely because of differences in the incentives for rulers to pay efficiency wages in the two types of societies, autocracies trade less than democracies for a given *official trade policy*.

[11]This notion corresponds to the findings of Gorodnichenko and Sabirianova Peter (2007). Focusing on the Ukraine, they find that although public sector employees receive approximately 30% lower wages as compared to those in the private sector their level of consumer expenditures and asset holdings is essentially identical. This indicates that bureaucrats receive "unofficial payments" of sizable amounts.

3.3 Empirical Specification

We want to estimate the relationship between the political regime of a country and its involvement in international trade thereby testing the two implications of our model listed in Proposition 3.1. To this end, we employ a standard gravity model of trade for a sample of up to 130 countries covering the years from 1962 to 2000. As the dependent variable, we use real import of country i from country e in year t rather than bilateral trade flows between pairs of countries.[12] Through this choice, we avoid what Baldwin (2006) calls the "silver-medal of gravity mistakes"; the problem that regressions with average bilateral trade flows as the dependent variable are subject to a sizable upward bias when trade is unbalanced.[13] More specifically, our baseline specification is the following panel model:

$$\begin{aligned} \ln(\textit{real import})_{iet} &= \beta_1 \textit{regime}_{it-1} + \beta_2 \textit{regime}_{et-1} + \beta_3 \ln(\textit{gdp}_{it}) + \\ &\quad \beta_4 \ln(\textit{gdp}_{et}) + \beta_5 \textit{landlocked}_{iet} + \beta_6 \ln(\textit{distance}_{iet}) + \\ &\quad \lambda X_{iet} + \alpha_i + \gamma_e + \delta_t + \varepsilon_{iet}, \end{aligned} \tag{3.16}$$

where $\textit{(real import)}_{iet}$ is real imports of country i from country e in year t, $\textit{regime}_{it-1}$ and $\textit{regime}_{et-1}$ are lagged values of particular measures of regime type (democracy/autocracy) of the importing and exporting country, respectively (to be discussed below), $\textit{gdp}_{it}$ and $\textit{gdp}_{et}$ are real GDP, in US dollars, of the importing and exporting country, respectively, $\textit{landlocked}_{iet}$ is a dummy variable taking the value of 1 if at least one trading partner is land locked, $\textit{distance}_{iet}$ is the distance between the most populated cities of the trading pair and ε_{iet} is an error term with zero mean. The vector X_{iet} contains a number of dummy variables that proxy the bilateral relationship between the trading partners. In

[12] Data on nominal import flows are taken from Feenstra (2000) and are converted into real import flows using the US GDP deflator. This is possible because nominal world trade is measured in dollars. Alternatively, we have also deflated nominal trade by each countries GDP deflator separately. Other than reducing the sample size due to missing data this does not change our findings.

[13] This follows from the fact that the log of the average is not equal to the average of the logs if the import and export flows are not identical in magnitude (Jensens's inequality). For a formal proof, we refer to Baldwin (2006, 18-19).

particular, the following variables are included: a dummy variable equal to 1 if the two trading partners share the same official language (*common language)*, a dummy variable equal to 1 if the trading partners have a common border (*common border*), a dummy variable equal to 1 if the trading partners were ever in a colonial relationship (*colonial ties*), a dummy variable equal to 1 if the trading partners share a common colonizer post 1945 (*common colonizer*), a dummy variable equal to 1 if the trading partners were in a colonial relationships post 1945 (*colony post 1945*), and a dummy taking the value of 1 if the trading partners are or were in the past the same nation (*same country*). Our choice of gravity variables follows Rose (2004) and we have no interest in these variables except as control variables.[14,15] We list the sources and exact definitions of all the variables used in our analysis in Table 3.1.

It is important to notice that our panel model allows us to estimate the effect of regime type on trade flows separately for an importing and for an exporting country. This allows us to test the theoretical implications of our model which would not be possible within the pair wise set-up of Morrow et al. (1998), Mansfield et al. (2000) and others.

Given the difficulty of obtaining reliable quantitative measures of regime type, we use three different indicators as proxies. They capture different aspects of the institutional environment and all have their own flaws and advantages. The first indicator is the Polity IV index constructed by Gurr et al. (2003).[16] The index is measured on a scale from -10 (autocracy) to 10 (democracy). In order to make the results obtained with this indicator comparable to those obtained with the two other indicators that we use, we re-code the variable such that higher values indicate that a society is more autocratic. The second indicator is the average of two indicators called "Political Rights" and "Civil Liberties"

[14]For further details on the gravity model, we refer to Anderson and van Wincoop (2003).

[15]However, we may note that they are all significant and have the correct sign.

[16]The Polity IV index or more accurately the "polity2" index summarizes different indicators of political authority patterns to measure three key aspects of a country's political system. The three aspects are: i) competitiveness and openness in the process of executive recruitment; ii) constraints on the chief executive and iii) competitiveness and regulation of political participation. A weighted sum of the components is used to construct two summary variables, measuring democracy on a scale from 0 to 10 and autocracy from -10 to 0. The Polity IV index is the sum of these two sub-indexes.

Table 3.1: Variables – definitions and sources

Variable	**Description**	**Source**
nimp	nominal imports in dollars (for 1962-2000) (for 2001-2003)	Feenstra (2000) Comtrade (2006)
defl	US GDP deflator (2000 = 1)	IMF (2005)
log of real imports	ln (nimp/defl)	own calculations
Polity IV*	inverse of "polity2" indicator: 1 = most democratic, 21 = most autocratic	Gurr et al. (2003)
Freedom House*	average of "political rights" and "civil liberties" indicators: 1 = most democratic, 7 = most autocratic	Freedom House (2006)
Przeworski et al.*	dummy variable taking the value of 1 for autocratic states	Alvarez et al. (1996); Przeworski et al. (2000); Cheibub and Gandhi (2004)
log GDP*	ln (GDP) (constant 2000 US$)	World Bank (2006)
landlocked	dummy for at least one trading partner being landlocked	CEPII (2006)
common language	dummy for both trading partners sharing an official language	CEPII (2006)
common border	dummy for common border	CEPII (2006)
colonial ties	dummy for pairs ever in colonial relationship	CEPII (2006)
common colonizer	dummy for common colonizer post 1945	CEPII (2006)
colony post 1945	dummy for pairs in colonial relationship post 1945	CEPII (2006)
same country	1 if countries were or are the same country	CEPII (2006)
log distance	ln of simple distance (most populated cities, km)	CEPII (2006)
restriction index*	sub-index economic restrictions of the KOF Index of Globalization; combines data on hidden import barriers, mean tariff rate, taxes on international trade (in percent of current revenue) and capital account restrictions	Dreher (2006a)
log GDP per capita*	ln (GDP/population) (constant 2000 US$)	World Bank (2006)
log population	ln (total population)	World Bank (2006)
common currency	dummy for pairs with a common currency	Rose (2004)
generalized system of preferences	dummy for pairs with a generalized system of preferences (GSP)	Rose (2004)
regional trade agreement	dummy for pairs that are a member of the same regional trade agreement	Rose (2004)
WTO membership*	dummy for WTO/GATT members	WTO (2007)

* for these variables $_i$ and $_e$ indicate the values of an importing and exporting country, respectively.

constructed by Freedom House (2006). The resulting indicator runs from 1 to 7 with higher values indicating that a society is more autocratic. The third indicator is the regime type indicator constructed by Alvarez et al. (1996) and Przeworski et al. (2000) and updated until 2000 by Cheibub and Ghandi (2004). Democracy is essentially defined as a political system in which incumbents can lose elections and are forced to comply with the results of elections. More specifically, a country is classified as a democracy if the executive and the legislature is filled through contested elections, where more than one party has a chance of winning. The resulting dummy variable takes the value of 1 for autocracies and zero for democracies.

It is hard to say which of the indicators is the "best." They have all drawn critique. The Polity IV index has been criticized for the way values are assigned to its various subcomponents. Freedom House sometimes draws critique because its indicators are completely survey based. Przeworski's regime type indicator uses the most clear-cut definition of the three, but has the disadvantage of being a dummy variable without "scales of gray." Furthermore, the three indicators focus on slightly different aspects of political institutions and can therefore perhaps best be viewed as complements rather than substitute measures of democracy/autocracy. The Polity IV index is basically a measure of political competition that ignores how widely extended the voting franchise is and other aspects of popular participation in politics.[17] The Freedom House index focuses more on political rights and civil liberties than on de facto political competition and participation. Przeworski et al. (2000) focus on a combination of political participation and contestability of political power. The complementarity of the three measures is another good reason to use all three indicators in the analysis. Finally, as argued by Milner and Kubota (2005), it takes time for changes in political institutions to affect trade patterns and the effects of democratic transitions are likely to be long-lasting. For this reason, we enter the three institutional indicators either with a one year lag or as the average of the five preceding years. This also mitigates potential endogeneity problems arising if international trade encourages the development of democratic institutions.

[17] See Aidt and Eterovic (2007) for a discussion of this.

All regressions include fixed effects for the importing and exporting country (α_i, γ_e) as well as year fixed effects (δ_t). This is a variant of the approach adopted by Feenstra (2004) who introduced the notion of country-specific effects as multilateral resistance terms. The country effects control for unobserved country characteristics that are fixed over time with the subtlety that we allow these unobservable effects to differ between importers and exporters, even if the same country is involved in import and export. The importance of correcting for these importer, exporter and time fixed effects is pointed out by Baltagi et al. (2003) as well as Baldwin (2006) who calls the omission of these effects the "gold-medal of gravity mistakes."

The baseline model allows us to test the first implication of the model, i.e., that autocratic countries trade less. The second implication of our model is that autocratic countries trade less *conditional* on official trade policy. To test this, we need to extend the baseline model with a proxy for trade policy. Given the many different forms that trade restrictions can take and the well-known difficulties in measuring trade policy (see, e.g., Milner and Kubota, 2005), we opt to include a multidimensional index. In particular, we employ the restriction sub-index from the KOF Index of Globalization (see Dreher, 2006a). This *restriction index* combines publicly available information on non-tariff import barriers, mean tariff rates, other taxes on international trade, and capital account restrictions. It ranges from 1 to 10 with higher values indicating fewer restrictions.

3.4 Main Empirical Results

The results of the estimation of equation (3.16) are shown in Table 3.2. We may begin by noting that all control variables have the correct sign and are highly significant with the exception of the landlockedness dummy variable. Given the numerous existing gravity studies, we shall refrain from interpreting the coefficients on these covariates.[18]

First and foremost, it is apparent that all three regime type indicators yield the same result: autocracies trade significantly less. Generally, the coefficients for importing and

[18]See, e.g., Rose (2004) for interpretations.

Table 3.2: Results OLS – dependent variable: ln(real imports)

	Polity IV		**Freedom House**		**Przeworski et al.**	
autocracy$_{i\ t-1}$	-0.018***	–	-0.055***	–	-0.237***	–
	(17.56)		(11.21)		(16.33)	
autocracy$_{e\ t-1}$	-0.012***	–	-0.050***	–	-0.202***	–
	(12.03)		(10.21)		(14.17)	
average autocracy$_{i\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.023***	–	-0.079***	–	-0.286***
		(18.53)		(12.56)		(16.09)
average autocracy$_{e\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.014***	–	-0.046***	–	-0.218***
		(11.07)		(7.15)		(12.48)
log GDP$_i$	1.327***	1.299***	1.260***	1.209***	1.318***	1.272***
	(67.33)	(58.22)	(50.15)	(39.72)	(67.13)	(57.07)
log GDP$_e$	1.277***	1.318***	1.230***	1.287***	1.266***	1.302***
	(66.76)	(61.37)	(50.53)	(43.46)	(66.71)	(61.08)
landlocked	-0.069**	0.031	-0.168***	-0.066*	-0.100***	-0.002
	(2.04)	(0.82)	(4.72)	(1.62)	(2.97)	(0.06)
common language	0.421***	0.390***	0.419***	0.374***	0.431***	0.399***
	(33.12)	(29.49)	(30.85)	(26.00)	(34.44)	(30.32)
common border	0.468***	0.386***	0.369***	0.250***	0.430***	0.341***
	(17.96)	(13.97)	(12.84)	(7.90)	(16.56)	(12.35)
colonial ties	0.596***	0.551***	0.614***	0.564***	0.641***	0.599***
	(18.32)	(17.32)	(18.00)	(16.83)	(20.23)	(19.28)
common colonizer	0.636***	0.545***	0.556***	0.448***	0.664***	0.591***
	(31.37)	(24.36)	(25.20)	(17.46)	(33.15)	(26.47)
colony post 1945	1.123***	1.029***	1.003***	0.931***	1.082***	0.985***
	(27.04)	(25.52)	(23.13)	(22.14)	(26.62)	(24.83)
same country	0.917***	0.809***	0.738***	0.359***	0.898***	0.783***
	(25.32)	(20.37)	(17.43)	(6.97)	(24.75)	(19.60)
log distance	-1.021***	-0.998***	-1.096***	-1.068***	-1.041***	-1.021***
	(164.59)	(152.55)	(165.70)	(148.52)	(170.99)	(157.87)
Observations	188,163	140,393	154,975	106,446	195,507	144,585
Importers	126	122	133	128	133	128
Exporters	126	123	133	129	133	129
R-squared	0.7247	0.7421	0.7317	0.7541	0.7259	0.7424

Notes: *average autocracy*$_{(t\text{-}1\text{–}t\text{-}5)}$ represents the average of the five years prior to the observation; $_i$ and $_e$ indicate importing and exporting country, respectively. Polity IV is the Variable "poliy2" from Gurr et al. (2003), Freedom House is the average of the "civil liberties" and "political rights" indicators from Freedom House (2006), higher numbers reflect higher levels of autocracy in both cases. Przeworski et al. is a dummy variable that takes the value of 1 for autocratic states. It is taken from Alvarez et al. (1996), Przeworski et al. (2000) and Cheibub and Ghandi (2004).
All regressions contain importer-, exporter- and time-specific fixed effects all of which are significant at the 1% level. */**/*** indicates significance at the 10/5/1-% level; absolute t-values are given in parentheses.

exporting countries have roughly identical magnitudes. The results have one more factor in common. The estimated coefficients on the regime type indicators are largest in the specifications that use five years averages. This indicates that the effect of regime type on trade is persistent; a finding that is in line with that of Milner and Kubota (2005). Furthermore, it suggests that changes in the trade flows take place gradually after a regime change.

Given its dichotomous nature, Przeworski's regime type indicator is the easiest to interpret. According to this indicator, democracies which turn into autocracies experience an average decrease of 23.7-28.6 percent imports and 20.2-21.8 percent exports, ceteris paribus. Given that the fixed effects setup does not allow to capture the effect of non-regime changing countries these figures are likely to underestimate the effect of autocracies. A glance at the data supports this notion. Nigeria transformed from being a democracy in 1982 to an autocracy in 1983. This shift yielded a decline of 37 percent of its imports and 26 percent of its exports.

Both the Polity IV and the Freedom House index are measured on an ordinal scale. On the 21 points scale of the Polity IV index, a one point move towards autocracy reduces imports by 1.8-2.3 percent and exports by 1.2-1.4 percent, ceteris paribus. This means that if a hypothetical country were to undergo a transition from full democracy to complete autocracy, it would lose about 36 percent of its imports and about 24 percent of its exports. On the 1 to 7 scale of the Freedom House index, a hypothetical country that went through the same transition would lose about a third of its imports and about 30 percent of its exports. To give a more concrete example. Imagine that the political regime of Switzerland was transformed into that of Myanmar in the year 2000. The consequence would be a reduction of imports and exports of 29.6 percent and 20.4 percent according to the Polity IV index and 33 percent and 30 percent according to Freedom House index, ceteris paribus. Although there are differences, it is striking how similar the results obtained with the three different indicators are.

Milner and Kubota (2005) show in a sample of developing countries that democracies have lower tariff rates than autocracies. Thus, the results reported in Table 3.2 – autocracies trade less – could simply be a result of this effect. To investigate this,

we add the restriction index, introduced in the previous section, to the specification in equation (3.16) and re-run the estimation. The results are shown in Table 3.3. Not surprisingly, the restriction index has a positive impact on trade flows and is highly significant for importing countries. This indicates that a country with few trade restrictions imports more. For exporting countries, the coefficients on the restriction index is positive and significant in some specifications. This finding is in line with standard trade and macroeconomic theory. This could, additionally, be interpreted as a "reward," i.e., a country exports more if it lowers its import barriers.

More importantly, we see from Table 3.3 that the main finding from the baseline model persists: autocracies trade less, even after controlling for differences in trade policy. The coefficients on the Polity IV index and on Przeworski's regime type indicator are somewhat lower than those reported in Table 3.2, but they are still highly significant. The coefficients on the Freedom House index remain virtually unchanged. This finding shows that the tariff channel, as identified by Milner and Kubota (2005), is not the only transmission mechanism. Our model points to two alternative transmission channels (the accountability channel and the bureaucracy channel) and our findings are consistent with the presence of both.

3.5 Robustness Analysis and IV Estimates

To see whether the results reported in Tables 3.2 and 3.3 are sensitive to changes in the specification and estimation method, we have conducted an extensive set of tests of robustness. We use the specification including the restriction index as the baseline (as reported in Table 3.3).

Firstly, we extend the model with additional control variables that have been proposed by, e.g., Rose (2004) as determinants of international trade flows. These variables are: log of GDP per capita, log of population, a dummy variable indicating a common currency, a dummy variable indicating a generalized system of preferences, a dummy variable taking on the value 1 if the trading partners are members in the same regional trading agreement, a dummy indicating WTO/GATT membership, and, finally, all of

Table 3.3: Results OLS with restriction index – dependent variable: ln(real imports)

	Polity IV		**Freedom House**		**Przeworski et al.**	
autocracy$_{i\ t-1}$	-0.013***	–	-0.054***	–	-0.161***	–
	(8.98)		(8.54)		(8.42)	
autocracy$_{e\ t-1}$	-0.013***	–	-0.063***	–	-0.180***	–
	(9.13)		(10.12)		(9.59)	
average autocracy$_{i\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.019***	–	-0.082***	–	-0.215***
		(11.94)		(10.29)		(9.82)
average autocracy$_{e\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.013***	–	-0.055***	–	-0.179***
		(8.44)		(6.91)		(8.31)
restriction index$_i$	0.109***	0.104***	0.123***	0.138***	0.118***	0.109***
	(10.71)	(10.12)	(11.86)	(12.46)	(11.61)	(10.61)
restriction index$_e$	0.020*	0.022**	0.015	-0.014	0.026**	0.020*
	(1.95)	(2.16)	(1.48)	(1.28)	(2.57)	(1.95)
log GDP$_i$	1.185***	1.152***	1.134***	1.051***	1.168***	1.117***
	(41.95)	(40.39)	(36.92)	(28.50)	(41.34)	(39.01)
log GDP$_e$	1.316***	1.339***	1.268***	1.253***	1.298***	1.315***
	(47.50)	(48.15)	(41.98)	(34.54)	(46.99)	(47.34)
landlocked	-0.077	-0.053	-0.066	-0.063	-0.090*	-0.096*
	(1.45)	(0.97)	(1.22)	(1.13)	(1.69)	(1.76)
common language	0.395***	0.356***	0.388***	0.307***	0.392***	0.352***
	(22.99)	(20.78)	(22.02)	(16.64)	(22.78)	(20.38)
common border	0.137***	0.109***	0.131***	0.154***	0.112***	0.083***
	(3.91)	(3.08)	(3.63)	(4.05)	(3.17)	(2.32)
colonial ties	0.442***	0.417***	0.402***	0.369***	0.434***	0.411***
	(10.82)	(10.74)	(9.61)	(8.99)	(10.58)	(10.53)
common colonizer	0.333***	0.318***	0.330***	0.266***	0.365***	0.345***
	(11.58)	(10.75)	(11.25)	(8.39)	(12.52)	(11.41)
colony post 1945	0.900***	0.919***	0.876***	0.873***	0.911***	0.913***
	(12.68)	(13.60)	(12.09)	(12.25)	(12.47)	(13.12)
same country	0.668***	0.547***	0.608***	0.267***	0.681***	0.551***
	(13.42)	(10.53)	(11.84)	(4.63)	(13.57)	(10.49)
log distance	-1.095***	-1.050***	-1.107***	-1.063***	-1.107***	-1.063***
	(132.76)	(126.43)	(131.96)	(120.51)	(134.16)	(127.26)
Observations	92,417	77,660	86,640	62,131	94,050	78,827
Importers	75	74	77	75	77	75
Exporters	75	75	77	76	77	76
R-squared	0.7247	0.7421	0.7369	0.7562	0.7337	0.7423

Notes: See notes to Table 3.2 for the explanation of the autocracy data. The trade restriction index is taken from Dreher (2006a).
All regressions contain importer-, exporter- and time-specific fixed effects all of which are significant at the 1% level. */**/*** indicates significance at the 10/5/1-% level; absolute t-values are given in parantheses.

the above at the same time.[19] The results of this are presented in Table 3.4. To save space, we only display the coefficients on the regime type indicators (*autoc*) in the table. We see that the results are not much affected by the inclusion of these additional variables. The significance level remains unchanged and the changes in the size of the coefficients are minuscule.

Secondly, to further elaborate on the robustness of the baseline results, we have employed different estimation techniques that reduce the risk that outliers are driving the results. Again, the specification of Table 3.3 is used as the starting point and the results are presented in the top of Table 3.5 and we only report the results for the regime type indicators. As a first step, we re-estimated the model using re-weighted least squares (RLS). This robust regression technique weighs observations in an iterative process.[20] Starting with OLS, estimates are obtained through weighted least squares where observations with relatively large residuals get smaller weights. We see that the coefficients remain highly significant although their magnitudes are somewhat reduced. Comparing the coefficients reported in Tables 3.3 and 3.5, we see that the coefficients on the political regime indicator of importing countries are approximately halved, while the coefficients for exporting countries change only minimally. Next, we used the least absolute value estimator, which minimizes the sum of the absolute deviations from the median.[21] Although the magnitude of the coefficient on the regime type indicator of importing countries is somewhat smaller, the results are comparable to those obtained with the RLS estimator and the regime type effect remains highly significant.

Thirdly, we have altered the sample and tested whether this has consequences for the results. First, we have extended the sample up to the year 2003 using trade data taken from the United Nations Statistical Division Commodity Trade Data Base (Comtrade, 2006).[22] Doing so does not change the results much. Second, Milner and Kubota

[19]Due to perfect collinearity it is not possible to include log(GDP), log(GDP per capita) and log(population) in the same regression. The results in Table 3.4 show the outcome without population. Of course, the results do not change if log(GDP per capita) is substituted by log(population).

[20]In this context we use the term "robust" as robustness with respect to the dependent variable.

[21]This is also known as mean absolute deviation (MAD) or L1 norm regression.

[22]Feenstra's (2000) data is based on this data source. He used additional data to augment the raw Comtrade data. Thus, for consistency reasons, we focus on his dataset for the main analysis.

Table 3.4: Results OLS additional variables – dependent variable: ln(real imports)

Additional Variable(s)		**Polity IV**		**Freedom House**		**Przeworski et al.**	
log GDP per capita (i and e)	$\mathbf{autoc}_{i\ t-1}$	-0.012***	–	-0.055***	–	-0.159***	–
	$\mathbf{autoc}_{e\ t-1}$	-0.013***	–	-0.065***	–	-0.178***	–
	$\mathbf{autoc}_{i\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.019***	–	-0.079***	–	-0.214***
	$\mathbf{autoc}_{e\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.013***	–	-0.053***	–	-0.179***
log population (i and e)	$\mathbf{autoc}_{i\ t-1}$	-0.012***	–	-0.055***	–	-0.159***	–
	$\mathbf{autoc}_{e\ t-1}$	-0.013***	–	-0.065***	–	-0.178***	–
	$\mathbf{autoc}_{i\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.019***	–	-0.079***	–	-0.214***
	$\mathbf{autoc}_{e\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.013***	–	-0.053***	–	-0.179***
common currency	$\mathbf{autoc}_{i\ t-1}$	-0.015***	–	-0.054***	–	-0.173***	–
	$\mathbf{autoc}_{e\ t-1}$	-0.015***	–	-0.060***	–	-0.188***	–
	$\mathbf{autoc}_{i\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.020***	–	-0.083***	–	-0.232***
	$\mathbf{autoc}_{e\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.015***	–	-0.052***	–	-0.197***
generalized system of preferences	$\mathbf{autoc}_{i\ t-1}$	-0.015***	–	-0.053***	–	-0.173***	–
	$\mathbf{autoc}_{e\ t-1}$	-0.015***	–	-0.060***	–	-0.189***	–
	$\mathbf{autoc}_{i\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.020***	–	-0.082***	–	-0.233***
	$\mathbf{autoc}_{e\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.015***	–	-0.051***	–	-0.198***
regional trade agreement	$\mathbf{autoc}_{i\ t-1}$	-0.014***	–	-0.053***	–	-0.173***	–
	$\mathbf{autoc}_{e\ t-1}$	-0.014***	–	-0.059***	–	-0.188***	–
	$\mathbf{autoc}_{i\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.019***	–	-0.081***	–	-0.228***
	$\mathbf{autoc}_{e\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.014***	–	-0.050***	–	-0.193***
WTO membership (i and e)	$\mathbf{autoc}_{i\ t-1}$	-0.013***	–	-0.054***	–	-0.160***	–
	$\mathbf{autoc}_{e\ t-1}$	-0.013***	–	-0.063***	–	-0.179***	–
	$\mathbf{autoc}_{i\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.019***	–	-0.082***	–	-0.215***
	$\mathbf{autoc}_{e\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.013***	–	-0.054***	–	-0.178***
all of the above [a]	$\mathbf{autoc}_{i\ t-1}$	-0.014***	–	-0.054***	–	-0.170***	–
	$\mathbf{autoc}_{e\ t-1}$	-0.014***	–	-0.060***	–	-0.186***	–
	$\mathbf{autoc}_{i\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.019***	–	-0.077***	–	-0.226***
	$\mathbf{autoc}_{e\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.014***	–	-0.048***	–	-0.192***

Notes: $autoc_{\ t-1}$ represents the one year lagged autocracy score while $autoc_{\ (t\text{-}1\text{–}t\text{-}5)}$ is the average of the five years prior to the observation; $_i$ and $_e$ stand for importing and exporting country, respectively; (i and e) indicate separate variables for importing and exporting countries. See notes to Table 3.2 for the explanation of the autocracy data. Only the results for the autocracy variables are shown in the table. However, the base specification is taken from Table 3.3.
All regressions contain importer-, exporter- and time-specific fixed effects all of which are significant at the 1% level. */**/*** indicates significance at the 10/5/1-% level.
[a] Due to perfect collinearity population is excluded in the estimation.

Table 3.5: Results tests of robustness – dependent variable: ln(real imports)

Technique / Sample		**Polity IV**		**Freedom House**		**Przeworski et al.**	
reweighted least squares (RLS)	$\mathbf{autoc}_{i\ t-1}$	-0.008***	–	-0.033***	–	-0.072***	–
	$\mathbf{autoc}_{e\ t-1}$	-0.013***	–	-0.059***	–	-0.161***	–
	$\mathbf{autoc}_{i\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.012***	–	-0.046***	–	-0.110***
	$\mathbf{autoc}_{e\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.013***	–	-0.047***	–	-0.172***
least absolute value (LAV aka MAD)	$\mathbf{autoc}_{i\ t-1}$	-0.004***	–	-0.033***	–	-0.043***	–
	$\mathbf{autoc}_{e\ t-1}$	-0.013***	–	-0.060***	–	-0.171***	–
	$\mathbf{autoc}_{i\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.009***	–	-0.042***	–	-0.080***
	$\mathbf{autoc}_{e\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.013***	–	-0.055***	–	-0.176***
extended sample up to 2003	$\mathbf{autoc}_{i\ t-1}$	-0.012***	–	-0.047***	–	-0.162***	–
	$\mathbf{autoc}_{e\ t-1}$	-0.011***	–	-0.061***	–	-0.181***	–
	$\mathbf{autoc}_{i\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.019***	–	-0.064***	–	-0.209***
	$\mathbf{autoc}_{e\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.015***	–	-0.054***	–	-0.183***
importer is developing	$\mathbf{autoc}_{i\ t-1}$	-0.005***	–	-0.045***	–	-0.088***	–
	$\mathbf{autoc}_{e\ t-1}$	-0.021***	–	-0.083***	–	-0.289***	–
	$\mathbf{autoc}_{i\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.015***	–	-0.072***	–	-0.166***
	$\mathbf{autoc}_{e\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.020***	–	-0.066***	–	-0.282***
exporter is developing	$\mathbf{autoc}_{i\ t-1}$	-0.020***	–	-0.051***	–	-0.247***	–
	$\mathbf{autoc}_{e\ t-1}$	-0.002	–	-0.038***	–	-0.070***	–
	$\mathbf{autoc}_{i\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.024***	–	-0.070***	–	-0.306***
	$\mathbf{autoc}_{e\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.004**	–	-0.030***	–	-0.075***
Instrumental Variables	$\mathbf{autoc}_{i\ t-1}$	-0.048***	–	-0.265***	–	-0.757***	–
	$\mathbf{autoc}_{e\ t-1}$	-0.012	–	-0.064	–	-0.260	–
	Sargan-Hansen	*0.136*	–	*0.715*	–	*0.236*	–
	$\mathbf{autoc}_{i\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.068***	–	-0.367***	–	-1.004***
	$\mathbf{autoc}_{e\ (t\text{-}1\text{–}t\text{-}5)}$	–	-0.010	–	-0.116**	–	-0.266
	Sargan-Hansen	–	*0.041*	–	*0.106*	–	*0.126*

Notes: See notes to Tables 3.2 and 3.4 for explanations on the autocracy data and the abbreviations used. Only the results for the autocracy variables are shown in the table. However, the base specification is taken from Table 3.3. In the Instrumental Variables regressions *Sargan-Hansen* reports the p-values for the test of overidentification. We instrument the autocracy variables by average party age, amount of checks and balances as well as voting in line with the G7 in the UN General assembly. The first stage F-statistic, indicating the power of the instruments, easily passes the threshold of 10 as proposed by Staiger and Stock (1997) in all specifications.
All regressions contain importer-, exporter- and time-specific fixed effects all of which are significant at the 1% level.
*/**/*** indicates significance at the 10/5/1-% level.

(2005) focus on developing countries only and to allow for a more direct comparison, we restricted the sample to include only developing countries, either as importers or as exporters, respectively.[23] We see that this reduces the magnitude of the estimated effect of regime type on trade, but with the exception of one regression, the effect remains significant. So, even within the group of developing countries, more autocratic countries trade less conditional on trade policy.

Fourthly, although we use lags of the three regime type indicators, this might not be enough to avoid all endogeneity problems and one concern about our results is that they may be contaminated by feedback effects from trade to democracy. As argued by, e.g., López-Córdova and Meisner (2005), involvement in international trade may foster democracy. If so, the coefficients on the regime type indicators reported so far might be biased. To deal with this issue, we re-estimate the model using instrumental variables (IV) techniques. In choosing the instruments, we largely follow the existing literature. Milner and Kubota (2005) use the average age of the parties in the political system as an instrument for regime type.[24] As a second instrument, we use an ordinal index of checks and balances constructed by Keefer and Stasavage (2003). Finally, we use the percentage of votes cast in line with the Group of 7 (G7) countries in the United Nations General Assembly in the IV regressions. Dreher and Sturm (2006) show that more democratic countries vote more in line with the G7. We contend that neither of these variables are correlated with the error term in equation (3.16). We report a summary of the results using all three instruments simultaneously in Table 3.5. In all specifications, the first stage F-statistic, indicating the relevance of the instruments, easily passes the threshold of 10 as proposed by Staiger and Stock (1997). We also report the p-value of the Sargan-Hansen test for over-identification and note that the test fails to reject at the 10 percent level in all, but one, specification. All specifications basically show the same pattern, namely that our previous results if anything *under-estimated* the effect of autocracy on

[23]We follow the WTO convention of coding the following countries as developed: Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Great Britain, Greece, Iceland, Ireland, Italy, Japan, Liechtenstein, Luxemburg, The Netherlands, Norway, New Zealand, Portugal, Spain, Sweden, Switzerland, USA. All other countries are coded as developing countries.

[24]The source for this data is Beck et al. (2001).

imports. All coefficients on the regime type indicators for importing countries remain significant at the one percent level and significantly increase their (absolute) size. In contrast, the coefficients on the regime type indicators for exporting countries hardly change their size but are no longer significant at conventional levels, with the exception of the specification with the five year average of the Freedom House index. Based on the IV estimates, we conclude that our previous results can be interpreted as a lower bound of the effect of autocracy on imports, while our baseline results on the impact of regime type on exports cannot be considered robust.

3.6 Conclusions

The question that motivates this chapter is a simple one: does the political regime of a country systematically affect how involved the country is in international trade? Our theoretical model provides two reasons why the answer to this question is likely to be yes. In contrast to previous theoretical work, we argue that the root cause of regime differences in trade flows is differences in political accountability. These differences affect trade flows directly through the impact on trade taxes (which are more prevalent in autocracies than in democracies), but they also work through a more subtle indirect channel. Rulers of societies with weak accountability institutions have no incentive to build up bureaucratic control structures that reduce red tape and other largely unobserved trade distortions introduced by the customs service. As a consequence, the theory suggests that not only do autocracies trade less but that they trade less conditional on official trade policy.

We test the implications of the model within the framework of a standard gravity model of international trade. This design allows us to distinguish between the effects of the political regime of an importing and of an exporting country. We find that autocracies trade significantly less than democracies, even after controlling for differences in trade policy. The magnitude of the effect is substantial: according to our most conservative estimates, autocracies have between 4.3 percent and 23.3 percent less imports and between 16.1 percent and 19.7 percent less exports, ceteris paribus. These results are

robust to a battery of different estimation techniques including instrumental variables estimates. The latter show that the relationship with respect to the exports cannot be considered completely robust. Overall, our analysis shows that autocracies import less (and maybe export less) and that this effect is not driven by differences in trade policy. We propose that it can be explained by systematic differences in the degree of political accountability. In other words, a democracy trades more with the rest of the world because democratically elected politicians are less tempted to use trade taxes to extract rents and because these politicians face the right incentives to build institutions that weed out trade-distorting red tape in the customs service.

Chapter 4

Relief for the Environment? The Importance of an Increasingly Unimportant Industrial sector

4.1 Introduction

Among the more controversial views about economic growth and globalization is that both will eventually benefit the environment (Arrow et al., 1995). In part, this view is predicated on the nature of structural changes that are normally associated with trade liberalization and economic development. More specifically, economic growth and the shift of production away from polluting sectors and "dirty" technologies help to arrest the deterioration in the environment. In addition, environmental quality is a normal good and wealthier economies will invest more heavily in environmental improvements and clean-up. According to this line of argument, another implication is that developing countries inevitably focus first on manufacturing production and basic forms of production, while tolerating some degradation in the quality of the environment. Compounding

This chapter is an adapted version of Gassebner et al. (2007b).

this feature is the fact that the political pressures associated with industrialization are also likely to be influential. The factor owners employed in manufacturing industries lobby for less regulation of polluting activities. This accelerates the decay of the environment.

With the inevitable economic decline of basic manufacturing activities in more mature economies, the declining significance of basic manufacturing in industrialized countries may very well create social pressures that reduce the demand for pollution abatement. For instance, it has been argued that greater inequality of wealth and income could be bad news for the environment (see Boyce, 1994 and Torras and Boyce, 1998). Other studies show that the pattern of sectoral resource ownership matters and that greater income inequality can yield either stricter or weaker environmental policies. For example, McAusland (2003) shows that the owners of clean factors of production may be less green voters because they may bear the burden of pollution taxes through adverse terms of trade effects on the production of "clean goods." However, in this chapter we propose the argument that associated with falling industrial wages may be declining political influence exercised by the factor owners in the polluting manufacturing industries of the economy. These latter features are likely to be manifested in the political process, i.e., voting for change and a cleaner environment. In other words, structural change may not only involve less reliance being placed on the use of polluting inputs but also may have the signal virtue of altering the demand for environmental policies.

More liberalized trade and the rapid onset of skill-biased technological change have been linked with the declining real incomes received by production workers in manufacturing industries.[1] Free trade raises national income which, in aggregate terms, increases the value placed on the environment. Political economic considerations are therefore likely to lead to a cleaner environment. Trade liberalization, which some authors continue to associate with increasing income inequality in OECD countries, may therefore be a "pro-environment" policy (see Grossman and Krueger, 1993 and Bommer and Schulze, 1999, for instance).

[1]There are many excellent surveys of the enormous literature on international trade and labor market outcomes, e.g., Johnson and Stafford (1999).

Associated with this relatively sanguine view has been an empirical relationship – in the form of an inverted U-shaped curve – between per capita income and various measures of environmental degradation. The relationship, or the environmental Kuznets curve, has been investigated for a wide variety of environmental indicators (e.g., Shafik, 1994; Selden and Song, 1994; Grossman and Krueger, 1995; Dinda, 2005). For any country, the implication is that economic growth will be associated with environmental degradation until a "critical" level of per capita income is attained; from that point, there will be an improvement in environmental conditions.

Of course, the turning points in the relationship between economic growth and environmental quality can be affected by the policies implemented by decision-makers (Shafik, 1994; Grossman and Krueger, 1995). Consequently, different political processes do not all imply that societies will grow their way out of environmental problems or that policies that promote economic growth can substitute for environmental policies.

This chapter is also indirectly related to the political economy literature that deals with the effect of income inequality on redistributive policies and economic growth (e.g., Alesina and Rodrik, 1994; Persson and Tabellini; 1994; Saint Paul and Verdier, 1996). A standard argument is that when income is more unequally distributed, the median voter is likely to be relatively less endowed with capital, either physical or human, and to thus favor a higher rate of capital taxation. A similar argument may well apply to pollution abatement policies. For instance, if the median voter is a low income worker who receives their livelihood from supplying labor to the basic manufacturing or pollution-intensive sectors, then greater income inequality may be associated with damage to the environment because it reduces the demand for pollution abatement.

However, while environmental policies are shaped by the importance of potentially affected constituencies, the relative political importance of different constituencies is likely to change over time. The idea of an interaction between industry decline and endogenous policy formation is not a novel one, of course (e.g., Cassing and Hillman, 1985). However, the perspective explored here is that the declining economic significance of polluting sectors in a developed economy is likely to be associated with greater income inequality. In turn, this is likely to reduce the "political clout" of the factor own-

ers in the polluting sectors. In particular, as the workers in these sectors of the economy become less important economically, as reflected by their falling real incomes and falling employment levels, they also become less influential politically.[2] Consequently, a regulator, motivated by political considerations, will increase the stringency of environmental regulations. Of course, dynamic comparative advantages dictate that mature, developed economies shift resources away from basic manufacturing activities.

In the next section, we set out a simple model and derive some results that highlight the relationship between the sectoral decline of manufacturing and the stringency of environmental policies. In section 4.3, we present different types of empirical evidence to test the key findings of our model. First, we show that deindustrialization may have a "silver lining" in terms of reducing emissions from basic manufacturing activities. Specifically, we show that organic water pollution and industrial employment levels are close complements. Secondly, we investigate whether labor market institutions that have traditionally supported blue-collar interests and lowered the inequality of earnings are associated with tougher environmental regulation of industry.[3] We show that a greater degree of union coordination of wage bargaining is strongly linked to the observed pattern of environmental taxation of industry relative to households. We conclude section 4.3 with a careful econometric study of panel data. In particular, we use extreme bounds analysis to examine whether countries with greater income inequality and declining manufacturing employment have more stringent environment policies. The last section concludes.

[2]This implies that "economic power" and political power are both unequally distributed (see Barro, 1999b, p.4).

[3]As unionization has declined, there is some evidence that wage inequality has increased (e.g., Freeman, 1998).

4.2 The Model

Consider an economy with two types of jobs: "blue-collar" and "white-collar", say. Further, assume that pollution creates blue-collar jobs (e.g., manufacturing) only.[4] All other jobs are white-collar (e.g., services, high-tech). Pollution afflicts all workers, however. A policy-maker must reconcile the conflict between blue-collar jobs and environmental quality, while also seeking the support of both groups of workers.

To make matters transparent, assume that the economy has a unit mass of each of two types of worker - blue-collar workers, indexed by b, and white-collar workers, indexed by w. For expositional purposes, we assume that white-collar workers are always employed. Blue-collar workers can be in one of two states at time t, employment (e) or unemployment (u). Workers receive income $y_t^{i,e}$ if they work, $i = b, w$. If unemployed, blue-collar workers receive income $y_t^{b,u}$. We assume that the $\{y_t^{i,j}\}$ are deterministic processes beyond the decision-maker's control.[5] If worker i supplies one unit of labor inelastically each period, $y_t^{i,e}$ can be interpreted as the wage rate in period t for worker i.

At time t, manufacturing generates a residual called pollution, s_t. Pollution and blue-collar labor are complementary inputs (see Cropper and Oates, 1992). If the policy-maker wants industry to create more blue-collar jobs, he must allow greater production - and pollution. The demand for blue-collar workers given by $l_t^b = f(y_t^{b,e}, s_t)$, with

$$\frac{\partial f(y_t^{b,e}, s_t)}{\partial y_t^{b,e}} = f_{y_t} < 0, \ \frac{\partial f(y_t^{b,e}, s_t)}{\partial s_t} = f_{s_t} > 0 \text{ and } \frac{\partial^2 f(y_t^{b,e}, s_t)}{\partial s_t^2} = f_{s_t s_t} < 0.$$

The pollution stock, p_t, decays at rate $\delta \in [0, 1]$. The transition equation is

$$p_{t+1} = (1 - \delta)p_t + s_t.$$

[4] In many developed countries, the combination of advanced pollution abatement technologies, as well as the toxic waste generated from office-situated photocopiers, suggests that it may not be entirely appropriate to classify blue-collar work as polluting and white-collar work as not.

[5] Apart from adding some largely irrelevant parameters to the model, allowing a dependence of income on emission flows adds very little.

Equating δ to 1 gives the classic case of a pollutant that dissipates immediately; $\delta = 0$ is the case for a pollutant that never dissipates. The utility for worker i is given by the concave function $U(y_t^i, p_t)$, for $i = b, w$. That is, all workers suffer from p_t.

In traditional political economy models it is assumed that the policy-maker maximizes a weighted average of the welfare of constituents over his career. The policy-maker might be a politician who considers voter welfare to win elections, or he might be a regulator, who considers constituent welfare to win promotions. In the current context, the political weights that a policy-maker assigns to the welfare of blue- and white-collar workers may reflect the relative political influence of the two types of workers. Different weights may be attributed to interest groups according to the degree of organization or unionization or may simply vary with size of membership, for instance.

The common agency model developed by Bernheim and Whinston (1986), and applied by Grossman and Helpman (1994), provides microeconomic foundations for the political weights that are assigned to each interest group in a society. Grossman and Helpman show that if policy-makers, when choosing a policy (s, say) care about interest groups' welfare ($V^j(s)$) on one hand and about campaign contributions on the other hand, then they actually end up maximizing a weighted sum of the interest groups' objective functions. That is, the policy-maker will choose a policy s to maximize

$$V^g(s) = \sum_{j \in I} (I^j + \alpha^j) V^j(s), \tag{4.1}$$

where $V^g(s)$ denotes the policy-maker's welfare function, I is the set of all interest groups, the indicator function, I^j, equals 1 if the interest group is engaged in lobbying activities; $I^j = 0$ otherwise.[6]

Grossman and Helpman concentrate on studying the distortionary effects of lobbying and assume that each group is originally given the same weight $\alpha^j = \alpha,\ j \in I$. From equation (4.1), it is clear that, "despite" the presence of lobbying, the outcome will be equal to the efficient solution selected by the utilitarian social planner that would assign equal weights to everybody. The political system creates inefficiencies when some groups in the economy do *not* lobby. Naturally, the policy-maker more heavily weights the policy preferences of the interest groups that do lobby (see Potters and Sloof, 1996 and Aidt, 1998). In the spirit of Grossman and Helpman, we analyze cases in which both or either of the blue- and white-collar workers may organize lobbies to help attain their preferred environmental policy settings, perhaps, via a trade union and an environmental lobby, respectively.

The welfare of *all* workers is assumed to be adversely affected by greater pollution-intensive production. However, blue-collar workers also benefit from higher pollution. While white-collar workers always prefer smaller production and less pollution; the interests and the lobbying stance of blue-collar workers depends on the elasticity of employment with respect to pollution emissions as well as the reservation utility if they

[6]If policy-makers derive utility from a weighted sum of campaign contributions and aggregate social welfare, i.e., $V^G(s) \equiv \sum_{i=W,E,K} [\alpha V^i(\alpha) + I^i C^i(s)]$, by applying Lemma 2 in Bernheim and Whinston (1986), Grossman and Helpman (1994) show that they actually end up maximizing a weighted sum of the interest groups' objective functions, i.e., equation 4.1). The application of the common agency framework by Grossman and Helpman to model the political decision-making process is not without its limitations. As a practical matter, political contributions by organized lobby groups are illegal in some countries. From a theoretical viewpoint there is a two-sided moral hazard problem associated with either politicians reneging on their policy promises after contributions are received or lobbies reneging on promised contributions once preferred policies are locked in place. However, note that the objective described by equation (4.1) is quite general and could be alternatively motivated by a linear, additive version of a political-support function (Hillman, 1982).

were to be unemployed. Consequently, the relative position of the two interest groups is generally antithetical.[7]

Returning to the problem at hand, the decision-maker's problem is

$$\max_{s_t} \sum_{t=0}^{\infty} (1+\rho)^{-t} \left((I_t^b + \alpha) W_t^b + (I_t^w + \alpha) W_t^w \right) \tag{4.2}$$

subject to $p_{t+1} = (1-\delta)p_t + s_t,\ p_0$ given, where $\rho > 0$ is the rate of time preference and $W_t^i = l_t^i U(y_t^{i,e}, p_t) + (1 - l_t^i) U(y_t^{i,u}, p_t)$, for $i = b,\ w$.[8]

Letting p_t be the state and s_t be the control, Bellman's equation is

$$V_t(p_t) = \max \left\{ \theta_t^b W_t^b + \theta_t^w W_t^w + \beta V_{t+1}(p_{t+1}) \right\}, \tag{4.3}$$

where $\beta = (1+\rho)^{-1}$ and $\theta^i \in \{\alpha,\ (1+\alpha)\}$, $i = b,\ w$.

Standard solution techniques yield the Euler equation (see Appendix for details),

$$\theta^b f_{s_t} \Delta_t + \beta \left(\theta_t^b l_{t+1}^b U_{p_{t+1}}^{b,e} + \theta_t^b (1 - l_{t+1}^b) U_{p_{t+1}}^{b,u} + \theta_t^w U_{p_{t+1}}^w \right) - (1-\delta)\beta \theta_t^b f_{s_{t+1}} \Delta_{t+1} + \beta \varepsilon_{t+1} = 0, \tag{4.4}$$

where $\Delta_t = U(y_t^{b,e}, p_t) - U(y_t^{b,u}, p_t)$.

In general, it is difficult to find closed-form solutions for the optimal s_t (or p_{t+1}) sequence. However, a simple perfect foresight example does illustrate some of the fundamental driving forces. For example, consider $U(y_t^i, p_t) = y_t^i - \gamma p_t,\ \gamma > 0,\ i = b, w$ and $f(y_t^{b,e}, s_t) = \ln(1+s_t) - \kappa \ln y_t^{b,e},\ \kappa > 0$. By appropriate substitutions into equation (4.4) and solving the difference equation, we obtain

$$s_t = \frac{(1 - (1-\delta)\beta)\, x_t}{\beta\gamma(1+\psi)} - 1, \tag{4.5}$$

[7] More specifically, since y^w is deterministic and for a threshold $\bar{p}^w \geq p_0$, then white-collar workers will *always* lobby when the stock of pollution reaches this threshold value. On the other hand, blue-collar workers lobby for laxer industry regulation until the marginal effect of higher pollution on the probability of being employed falls below the marginal disutility of greater pollution emissions.

[8] By the law of large numbers l_t^b $\left((1-l_t^b)\right)$ can be interpreted as the fraction of employed (unemployed) blue-collar workers. Recall that white-collar workers are always employed, i.e., $l_t^w = 1$.

where $x_t = y_t^{b,e} - y_t^{b,u}$ and $\psi = \frac{\theta^w}{\theta^b}$.

To highlight the effects of changing real wages for blue-collar employees we suppose that $x_t = \omega g^{-t}$, where $g > (<)\ 1$ if wages are shrinking (growing) exponentially.[9] From equation (4.5), it follows that

$$s_t = \frac{(\rho + \delta)\omega}{\gamma(1 + \psi)g^t} - 1. \tag{4.6}$$

The comparative dynamic effects are summarized in the following Proposition.

Proposition 4.1. *Suppose that blue-collar income is given by $x_t = \omega g^{-t}$, then the stringency of environmental regulations falls in*

(a) blue-collar income, ω;

(b) the policy-maker's discount rate, ρ;

(c) the pollution decay rate, δ.

Environmental regulations are stricter

(d) the higher is the rate of diminution of blue-collar income, g;

(e) the higher is the marginal disutility of pollution, γ;

(f) when blue-collar workers do not *lobby the policy-maker.*

Proof. Differentiating equation (4.6) yields parts (a) through (e). As for part (f), note that if only blue-collar workers lobby, then $\psi^b = \frac{\alpha}{1+\alpha}$, if only white-collar workers lobby, then $\psi^w = \frac{1+\alpha}{\alpha}$, and if both groups lobby, then $\psi^{b,w} = 1$. Clearly, $\psi^b < \psi^{b,w} < \psi^w$. Finally, note that environmental quality improves in ψ. ■

There are some transparent implications. For example, if the policy-maker discounts the future more heavily, then this is associated with deteriorating environmental quality. Congleton (1992) shows that autocratic countries are inclined to select less stringent

[9] Alternatively, g may represent a direct measure of wage inequality between white- and blue-collar workers. For example, define $y_t^{w,e} \Big/ y_t^{b,e} = g_t$ and $y_t^{b,u} = 0$.

environmental regulations. He argues that dictators tend to have shorter time horizons (i.e., higher ρ) and are less likely to adopt pro-environment policies, since the benefits of doing so are likely to accrue only after they have left office, whereas the costs are incurred earlier.[10] A higher pollution decay rate, δ, also increases pollution. This somewhat counter-intuitive result occurs because worker utility depends on p_t, and not on p_{t+1}, so that the policy-maker is likely to take a less conservative attitude with a pollutant that dissipates immediately, as opposed to the case for a pollutant that never dissipates. Consistent with this finding is that policy-makers are more likely to be "policy-active" for the types of pollutants with short-term and local impacts (see Emerson and Pendleton, 2004).

Higher income for workers in pollution-producing industries (ω) is associated with an increasing amount of economic importance attached to the polluting sector of the economy. However, if this economic "weight" falls, then because environmental quality is a normal good, the stringency of environmental regulations rises over time and consequently, so too does the quality of the environment. Strictly speaking, it is not just the continued erosion of the earnings of blue-collar workers that beneficially impacts pollution emissions. More generally, it is the falling *relative* earnings of working in the polluting sector. For example, if the income while unemployed increases more rapidly than the income while employed in the polluting sector, then the same benefit to the environment results. Thus, some authors have argued that more generous unemployment benefits and changes to cash transfer and income tax systems have arisen to ensure worker acquiescence to potentially disruptive microeconomic reforms, such as trade liberalization (e.g., Rodrik, 1998). Hence, while the earned income distribution may have widened in many OECD countries, the same is not true for the post-tax and post-government transfer distribution of income (see, e.g., Smeeding and Gottschalk, 1995).

Recall that when each interest group receives an equal political weight that this is equivalent to the utilitarian social planner's problem. However, if a lower political weight

[10]For reasons other than the expected shorter duration of dictatorships, Olson (1993) argues that dictators wish to maximize tax revenues and thus oppose any policies that would reduce revenue, e.g., those that result from increased pollution abatement expenditures.

is attached to blue-collar worker interests (ψ), then less importance is attached to the polluting sectors of the economy. As noted above, the increased likelihood of free-riding in larger political constituencies poses problems for a straightforward interpretation of the political weights attached to interest groups by policy-makers. In democratic countries, government officials may favor groups with more members. Larger groups are also likely to have greater electoral resources. However, groups with more members tend to be prone to free-riding problems. In addition, larger groups are likely to be costlier to organize, more difficult to develop a coherent and consistent platform for, and to involve greater difficulties in ensuring the political participation of all members. Potters and Sloof (1996) provide a fairly comprehensive survey of the empirical effects of group size on political outcomes. Overall, they conclude that free-riding is, in fact, a serious problem for larger, *unorganized* groups. On the other hand, larger, *organized* groups, such as trade unions, for example, wield greater influence. In deindustrializing economies, the reduction in blue-collar power has in part been manifested by the declining influence of trade unionism (see Freeman, 1993). Clearly, deunionization is likely to reinforce the declining political significance attached to the blue-collar interests in relaxing environmental standards.[11]

4.3 Empirical Implications and Evidence

In this section we present evidence to examine the predictions of the model. More informally, in the next two sub-sections we present some simple tabulations and correlations. First, we show that industrial employment and polluting activity go hand-in-hand. Secondly, we show that more unionized economies, which also tend to have more equitable earnings distributions, favor the imposition of eco-taxes on consumers rather than on industry. Finally, and more importantly, we present a formal econometric analysis using panel data to investigate the determinants of the stringency of environmental policy.

[11]Fredriksson and Gaston (1999) show that the stance taken by the trade union movement on the environmental policies is far from unambiguous. Among other things, union "environmentalism" may depend on the risk of unemployment for their members as well as the presence of unemployed, non-unionized "outsiders."

4.3.1 Deindustrialization and the Environment

A key feature of the model is that deindustrialization is associated with a cleaner environment. This occurs for two reasons – an economic one and a political one. The economic reason involves the trade-off between a cleaner environment and the production of basic manufacturing goods. In turn, higher employment in this sector is associated with greater pollution emissions.

Political pressures also imply a positive correlation between manufacturing employment levels and pollution emissions. For example, if underlying economic growth and dynamic comparative advantages reduce production and employment in basic manufacturing activities, then the remaining workers in these sectors are likely to receive smaller consideration in the political process. Consequently, policy-makers weigh more heavily the preferences of workers (and voters) involved in the production of "cleaner" goods. Doing so, of course, simply reinforces the decline in basic manufacturing industry.

To provide a visual perspective of the relationship between industrial employment and pollution emissions, we present plots for organic water pollutants and industrial employment for seven countries in Figure 4.1. The data are from a study of industrial emissions for a limited number of countries by Hettige et al. (1998). Data on water pollution are more readily available than other emissions data because most industrial pollution control programs start by regulating organic water emissions. Such data are also fairly reliable because sampling techniques for measuring water pollution are more widely understood and much less expensive than those for air pollution. The emissions estimates represent biochemical oxygen demand (BOD) in kilograms per day for each country and year.[12] The employment data are from the United Nations Industrial Development Organization (UNIDO). The data series for each country are for 1975

[12]Emissions of organic pollutants from industrial activities are a major source of water quality degradation. The Hettige et al. (1998) data are based on measurements of plant-level water pollution in a number of countries. The focus is on organic water pollution as indicated by the presence of organic matter, metals, minerals, sediment, bacteria and toxic chemicals. The pollution is measured by biochemical oxygen demand, BOD, because it provides the most plentiful and reliable source of comparable cross-country emissions data. BOD measures the strength of an organic waste in terms of the amount of oxygen consumed in breaking it down. A sewage overload in natural waters exhausts the water's dissolved oxygen content. Wastewater treatment, by contrast, reduces BOD. (The previous discussion is drawn from World Bank (1999, p.143).)

to 1992 (or 1993). For ease of comparison, the emissions and employment data were converted to indices (with 1975 as the base year).

A couple of observations are immediate. First, changes in industrial employment and water pollution are strongly complementary.[13] For instance, Canada suffered very steep reductions in its manufacturing employment in the late 1980s and early 1990s due to an unexpectedly severe recession and to the, somewhat more debatable, effects of the passage of the Canada-U.S. Free Trade Agreement (see Gaston and Trefler, 1997). In non-OECD countries in which manufacturing employment has grown rapidly (e.g., Singapore) there has been a corresponding increase in pollution emissions.[14]

In those countries that have experienced the most marked deindustrialization (see Baldwin and Martin, 1999), e.g., the United Kingdom, pollution emissions have steadily fallen as employment in manufacturing industry has declined over the entire time period. In the relatively few developed countries that have not deindustrialized as rapidly, emissions have remained relatively unchanged.[15]

4.3.2 Unions and the Environment

Next, we examine a subsidiary implication of the model developed in section 2. Specifically, as the political institutions that have traditionally supported blue-collar interests have declined in importance, associated environmental regulations have toughened.

Portney (1982) argues that increasing unemployment pressures policy-makers to ease environmental standards.[16] By implication, political pressure brought to bear on envi-

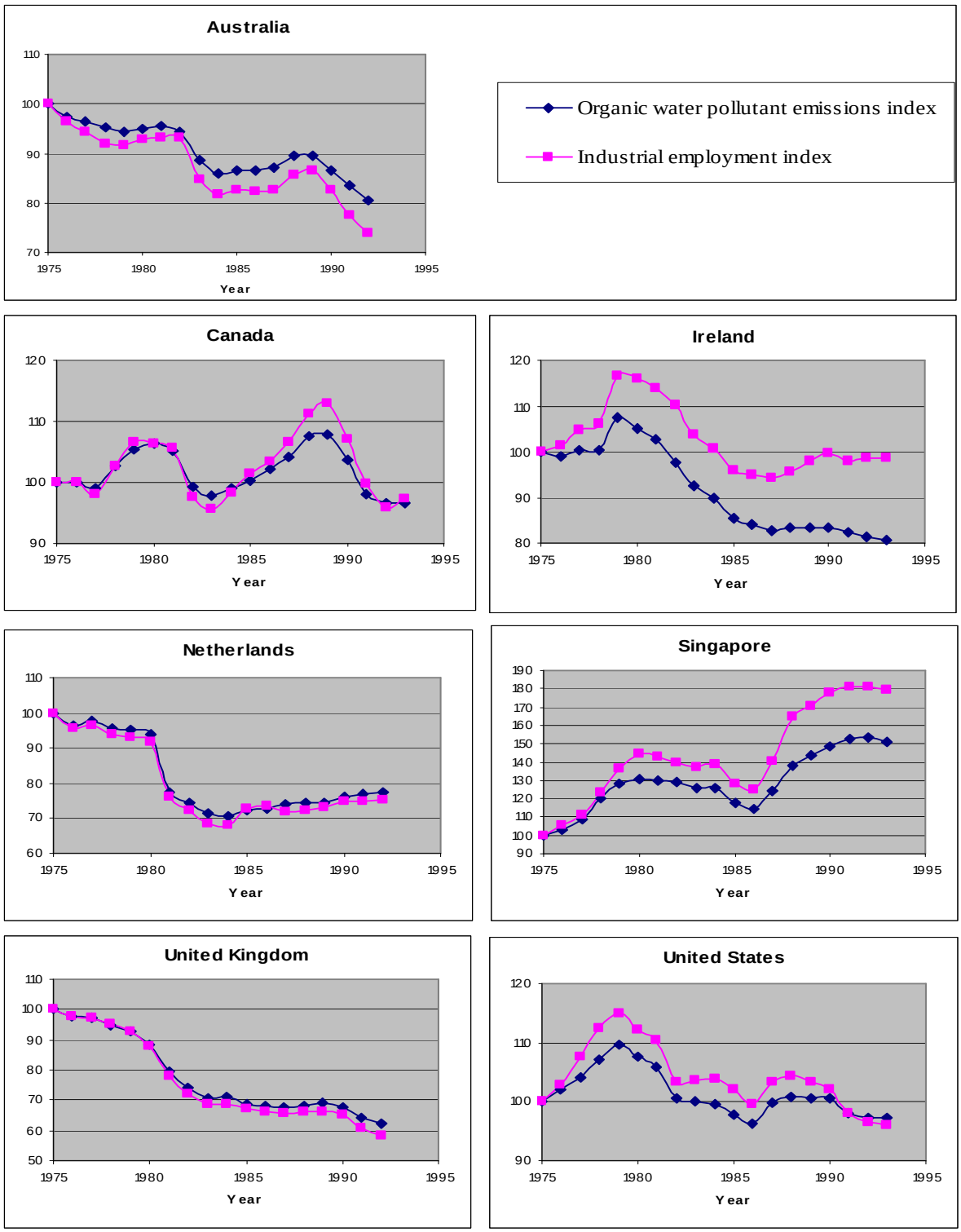
[13]Obviously, one cannot imply causation from this graphical analysis. However, as Gassebner et al. (2006) show, industrial employment is indeed a determinant of water pollution.

[14]In the case of Ireland, the growing disparity between the two indices may reflect the rapid increase in the service sector industries associated with Ireland's "green tiger." We are grateful to an anonymous referee for this observation.

[15]Baldwin and Martin (1999) note that the "first wave" of globalization (pre-WW1) which generated rapid economic development for many countries was characterized by rapid industrialization. In contrast, the "second wave" of globalization (since 1960), which generated rapid income growth for many developed countries, has been characterized by a process of deindustrialization and the associated steady decline in industrial employment.

[16]In reference to the stance taken by European labor unions on environmental regulation, Klepper (1992, p.253) notes that the primary objective of securing or increasing employment was thought to be threatened by environmental policies.

Figure 4.1: Water pollution and industrial employment



Source: Authors' calculations based on data from Hettige et al. (1998).

ronmental policy-makers is greater when industrial employment levels fall. Fredriksson and Gaston (1999) have noted that unions lobbying on behalf of unemployed members may encourage policy-makers to respond favorably to calls for easing environmental restrictions. Yandle (1983) found that state expenditures on environmental regulation in the United States were negatively related to the number of workers in polluting industries and positively related to the percentage of the manufacturing industry workforce that was unionized. He interpreted the former relationship as evidence that policy-makers operate according to an environmental quality versus jobs trade-off and the latter relationship as evidence of union rent-seeking.[17]

Overall, one expects that the decline of unionization in many countries has helped the passage of more stringent environmental regulations that affect industry. On the surface, the evidence on this point is somewhat mixed. According to Tobey's (1990) indices of environmental stringency, two of the three countries with the strictest environmental standards (the United States and Japan) have among the lowest rates of union membership in the world (as well as the lowest percentage of workers covered by collective bargaining agreements). However, the third, Sweden, has among the highest rates of unionization in the world. Fredriksson and Gaston (1999) explain this phenomenon by noting that the ambiguous stance of the trade union movement on environmental policies depends on the exposure to unemployment of their *own* members. It needs to be emphasized that it is the actual level of industrial employment, rather than the rates of unionization of a presumably smaller pool of manufacturing workers in deindustrializing economies, that may be of greater significance for policy-makers.

In this chapter, special interest groups, representing blue-collar and white-collar interests, help to determine the stringency of environmental policy. In most countries, trade unions are the most visible advocates of blue-collar interests.[18] If blue-collar

[17]Endersby and Munger (1992) found that union contributions were given disproportionately to members of Congress who were members of committees with legislative and regulatory jurisdiction over activities that would affect labor.

[18]Of significance, for the purpose of this chapter at least, is that countries with encompassing labor market institutions (i.e., large unionized sectors with centralized bargaining) are characterized by lower wage inequality (see e.g., Rowthorn, 1992; Freeman, 1993; OECD, 1997).

Table 4.1: Total taxes as percent of end-user price for automotive fuels, 1994

Country	Household Use (1)	Industrial Use (2)	Ratio (2)÷(1)	Union co-ordination index
Denmark	68.0	41.5	0.61	3
Sweden	76.5	48.3	0.63	3
Norway	67.3	46.0	0.68	3
Austria	63.9	49.1	0.77	3
Belgium	74.2	57.3	0.77	2
Netherlands	75.9	59.7	0.79	2
France	80.8	65.1	0.81	2
Portugal	73.5	59.4	0.81	2
Germany	76.9	62.5	0.81	2
Spain	68.6	56.9	0.83	2
Canada	50.0	41.6	0.83	1
Italy	76.1	65.1	0.86	2
U.K.	73.5	63.6	0.87	1
Switzerland	71.3	68.9	0.97	1
U.S.A.	34.4	39.6	1.15	1

Sources: Columns (1) and (2): OECD (1995), Table 2, p.48. Last column: Layard et al. (1994), Table 6, p.78. values represent: 3 = High (National coordination); 2 = Intermediate; 1 = Low (firm-level or uncoordinated).

workers perceive a trade-off between environmental regulations and jobs, unions are likely to oppose policies that threaten manufacturing employment.

Many OECD countries have recently introduced, or are considering implementing, fiscal instruments or "eco-taxes" for environmental management. Consider Table 4.1, which illustrates a specific example of an "eco-tax." The data in columns (1) and (2) contain data on tax rates for household-use and industrial-use fuel for a number of OECD countries. A number of features are apparent. For example, the household use tax rate is highest in France and the industrial use tax rate is highest in Switzerland and both tax rates are lowest for the United States. The differences in tax rates across countries reflect a number of influences, including such disparate factors as the political importance of community environmental concerns as well as fiscal considerations.

Of more interest is the difference between the rates of taxation for industrial and household use. Column (3) indicates that the tax rate on industrial use fuel is 61 percent of the tax rate on household use fuel in Denmark; in the United States, the industrial

use tax rate is 15 percent higher than the household use tax rate. Once again, there are likely to be a number of determinants of these cross-national differences. However, these differences are also likely to reflect the importance of industry concerns (i.e., shareholders and workers). Environmental and community concerns are likely to be reflected in the tax rate levels. On the other hand, national differences in the *relative* tax rates for industrial and household fuels are likely to reflect the *relative* influence of industry vis-a-vis households in the political process in which tax rates are determined. Moreover, institutional features of the labor market are important determinants of industry and union lobbying incentives, and consequently, the observed pattern of environmental policy.

Consider the last column of Table 4.1 – "Union coordination index." Layard et al. (1994, pp.80-81) argue that when unions have a national focus (designated by an index of '3'), they take into account the common interests of the workforce in full employment "rather than bargaining as atomistic groups of insiders" (designated by an index of '1'). The data reveal that bargaining at the national level is negatively related to the tax rate disparity (the 'Ratio' column). Of course, this correlation may be purely coincidental. On the other hand, it appears that a strong coordinated union movement is associated with relatively higher tax burdens on households (i.e., which comprise blue-collar and white-collar workers) as opposed to industry (which primarily employ the blue-collar workers). Overall, unionization does appear to be strongly linked to the observed pattern of environmental taxation of industry relative to households.

4.3.3 Inequality, Industrial Employment and Environmental Regulation

To conclude the empirical section, we present a formal econometric analysis of the determinants of environmental policy. As much as possible, we follow the empirical specifications previously suggested in the literature. The major innovation, of course, is the introduction of variables suggested by our own model. Another major step forward

is our use of extreme bounds analysis to isolate the determinants of environmental regulation which are the least sensitive to specification changes.

Data and variables

To proxy environmental stringency we use the lead content of gasoline. This measure has been used in previous research (e.g., Damania et al., 2003); its major advantage is that the data are available as a panel for the period from 1982 to 1992 and for up to 48 countries. We transform the series by taking the logarithm of it and multiplying it by (-1). It is denoted by *LREGS*.[19]

Our primary focus is examining the importance of blue-collar workers for the political process that shapes the environmental policy. Damania et al. (2003) use the percentage of the labor force employed in industry (*INDSHEMP*) to proxy political pressure by industrial workers.[20] This pressure is also central to our model's predictions. Since environmental regulations may increase employment uncertainty, industrial workers use their political power to prevent stricter regulations. The other variable important in our model is wage inequality. The stringency of environmental regulations is predicted to increase as blue-collar income declines. If wages fall exogenously (e.g., due to skilled-labor biased or sector-biased technological change that favors white-collar workers), then we predict a more stringent environmental policy (i.e., a lower lead content of gasoline).

In a recent paper, McAusland (2003) argues that greener pollution policies could be associated with either greater or smaller income inequality. In earlier research, inequality has often been associated with an intensification of polluting activities (e.g., Boyce, 1994; Torras and Boyce, 1998). Our model's predictions point in precisely the *opposite* direction. That is, as an economy deindustrializes income inequality may increase as the wages paid to manufacturing workers in low-tech, pollution-intensive industries fall. As

[19] A rise in the transformed *LREGS* therefore represents a higher level of environmental stringency. Hilton and Levinson (1998) and Octel (1982-92) provide a more detailed description of these data.

[20] Unless stated otherwise, all data is taken from the World Bank's World Development Indicators (World Bank, 2003).

it does so, the influence of blue-collar workers in the policy-making process declines.[21] To measure income inequality we use the Gini coefficient data recently updated and re-calculated by Francois and Rojas-Romagosa (2005).[22]

Needless to say, a large number of other variables have been proposed as determinants of the level of environmental stringency. Cole et al. (2006) use the urban population share (*URBAN*) to test whether a greater exposure to industrial pollution by a larger number of citizens increases environmental stringency. Cole et al. (2006) also argue that the demand for environmental quality increases with per capita income (*LGDPPC*). On the other hand, Congleton (1992) emphasizes that the effect of per capita income is theoretically indeterminate (even though he estimates a positive relationship in his study). He argues that despite the fact that the demand for environmental quality is likely to be increasing with personal wealth, voters and tax-payers also have to bear a higher share of the costs associated with environmental regulations. These costs reduce national income. A similar ambiguity is predicted for the effect of population density (*LPOPDENS*). Congleton argues that population also serves as a proxy for a country's human capital resources.

Damania et al. (2003) contend that more open economies will have higher environmental standards. McAusland (2003) shows that trade openness and the pattern of factor ownership are important determinants of the preference for pollution standards. If an economy is small and open then environmental policies have no effects on the terms of trade. Hence, if the poor have a larger relative stake in the production of dirty goods, then they may vote for weaker policies when the economy is open because there are no beneficial terms of trade effects associated with environmental regulations. We therefore use trade intensity (*TRADE*), measured by the ratio of trade flows to GDP. Another commonly used openness measure is foreign direct investment, which we measure as the

[21] Consistent evidence is provided by Taylor (1998). He uses data for State expenditures per capita in the United States for hazardous waste in the 1980's and for air pollution in the 1960's and rejects the hypothesis that there is a trade-off between future environmental quality and current manufacturing jobs.

[22] These authors address measurement error problems in the well-known World Bank inequality dataset of Deininger and Squire (1996) and produce a new dataset of consistent inequality series.

ratio of the net inflows of FDI to GDP (*FDIGDP*).[23] As a final proxy for openness we employ the KOF Index of Globalization (*GLOBAL*) (see Dreher, 2006a).[24]

Congleton (1992) argues that autocratic countries have lower environmental standards because their rulers have shorter time horizons. Consequently, the incentives to invest in environmental protection are lower. Following Congleton, we also include a dictatorship dummy (*DICT*) which takes the value one if the executive index of electoral competitiveness is smaller than three (see Beck et al., 2001). In addition, we employ *POLFREE* which we measure as the average of the Freedom House (2005) indices for civil liberties and political rights. Another included variable is *LEFT*, which measures whether the chief executive has a left-wing orientation or not.[25] Neumayer (2003) argues that a left-wing executive is traditionally more likely to care for the interests of blue-collar workers. As they work mostly in dirty sectors this may reduce environmental stringency (see also Fredriksson and Gaston, 1999). However, Neumayer notes that left-wing governments might also be more amenable to policies that protect the environment.[26]

Damania et al. (2003), Fredriksson et al. (2003) and Fredriksson and Svensson (2003) emphasize the role that corruption might play in affecting the political agenda. Accordingly, we include *CORRUPT* to measure the level of government corruption. This variable is the "Government Honesty" variable reported by the International Country Risk Guide (ICRG).[27] For a summary of all variables, their sources, their descriptions as well as the study that originally proposed them see Table 4.2; Table 4.3 gives the descriptive statistics and correlations of the variables.

[23]Fredriksson and Gaston (2000) argue that greater capital mobility weakens the incentive for capital owners to lobby, and ceteris paribus, may lead to stricter environmental policies.

[24]This index incorporates economic as well as the political and social dimensions of globalization.

[25]This variable is taken from Beck et al. (2001).

[26]Neumayer argues that blue-collar workers are likely to be among the first exposed to the effects of environmental degradation.

[27]For details see Knack and Keefer (1995).

Table 4.2: Variables – definitions, sources and hypotheses

Variable	**Source**	**Description**	**Sign**	**Proposed by**
LREGS	Octel (1982-1992)	Log of lead content of gasoline, multiplied by (–1)		Damania et al. (2003)
CORRUPT	ICRG	"Government Honesty", higher values indicate less corruption	+	Damania et al. (2003)
DICT	Beck et al. (2001)	Dummy variable for dictatorship (executive index of electoral competitiveness < 3)	-	Congleton (1992)
FDIGDP	World Bank (2003)	Net inflows of foreign direct investment (% of GDP)	+	Cole et al. (2006)
GLOBAL	Dreher (2006)	KOF Index of Globalization	+	This chapter
INDSHEMP	World Bank (2003)	Employment in industry sector (% of total employment)	-	Damania et al. (2003)
INEQUAL	Francois and Rojas-Romagosa (2005)	Gini coefficient – household income	+	This chapter
LEFT	Beck et al. (2001)	Dummy variable for the chief executive's party being left-wing	?	Neumayer (2003)
LGDPPC	World Bank (2003)	Log of GDP per capita (in constant 1995 $US)	?	Congleton (1992)
LPOPDENS	World Bank (2003)	Log of population per hectare	?	Congleton (1992)
POLFREE	Freedom House (2005)	Average of "Civil Liberties" and "Political Rights"	+	Congleton (1992)
TRADE	World Bank (2003)	Trade intensity ((imports + exports)/GDP)	+	Damania et al. (2003)
URBAN	World Bank (2003)	Urban population (% of total)	+	Damania et al. (2003)

Notes: 'Sign' refers to the expected sign of the variable according to the literature '+/-' indicates a positive/negative sign while '?' represents an *a priori* indeterminate effect.

Table 4.3: Variables – descriptive statistics and correlation matrix

	Mean	S.D.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
(1) LREGS	-0.37	0.95	341	0.418	-0.225	-0.047	0.528	0.349	-0.439	-0.110	0.490	0.285	-0.396	0.101	0.383
(2) CORRUPT	3.49	1.55	328	328	-0.360	0.190	0.769	0.533	-0.381	0.130	0.707	-0.208	-0.540	0.340	0.608
(3) DICT	0.22	0.42	341	328	341	-0.138	-0.357	-0.264	0.471	-0.167	-0.537	-0.062	0.635	-0.172	-0.372
(4) FDIGDP	1.17	1.71	324	311	324	324	0.250	0.127	0.087	0.068	0.131	-0.240	-0.182	0.504	0.060
(5) GLOBAL	1.91	0.74	310	310	310	293	310	0.502	-0.582	0.053	0.865	-0.013	-0.706	0.438	0.771
(6) INDSHEMP	26.18	8.03	194	194	194	192	193	194	-0.399	0.180	0.724	0.037	-0.527	0.096	0.565
(7) INEQUAL	43.37	10.49	57	57	57	57	57	52	57	-0.159	-0.428	-0.365	0.516	-0.429	-0.037
(8) LEFT	0.36	0.48	332	319	332	315	302	188	55	332	-0.104	-0.193	-0.040	-0.143	-0.212
(9) LGDPPC	7.78	1.76	335	322	335	324	304	194	57	326	335	0.017	-0.797	0.327	0.869
(10) LPOPDENS	-0.45	1.36	330	319	330	313	310	194	57	322	324	330	-0.110	0.187	-0.058
(11) POLFREE	3.32	1.92	341	328	341	324	310	194	57	332	335	330	341	-0.248	-0.670
(12) TRADE	49.28	27.30	333	320	333	322	302	194	57	324	333	322	333	333	0.235
(13) URBAN	52.34	25.74	341	328	341	324	310	194	57	332	335	330	341	333	341

Notes: The first two columns report the mean and the standard deviation (S.D.) of each series; the upper-right part of the remaining table reports correlation coefficients, the main diagonal gives the number of observations for each variable, while the lower left shows the number of observations used to calculate the correlation coefficients.

Extreme Bounds Analysis

Since there are several studies that investigate the effects on environmental stringency, there is a long list of potential explanatory variables. Studies often restrict their analysis to certain subsets of these variables and often ignore the effects of any omitted variable bias when other variables are not included. In addition to any model uncertainty, the limited number of observations often restricts the power of statistical tests that rule out irrelevant explanatory variables.

In order to address these issues we use extreme bounds analysis (EBA), as proposed by Leamer (1983) and Levine and Renelt (1992). EBA enables us to examine which explanatory variables are robustly related to our stringency measure and is a relatively neutral way of coping with the problem of selecting variables for an empirical model in situations where there are conflicting or inconclusive suggestions in the literature.

To conduct an EBA, equations of the following general form are estimated

$$Y = \beta_M M + \beta_F F + \beta_Z Z + \upsilon \tag{4.7}$$

where Y is the dependent variable, M is a vector of "commonly accepted" explanatory variables and F is a vector containing the variables of interest. The vector Z contains up to three possible additional explanatory variables (as in Levine and Renelt, 1992) which, according to the broader literature, are related to the dependent variable. The error term is υ. The EBA test for a variable in F_k states that if the lower extreme bound for β_{F_k} – i.e., the lowest value for β_{F_k} minus two standard deviations – is negative, while the upper extreme bound for β_{F_k}– i.e., the highest value for β_{F_k} plus two standard deviations – is positive, the variable F_k is *not* robustly related to Y.

Sala-i-Martin (1997) argues that this testing criterion is far too strong for any variable to ever pass it. If the distribution of the parameter of interest has both positive and negative support, then a researcher is bound to find at least one regression model for which the estimated coefficient changes sign if enough regressions are run. Consequently, in what follows we not only report the extreme bounds, but also the percentage of the regressions in which the coefficient of the variable F_k is statistically different from zero.

Moreover, instead of only analyzing the extreme bounds of the estimates of the coefficient of a particular variable, we follow Sala-i-Martin's (1997) recommended procedure and analyze the entire distribution. Accordingly, we also report the unweighted parameter estimate of β_{F_k} and its standard error, as well as the unweighted cumulative distribution function, CDF(0). The latter represents the proportion of the cumulative distribution function lying on each side of zero. CDF(0) indicates the larger of the areas under the density function either above or below zero, i.e., whether this happens to be CDF(0) or 1 - CDF(0). So CDF(0) always lies between 0.5 and 1.0. However, in contrast to Sala-i-Martin, we use the unweighted, instead of the weighted, CDF(0).[28]

Another objection to EBA is that the initial partition of variables in the M and in the Z vector is likely to be arbitrary. However, as pointed out by Temple (2000), there is no reason why standard model selection procedures (such as testing down from a general specification) cannot be used in advance to identify variables that are particularly relevant. Furthermore, some variables are included in the large majority of studies and are by now common in this branch of the literature.

In our view, the inclusion of *LGDPPC* in the M vector is the only non-contentious inclusion as a regressor. In the literature on the environmental Kuznets curve the relationship between GDP per capita and environmental quality has been widely discussed. Therefore, this variable may also play an important role in determining the stringency of environmental policy. While it is tempting to include our central variables (*INDSHEMP* and *INEQUAL*) in the M matrix, we are conscious of not prejudging the importance of our model and the outcome of the EBA.

The results

As a preliminary to the EBA, we ran a first regression using *LGDPPC* as well as our central variables and conducted specification tests to test whether we have to correct

[28]Sala-i-Martin (1997) proposes using the integrated likelihood to construct a weighted CDF(0). However, missing observations for some of the variables poses a problem. Sturm and de Haan (2002) show that the goodness-of-fit measure may not be a good indicator of the probability that a model is the true model and that the weights constructed in this way are not invariant to linear transformations of the dependent variable. Hence, changing scales could result in different outcomes and conclusions. We therefore employ the unweighted version.

for country- and/or time-specific effects in our panel setup. As a result of these tests we include random country-effects in all equations.[29]

Table 4.4 depicts the results of the EBA.[30] The criterion for considering a variable to be robustly related to stringency is the CDF(0) value. Sala-i-Martin (1997) suggested considering a variable to be robust if the CDF(0) criterion is greater than 0.90. Instead we follow Sturm and de Haan's (2005) proposal to use a stricter threshold value of 0.95, due to the two-sided nature of the test.

Table 4.4: Results EBA – dependent variable: ln(stringency)

Variable	**Lower Bound**	**Upper Bound**	**%Sign.**	**Unwght. CDF(0)**	**Unwght. β**	**Std. Error**	**Impact Rank**
Base Model							
LGDPPC	-0.482	3.861	83.55	0.9878	1.513	0.445	3
Extended Model							
INDSHEMP	-0.189	0.023	95.43	0.9813	-0.079	0.034	4
LEFT	-1.398	0.150	93.14	0.9779	-0.626	0.285	1
URBAN	-0.193	0.035	74.29	0.9651	-0.069	0.035	2
INEQUAL	-0.029	0.130	70.86	0.9575	0.053	0.029	6
LPOPDENS	-1.115	3.579	34.29	0.9194	0.753	0.508	11
POLFREE	-0.292	0.583	33.14	0.8756	0.121	0.096	7
FDIGDP	-0.112	0.275	9.71	0.8625	0.077	0.065	8
GLOBAL	-1.685	2.893	40.57	0.8501	0.866	0.665	5
TRADE	-0.044	0.026	10.86	0.8066	-0.010	0.010	9
DICT	-1.272	1.157	14.29	0.7807	0.218	0.277	10
CORRUPT	-0.648	0.438	2.86	0.5707	-0.046	0.145	12

Notes: Results based on 231 (base model) and 175 (extended model) regressions, respectively, using country-specific random effects. '%Sign' refers to the percentage of the regressions in which the respective variable is significant at the 10% significance level. 'Impact Rank' lists the variables in descending order according to the impact of a one standard deviation shock. The standard deviation is calculated after de-meaning each variable to correct for country-specific effects. Variables are sorted according to the CDF(0) criterion.

[29]For readability, the results of the specification test of the EBA are not shown. (However, they are available from the authors upon request).

[30]Since there are substantial differences in the number of observations for each variable, which potentially could influence our results, we opt to restrict our sample based on our inequality measure and hence ensure a more homogeneous sample.

Turning to the results of the EBA we see that real GDP per capita (*LGDPPC*) is robustly and positively linked to the level of stringency.[31] This result is also found in the existing empirical literature. Therefore, to some extent it resolves the potential theoretical ambiguity which Congleton (1992) highlighted.[32]

We now turn to the extended model. Here each of the variables takes the role of the F vector once with the other 11 variables used in 175 combinations to test the robustness of this particular variable. The variable *LEFT*, representing a left-wing chief executive, is usually considered to be negatively related to stringency. A left-wing executive traditionally cares for the interest of industrial workers and might therefore be reluctant to increase environmental stringency. The share of the urbanized population (*URBAN*) has a negative relationship with stringency. Hence, citizens living in urban areas tolerate lower levels of environmental stringency.

From the viewpoint of our simple model, the most important findings of our analysis are the results for *INDSHEMP* and *INEQUAL*.[33] According to the EBA the former variable is robustly negatively related to the measure of stringency.[34] Therefore, just as our theory suggests, a declining blue-collar labor force is associated with diminished blue-collar political power and leads to more stringent environmental regulations. Our result for inequality is the more novel finding, however. Greater dispersion in incomes is associated with a more, and *not* less, stringent environmental policy. While this finding stands in stark contrast to previous research, it is consistent with our model. All other variables that are proposed in the literature, as being an influencing factor for the stringency of environmental agenda setting, clearly fail to meet the robustness criterion

[31] This result is based on 231 regressions.

[32] We also tested for a potential non-linear relationship by including the squared term into the model. However, this is not supported by the data.

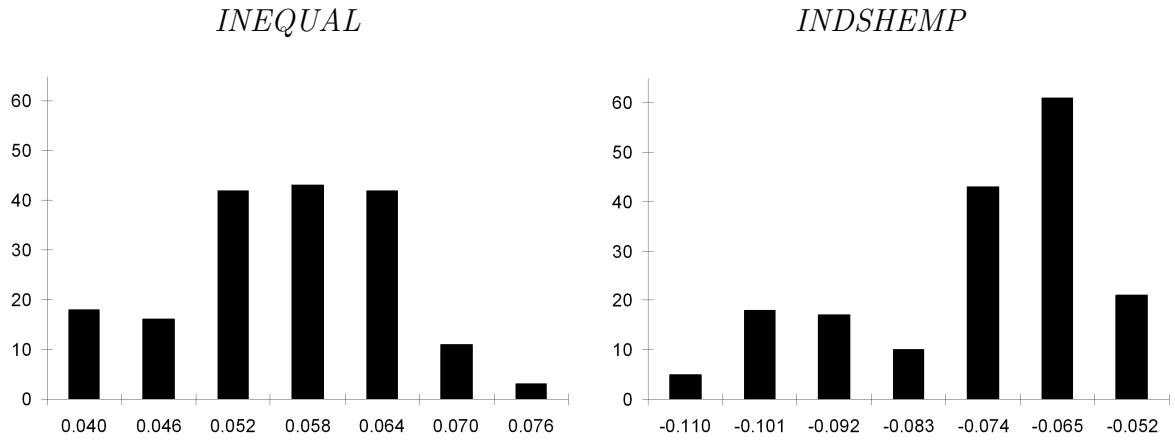
[33] Note, we also tested for potential non-linearities of both variables by adding a squared term. In both cases it turns out to be insignificant.

[34] We employ panel estimations which use the within and between variation of the variables in focus and thus estimate coefficients which describe how the dependent variable reacts to changes of the explanatory variables. Moreover, in our regression model the estimated coefficients represent semi-elasticities. Hence, a one unit change in the level of both variables implies a beta percentage change in the level of the dependent variable. As *INDSHEMP* is measured in percentages, a one percent increase in the percentage employed in the industry sector leads to a beta percentage change in the dependent variable.

of a CDF(0) value above 0.95. This is another major finding of the EBA, although obviously a rather negative one with respect to the extant literature.

In order to evaluate the relevance of the variables we also estimate the magnitude of the impact that all variables have on the policy stringency measure. We do this by calculating the effect that a shock of one standard deviation of each variable has on *LREGS*. We therefore multiply the average EBA coefficient with the standard deviation of the respective variable and rank them in descending order according to absolute value.[35] The resulting ranking is included in Table 4.4 in the column "Impact Rank." The five variables that exhibit a robust relation with the measure of stringency are among the six variables which have the biggest impact on the dependent variable. In addition we report the histograms of the coefficients of our two central variables. Figure 4.2 reveals that the estimated coefficients of the key variables are distributed close to their respective means and that there are no major outliers.

Figure 4.2: Histograms of the EBA coefficients



Notes: The frequency distributions summarize the coefficients of the 175 regressions of the EBA for the respective variable. The number beneath each bar indicates the upper bound of the bin.

Concerning the robustness of our results we use the five variables that the EBA suggests are robustly linked to environmental stringency and estimate our final model. The results are contained in Table 4.5.

[35]Since the estimation results include country-specific random effects we de-mean all variables. Failing to do so could seriously bias the results since the country-specific effects that were already taken into account would again contribute to the result.

Table 4.5: Results final model – dependent variable: ln(stringency)

	Random effects	**Fixed effects**
LGDPPC	3.652***	4.363***
	(0.519)	(0.629)
INDSHEMP	-0.098***	-0.132***
	(0.028)	(0.033)
LEFT	-0.713***	-0.756***
	(0.232)	(0.253)
URBAN	-0.107***	-0.080*
	(0.035)	(0.049)
INEQUAL	0.062***	0.063**
	(0.024)	(0.027)
Constant	-24.221***	-
	(3.472)	
Observations	50	50
R-squared	0.944	0.925
Hausman test	2.863	-
(H_0: random effects, H_1: fixed effects)		
F-test	4.573***	-
(significance of country-specific random effects)		
LR-test	-	107.7***
(H_0: pooled OLS, H_1: country-specific fixed effects)		

Notes: Both regressions contain country-specific effects. Standard errors are displayed in parentheses. */**/*** indicates significance at the 10/5/1%-level.

Based on purely statistical criteria, it is the preferred model. Again, the specification tests lead us to include country-specific random effects.[36] However, we also present the results when adding time-specific effects as a further robustness check.[37] Potentially worrisome is the relatively small number of observations (due to the list-wise deletion of missing observations on key variables). In order to test whether our results are driven by the small sample we linearly interpolate our inequality measure to create more observations. The results of these estimations are summarized in Table 4.6. Except for *URBAN*, in the case of country- and time-specific fixed effects all variables are robust to the estimation technique, the inclusion of time effects and the sample size, i.e., they are statistically significant at conventional levels. Overall, the results of the EBA are reinforced.

To summarize, there is obviously strong support for the argument forwarded in this chapter. Namely, that declining economic significance is associated with a decline in political significance. Both of these factors reinforce one another and lead to more stringent environmental regulation.

4.4 Conclusions

This chapter has emphasized that political and economic considerations interact to help explain the observed relationship between measures of economic development and environmental quality. Deindustrialization, falling real incomes of production workers and a greater dispersion of income are increasingly prominent features in many industrialized countries. From a political economy perspective, such features can also explain observed environmental policies.

When the social and economic consequences of either high unemployment or falling incomes in manufacturing industries are high, policy-makers may be tempted to ease environmental regulations. Symmetrically, as deindustrialization proceeds, as reflected by declining industrial employment and the falling wages and incomes for workers in basic

[36]For comparison, Table 4.5 also contains the results for country-specific fixed effects.

[37]We also estimated model specifications that included a time trend. There were no changes to the results.

Table 4.6: Results tests of robustness – dependent variable: ln(stringency)

	(a)	**(b)**	**(c)**	**(d)**
LGDPPC	3.724***	4.821**	3.405***	1.883***
	(0.533)	(2.141)	(0.479)	(0.466)
INDSHEMP	-0.100***	-0.128***	-0.093***	-0.065**
	(0.030)	(0.046)	(0.030)	(0.028)
LEFT	-0.702***	-0.970***	-0.378*	-0.478**
	(0.239)	(0.344)	(0.232)	(0.234)
URBAN	-0.106***	-0.116	-0.100***	-0.064**
	(0.036)	(0.076)	(0.037)	(0.029)
INEQUAL	0.063**	0.057*	-	-
	(0.025)	(0.031)		
INEQUAL-interpolated	-	-	0.060**	0.041*
			(0.029)	(0.026)
Constant	-24.872***	-	-0.378***	0.041***
	(3.595)		(0.232)	(0.026)
Observations	50	50	102	102
R-squared	0.941	0.913	0.921	0.934
Hausman test	0.841	-	2.965	0.891
(H_0: random effects, H_1: fixed effects)				
F-test	4.812***	-	12.960***	18.815***
(sign. of country-specific random effects)				
F-test	1.329	-	-	4.840***
(sign. of time-specific random effects)				
LR-test	-	120.0***	-	-
(H_0: pooled OLS, H_1: country and time-specific fixed effects)				

Notes: Columns (a) and (d) contain country- and time-specific random effects, (b) includes country- and time-specific fixed effects, and (c) incorporates country-specific random effects. Standard errors are displayed in parentheses. */**/*** indicates significance at the 10/5/1-level.

manufacturing and pollution-intensive industries, environmental stringency increases. That is, as these sectors of the economy become less important economically, they are also likely to carry less weight politically. Consequently, a regulator optimally increases the stringency of environmental regulations. The argument is simple and straightforward. Dynamic comparative advantages dictate that mature, developed economies shift resources away from basic manufacturing activities. Environmental policy simply reinforces this movement.

To some readers, the argument developed in this chapter may seem overly optimistic from the point of the view of the environment and overly cynical from a social equity perspective. The risk of being over-cynical is particularly acute for those who believe that a sense of social justice should prevail during times of rapid deindustrialization and falling blue-collar worker incomes. In turn, the social and political pressures may be thought to help override the demand for increased regulatory stringency. If this were in fact the case, it would be expected that environmental policies are least stringent in those industrialized and democratic countries in which income inequality is greatest. The evidence presented here is consistent with the exact opposite view. That is, countries with the strictest environmental standards tend to be those with the greatest dispersion in their incomes.

4.5 Appendix

Derivation of the Euler Equation

The first-order condition for the maximization of Bellman's Equation is

$$\theta^b \frac{\partial W_t^b}{\partial s_t} + \theta^w \frac{\partial W_t^w}{\partial s_t} + \beta V_{t+1}^{'}(p_{t+1}) = 0.$$

Rearranging and simplifying we have

$$\theta^b f_{s_t} \Delta_t + \beta V_{t+1}^{'}(p_{t+1}) = 0 \tag{4.8}$$

where $\Delta_t = U(y_t^{b,e}, p_t) - U(y_t^{b,u}, p_t)$. Differentiating the value function yields

$$V_t^{'}(p_t) = \theta^b \frac{\partial W_t^b}{\partial p_t} + \theta^w \frac{\partial W_t^w}{\partial p_t} + (1-\delta)\beta V_{t+1}^{'}(p_{t+1}).$$

After simplifying we have

$$V_t^{'}(p_t) = \theta^b l_t^b U_{p_t}^{b,e} + \theta^b (1-l_t^b) U_{p_t}^{b,u} + \theta^w U_{p_t}^w + (1-\delta)\beta V_{t+1}^{'}(p_{t+1}). \tag{4.9}$$

Substituting (A-2) into (A-1) yields

$$\theta^b f_{s_t} \Delta_t + (1-\delta)^{-1} \left(V_t^{'}(p_t) - \theta^b l_t^b U_{p_t}^{b,e} - \theta^b (1-l_t^b) U_{p_t}^{b,u} - \theta^w U_{p_t}^w \right) = 0,$$

or

$$V_t^{'}(p_t) = \left(\theta^b l_t^b U_{p_t}^{b,e} + \theta^b (1-l_t^b) U_{p_t}^{b,u} + \theta^w U_{p_t}^w \right) - (1-\delta)\theta^b f_{s_t} \Delta_t. \tag{4.10}$$

Substituting (4.10) into (4.8) yields the Euler equation (i.e., equation (4.4) in the text).

Chapter 5

Does Terror Threaten Human Rights? Evidence from Panel Data

5.1 Introduction

Governments' respect for human rights has opposing effects on national security. On the positive side, granting inalienable rights such as the freedom of speech, freedom of religion, the guarantee of impartial treatment in court, the protection from invasion of privacy and – even more importantly – the absence of torture, extrajudicial killings and political imprisonment enables citizens to live in freedom and safety, and to express potential political critique in non-violent form. But on the negative side, greater human rights increase a country's vulnerability to external and internal threats. Consequently, the net effect of human rights on national security is not obvious.

In Western democracies, the majority of leading politicians seems to endorse the negative view on the consequences of human rights, at least concerning the threat of fundamentalist religious terror. After terror attacks, politicians tend to suggest restricting human rights as a means to improve national security. U.S. President Bush, for

This chapter is an adapted version of Dreher et al. (2007).

instance, explicitly formulated objections against a legislation in 2005 which prohibits torture and inhumane treatment of detainees anywhere in the world, as this would hamper the ability of U.S. authorities to obtain information, especially in the "war against terror" (Amnesty International, 2006). Until the U.S. Supreme Court rendered this practice unconstitutional, prisoners in Guantánamo Bay, Cuba, were neither treated as prisoners of war nor as "ordinary" prisoners.[1] They were denied all basic human rights as well as treatment according to the Geneva Convention. Furthermore, as reported by Risen and Lichtblau (2005), President Bush authorized the National Security Agency to eavesdrop telephone and email communication between the U.S. and abroad without warrants starting in 2002, severely violating basic human rights.

In the U.K., the Prevention of Terrorism Act 2005 allows government ministers to issue control orders restricting the liberty, movement and activities of people purportedly suspected of terrorism-related activity. In Australia, similar legislations have recently been enacted. Thus, anecdotal evidence suggests that countries respond to terrorism and threat to their Western values by diminishing those very rights they wanted to protect in the first place.[2]

However, though anecdotal evidence abounds, systematic analysis is lacking.[3] The question whether terrorism systematically reduces human rights is yet unsettled. This is the question this chapter addresses. Specifically, we employ panel data for 111 countries over the period 1973–2002 to analyze whether and to what extent terror does – on average – affect human rights.

To anticipate our main results, we find that terror diminishes governments' respect for basic human rights such as the principle of absence of extrajudicial killings, political imprisonment, and torture. To some extent, civil rights are also restricted as a consequence of terrorism. Our basic results are extremely robust as to how we spec-

[1]Hamdan v. Rumsfeld, U.S. Supreme Court, June 29, 2006; 548 U.S.___, 126 S.Ct. 2749(2006).

[2]Restrictive anti-terror laws have been passed not only in Western societies. According to Amnesty International (2006), for instance, China and India have recently passed even stricter human rights-restricting anti-terrorism laws.

[3]This seems to be true for the literature on human rights in general. In the words of Kaufmann (2004, p.2) "the literature on human rights is overwhelmingly prose-rich and data-poor." For a recent exception see Dreher et al. (2006a).

ify our model, as extreme bounds analysis with almost 23,000 regressions shows. We find no effect of terror on empowerment rights, i.e., "positive rights," such as political participation and freedom of movement or religion.

We continue as follows. The next section presents our basic theoretical model. We introduce our hypotheses in section 5.3 and our measures of human rights and terror in section 5.4. We proceed by explaining our method of estimation. Section 5.6 presents the results, while we test the robustness of these results in section 5.7. The final section concludes.

5.2 Basic Model

Let there be a finite number of citizens i. Every citizen i has the same basic preferences over the publicly provided goods "security," labeled s_t, and "human rights," labeled h_t, which is given by:

$$u_t^i = u^i\left(s_t, h_t\right) \tag{5.1}$$

We assume positive but decreasing marginal utility in both goods. Individuals face a constant trade-off between security and human rights respect, which is given by $s^i(h_t)$, with $\partial s^i(h_t)/\partial h_t < 0$. The importance of security is determined by the true probability of a terror attack. However, as citizens do not know the true probability of a terror attack, they are forced to derive subjective probabilities, labeled $p_t^i = P^i\left(terror_t\right)$, which gives the expected probability of a terror attack in period t of citizen i. Therefore, citizens differ in their subjective expectations about the probability of a terror attack and by the perceived trade-off between security and human rights. The assumed preferences can, for instance, be expressed by:

$$u_t^i = \left[s^i\left(h_t\right)\right]^{p_t^i} \cdot h_t^{1-p_t^i} \tag{5.2}$$

That is, the subjective probability functions as the utility elasticity of security, and the counter-probability as the utility elasticity of human rights respect. The optimal level of human rights, labeled h_t^i, is then determined by:

$$\eta_{s^i(h_t),h_t}(h_t) \equiv -\frac{\partial s^i(h_t)}{\partial h_t}\frac{h_t}{s^i(h_t)} = \frac{1-P^i(terror_t)}{P^i(terror_t)} \tag{5.3}$$

That is, if the subjective probability of a terror attack is very high, the citizen will demand much security relative to the level of human rights, respectively, low human rights respect relative to security. Moreover, the more human rights are perceived to reduce security, the less human rights are demanded. Hence, citizens are willing to pay a prize in terms of lower human rights to enjoy a lower threat of terror.[4] Thus, the optimal combination of human rights and security of a citizen is determined by the individual belief concerning the trade-off between security and human rights, and by the subjective probability of a terror attack.

Having described the behavior of the citizens, we now turn to the behavior of governments. Similar to the median voter model (Black, 1948; Downs, 1957; Persson and Tabellini, 2000, chap. 3), we assume that governments want to remain in power. In order to achieve this goal, a country's government has to win support of a particular fraction $\pi \in (0,1]$ of society. We assume that providing human rights involves costs to the government, so that governments have no incentive to provide more human rights than the level required ensuring the support of fraction π of society. Given our assumptions, preferences are single-peaked, so that, applying the logic of the median voter theorem to our issue, there exists a pivotal citizen, whose bliss point represents a platform that cannot lose against any other competing platform (the proof in Mueller, 2003, p.86, directly applies to our generalized model). The equilibrium level of human rights, denoted by h_t^*, is determined by the subjective probability of a terror attack of the pivotal citizen, i.e. the citizen who completes the critical mass of citizens, π.[5]

[4]Of course, not all types of human rights are likely to reduce national security, some may even improve it. However, considering the aggregate level of governments' respect for human rights, our assumption appears plausible.

[5]Suppose, for instance, we consider a democracy ruled by majority voting, that is, $\pi = 1/2$. Then the pivotal citizen is equivalent to the median voter.

In nondemocratic regimes, the governments compete with the opposition for the support of the citizens, as it is threatened by potential revolution and uprisings (Acemoglu and Robinson, 2001). In order to stay in power, it has to please a critical mass of the society, which can be any fraction $\pi \in (0, 1]$. Consider, for instance, an autocratic regime requires the support of a fraction $\pi = 1/3$ to remain in power, because otherwise the opposition would be able to organize a successful uprising. As an example, let there be three citizens. As the government of an autocracy wants to stay in power, it will ensure the support of the one citizen with the lowest demand h_t^i, so that the opposition cannot organize a successful uprising, no matter which platform it chooses. Hence, the Nash, respectively the political, equilibrium is described by the government choosing the lowest demand for human rights of the three citizens. The median voter theorem for democracies governed by majority voting is a sub-case of our general model. In the median-voter model, we have $\pi = 1/2$ and the government competes with the opposition in electoral competition.[6] The Nash, respectively political, equilibrium of the model is then the median human rights demand h_t^{median}. Therefore, we generalize the median voter model and state that, irrespective of regime type and decision rule, there exists a pivotal citizen whose bliss point represents the dominant platform, that is, the political equilibrium.

Having described the political equilibrium of our basic model, we now turn to the issue how terror attacks affect this equilibrium outcome. Obviously the subjective probabilities of terror attacks are updated when terror attacks occur in period t, because such events represent signals and new evidence that have to be accounted for. We assume that individuals update their subjective probability applying Bayesian inference. That is, citizens who observe terror attacks in period t update, respectively newly infer, their subjective probability of terror attacks in period $t + 1$, given the new evidence that a terror attack occurred in period t. Hence, the newly inferred subjective probability is given by $p_{t+1}^i = P^i\left(terror_{t+1} \left| terror_t\right.\right) \in [0, 1]$.

[6] Applied to the median-voter model, there are (at least) two competing parties: one party prefers a low level of human rights and higher human rights involve costs, and another party, which prefers high levels of human rights and lower levels of human rights involve costs.

It is clear that if the subjective probabilities of all citizens remain unchanged, the same is true for the political equilibrium level of human rights, and terror attacks do not affect the political equilibrium. However, if some or all subjective probabilities are changed, the political equilibrium level of human rights is likely to change as well, depending on the bliss point of the pivotal citizen after Bayesian updating took place. If the updated subjective probability of the new pivotal citizen is increasing, she/he evaluates security relative to the level of human rights higher, and consequently demands a lower level of human rights: $h_{t+1}^{pivotal}$ decreases. Thus, in response to terror attacks, the citizens of society endogenously change their behavior and claim for more safety at the expense of lower human rights. Politicians in democracies and autocracies alike compete with each other and follow this shift of perception of the citizens, so that the governments will try to increase security by eroding human rights in equilibrium.

Applied to U.S. policy in the aftermath of September 11, 2001 (9/11), for instance, U.S. citizens were shocked by the attacks. They significantly increased their expected probability of a terror attack in the U.S. and thus gave security higher weight in their preferences. Consequently, as their security perception was basically extinguished, they were willing to accept, respectively demanded, a lower level of human rights to increase security. In polls, president Bush received approval rates of 60 to 90 percent when his administration "engineered the biggest expansion in executive power since the days of Franklin Roosevelt" with the Patriot Act, comprising, e.g., the right of monitoring telephone calls without explicit approval from the courts, reading private e-mails, prying into library records and establishing military tribunals (The Economist, 2006a, pp.20-21). The threat of terror led to a situation in which national security became the major political topic in elections. Similar developments can be observed all over the Western World – though only few countries, such as Great Britain, acted as strictly as the U.S. Furthermore, this pattern of behavior already existed prior to 9/11: after the 1993 bombings of the World Trade Center and the federal building in Oklahoma City the Antiterrorism Act of 1996 was passed. Cole and Dempsey (2002, p.117) rank the Act as "some of the worst assaults on civil liberties [prior to the 2001 antiterrorism measures]."

However, as p_t^i represents the utility elasticity of individual i referring to security and $1-p_t^i$ the one referring to the level of human rights, citizens could also update their subjective preference parameter mixture between security and human rights respect in the other direction, namely such that they evaluate human rights respect in the aftermath of a terror attack higher than before, for disrespect of human rights can provoke terror attacks and thus reduce security. A potential example is the human rights disaster at Abu Ghraib, which may have increased the importance of human rights in the preferences of citizens. Considering the threat of terror in the context of the Basque or Northern Ireland conflict, where the Spanish and British governments eventually decided to improve the human rights situation after a period of confrontation, because they – or rather the majority of voters – realized that what occurred was a vicious circle of violence. That is, it is also possible that the pivotal citizen, here the median voter, believes that stricter human rights increase security, so that the importance of the good "security" in the preference function is reduced. The Palestinian issue saw both developments, human rights improvements and more frequent human rights violations. In our model, both developments can occur. Which reaction is more relevant in practice has to be tested by empirical analysis in the next sections.

Note that the trade-off relevant for political outcome is the subjectively perceived link between human rights and terror, and not necessarily the "actual" trade-off between respect for human rights and terrorism.[7] In the U.S., for instance, not a single terrorist was found by rounding up hundreds of foreigners, most of them Muslims, after 9/11 and holding them without charge, sometimes for months (The Economist, 2006b). Hence, citizens may have adaptive expectations and learn that at least some anti-terrorist measures do not improve national security.[8] In the next section, we explicitly formulate the two major hypotheses derived from our model, which we are going to confront with data further below.

[7]Empirical research suggests an inverted U-shape relationship between political rights and terrorism (Eubank and Weinberg, 2001; Abadie, 2006).

[8]However, recent polls in Britain and the US suggest that most voters still feel their governments are not doing enough to counter terrorism (The Economist, 2006b).

5.3 Hypotheses

Overall, it seems reasonable to expect that the relationship between terrorism and human rights is negative. Governments seem to react to an increase in terrorism by constraining the freedom and privileges of their citizens, and our model suggests that this reaction may be based on the demand for security of the majority of citizens. Stricter surveillance and control makes it harder to exchange secret information and (radical) ideas. As stricter controls impair terrorists' ability to plan attacks, the public is more willing to accept them in times of increased threat. Citizens may correct their subjective probabilities of a terror attack in their country upwards when the country has experienced an attack. Consequently, they are more willing to accept a decrease in the level of human rights in order to enhance security. Moreover, terrorists might take successful attacks as an indicator of the government's weakness, in turn improving their confidence to be able to effectively threaten or even overthrow the government. Consequently, the government has an incentive to decrease human rights respect in order to be better able to monitor terrorist activity. The illustrative example of President Bush initially objecting to a bill prohibiting the torture and inhumane treatment of detainees anywhere in the world because it would negatively affect the possibility of obtaining information (Amnesty International, 2006) demonstrates that even fundamental human rights and values are put into perspective if they are expected to reduce national security. Based on this reasoning we expect:

Hypothesis 5.1. *An increase in the level of terrorist activity leads to a reduction in the level of human rights.*

Although this reasoning appears straight forward, one can also argue, however, in the opposite direction. In our model, it is equally possible that the pivotal citizen believes the increase in human rights to improve national security. Terror attacks might be perceived as reactions to bad human rights situations (all over the world).[9] Hence, citizens increase their evaluation of human rights respect in their preferences. Governments thus might give in to terrorism that pretendedly aims at improving the human

[9]For a discussion of unilateral vs. multilateral actions of terrorists and states see Sandler (2005).

rights situation. Of course, giving in to kidnapping and other forms of blackmailing may be time-inconsistent, as one becomes vulnerable to further attempts. However, states did give in to kidnapping events in the past. In October 1972, for instance, Germany released the three Palestine survivors of the terror attacks at the Olympic Games in Munich after an airplane had been hijacked. In March 1975, Germany released five prisoners from the terror scene after the politician Peter Lorenz had been kidnapped. Although the German government does not give in to political demands anymore, it is still willing to pay money in exchange for the release of kidnapped citizens – and other countries do the same. Therefore, the probability of states giving in to political demands is greater than zero. Consequently, Krueger and Malečková (2003) find, analyzing a survey conducted by the Palestinian Center for Policy Research in 2001, that a majority of Palestinians living in the West Banks and Gaza Strip believe that they would not have achieved their current status by negotiations alone. Our examples regarding the Basque and Northern Ireland conflict also suggest that terror is a means that might, eventually, improve human rights levels at particular places. In our model, it is equally well possible that the pivotal citizen believes that human rights respect improves national security. Hence, we state the counter-hypothesis:

Hypothesis 5.2. *An increase in terrorist activity leads to an increase in the level of human rights.*

Obviously, Hypotheses 5.1 and 5.2 might to some extent be valid at the same time, especially taking into account that every terrorism problem has its own specific origin. While we cannot empirically distinguish between them, what we can do is estimating which of those effects dominates. In other words, what we estimate is the average net effect potentially resulting from two opposing effects. This is what we turn to below.

5.4 Measuring Human Rights and Terror

Our definition of citizens' human rights follows Cingranelli and Richards (1999). Their Human Rights Dataset (CIRI, Cingranelli and Richards, 2006) provides quantitative

information on government respect for 13 internationally recognized human rights, on an annual basis and for almost all countries of the world. Cingranelli and Richards focus on actual human rights-related actions of governments, including all government agents such as police and military. Their database draws from two sources, the U.S. Department of State's Country Reports on Human Rights Practices, and from the Amnesty International Report, offering detailed descriptions of human rights practices.

In particular, the CIRI data refer to extrajudicial killings, people who have disappeared for political reasons, torture, political imprisonment, freedom of speech, freedom of religion, freedom of movement, freedom of assembly and association, political participation, worker's rights, women's political rights, women's economic rights, and women's social rights. Each variable is coded on an ordinal scale, ranging between 0-2 and, depending on the variable considered, 0-4, where higher values reflect better ratings in the respective human rights dimension.

In this chapter we mainly focus on two composite indicators proposed in Cingranelli and Richards (1999) and Richards, Gelleny and Sacko (2001). The first composite index refers to physical integrity rights – it is the additive of absence of torture, extrajudicial killings, political imprisonments, and disappearance, on a scale from 0-8 (so-called "negative rights"). The second composite refers to empowerment rights and comprises the freedom of movements, freedom of speech, workers' rights, political participation, and freedom of religion indicators, ranging from 0-10 ("positive rights").

The upper line in Figure 5.1 shows the time path of the unweighted average of the physical integrity index for the OECD countries over the years 1981-2004. The lower line represents the average for low income countries,[10] while the unweighted world average is shown in the middle. The number of countries covered rises from 125 in 1981 to 179 in 2004. The figure shows that the mean of the world index was fairly constant over time at a value of around 5 with, however, a substantial drop around 1990. The average for the OECD countries is also rather constant over time, at a higher level of around 7. Figure 5.1 shows that there is some variation in physical integrity for developing

[10]Countries with low income are those with 2004 GNI per capita of a maximum of 825 US$, according to the definition of the World Bank (2006).

countries, with a substantial negative trend. The level of physical integrity peaked in 1981 at 5, fell to a low point of 3.2 in 1990, rose to 4 in 1995, and after declining again until 2000 is slightly below 4 in 2004.

Figure 5.1: Development of physical integrity rights over time

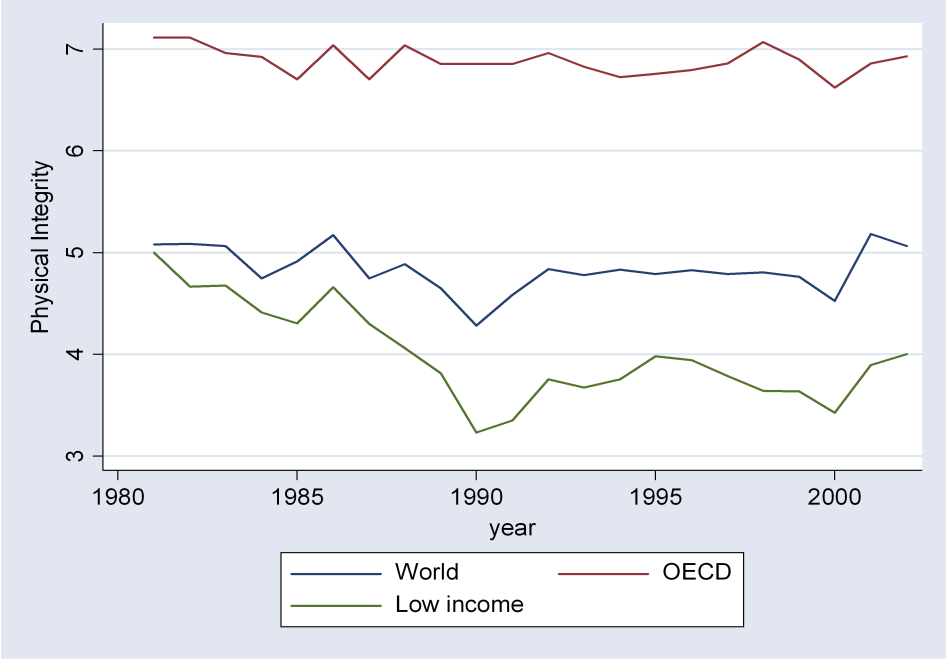
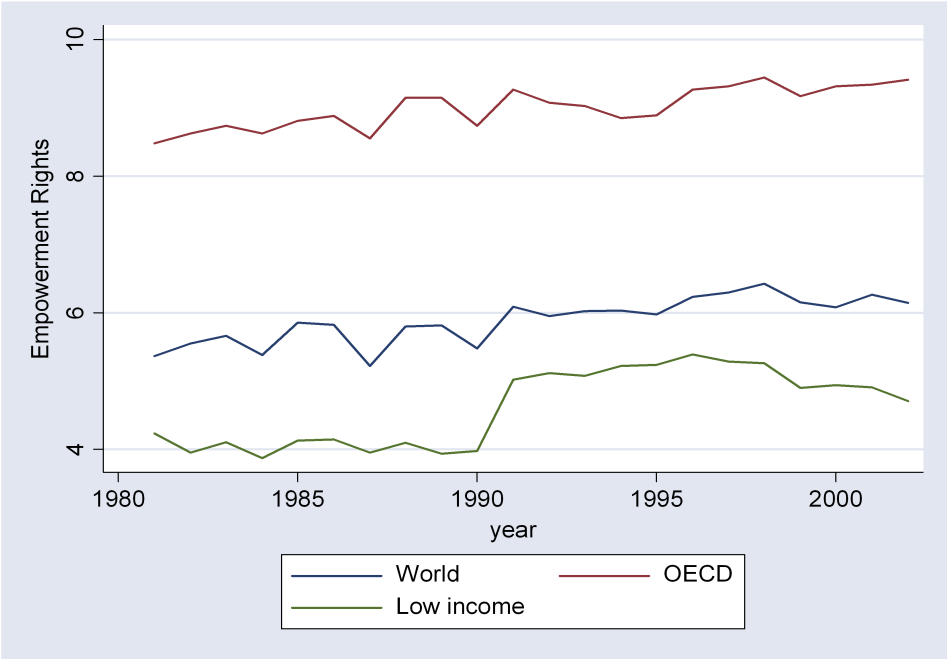


Figure 5.2 shows the development of empowerment rights. As can be seen, the average level of empowerment rose steadily over the period of observation, with similar developments in developing and OECD countries. The most substantial increase in empowerment was experienced in 1990, in particular in low income countries.[11] The index mean is 4.6 for low income countries, more than 9 in OECD countries, and 5.9 for the world sample. Since 1996 we observe a negative trend in low-income countries. The world sample contains 130 countries in 1981, and 181 in 2004.

As a third proxy for human rights we employ the civil liberties indicator provided by Freedom House (2006). The survey based index combines four subcategories: freedom of expression and belief, associational and organizational rights, rule of law, and personal and individual rights. The rule of law subcategory measures whether equal treatment,

[11]The apparent increase in low income countries from 1990 to 1991 is mainly driven by Mali, The Democratic Republic of Congo and Togo (increases of 4, 5 and 9 points, respectively).

Figure 5.2: Development of empowerment rights over time



police control and absence of torture prevails. To some extent, the index thus combines empowerment and physical integrity rights.[12] We rescale the original index so that a value of seven indicates greatest freedom while one is the lowest score.

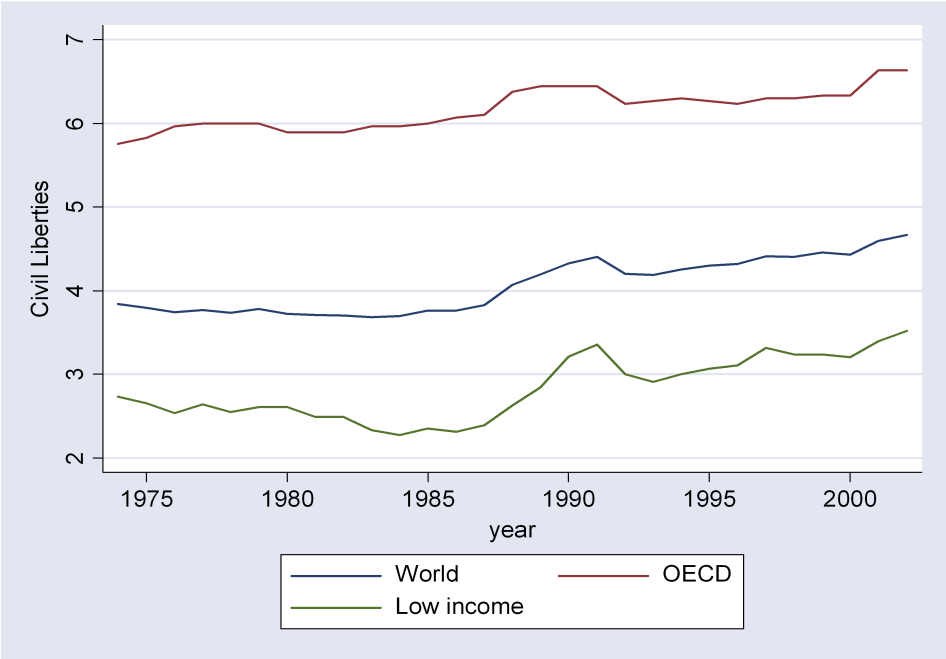
Figure 5.3 shows the development of civil liberties over time. Clearly, all three groups of countries display a positive trend. In the early 1990ies, the index peaks, in particular in low-income countries. In these countries, the increase has been particularly pronounced over the years 1987 to 1992, with an index score of roughly 4.5. In 1993, in all three groups the civil rights index returned to its longer term trend.

Turning to our measure of terrorist activity, we employ data provided in the MIPT Terrorism Knowledge Base.[13] The Terrorism Knowledge Base integrates data from the RAND Terrorism Chronology and RAND-MIPT Terrorism Incident databases, the

[12]The correlation between physical integrity and empowerment is 0.51, while it is 0.59 between integrity and civil liberties, and 0.82 between empowerment and civil liberties.

[13]Available at: http://www.tkb.org/.

Figure 5.3: Development of civil liberties over time



Terrorism Indictment database, and DFI International's research on terrorist organizations.[14]

The Terrorism Knowledge Base defines terror as "violence, or the threat of violence, calculated to create an atmosphere of fear and alarm." The focus of terror is to discourage opposition from acting at their free will. The motives for engaging in terror are political while the acts themselves are generally conducted in a way that will achieve maximum publicity. Moreover, terrorist acts are mostly intended to create more than immediate physical damage – a long-time situation of fear and intimidation. For an extensive summary on the various effects of terrorism see Frey et al. (2007).

We extract the number of terror events for each country and year as our variable of main interest.[15] We also employ the number of suicide attacks and the number of people killed in a certain year and country. Suicide attacks are likely to be perceived as particularly threatening. They might be more effective from the terrorists' point of view

[14]There are also different sources for terrorism data. We choose MIPT because it combines various sources. For a detailed discussion on measurement of terrorism we refer to Frey and Luechinger (2005).

[15]Territories are assigned to the country formally governing the territory. Kashmir and the Persian Gulf are excluded as it is not obvious to which country they should be assigned to.

as it is hard to fight someone who is explicitly willing to sacrifice the own life. Suicide attacks might thus provoke particularly harsh reactions by the government concerned.[16] The same is likely to be true when the attacks are more severe, i.e., when more people are killed. Given that the database covers the whole world, we assign zeros to all countries and years without data.

Figure 5.4: Development of terror over time

Figure 5.4 shows how terror has evolved over time. OECD countries experience the highest amount of terror while low income countries encounter the lowest value.[17] Particularly noteworthy is the rise across all country groups in the recent past.[18]

[16]For a summary of studies on suicide bombing see, e.g., Sandler (2003).

[17]For time-series studies on the occurrence and distribution of terrorism see Enders and Sandler (2005, 2006).

[18]The increase is driven in part by the explicit inclusion of domestic terror in the dataset from 1998 onward. We discuss this issue in section 5.7.

5.5 Empirical Method

We estimate pooled time-series cross-section (panel data) regressions. The data extend to a maximum of 111 countries and cover the years 1973-2002. Since some of the data are not available for all countries or years, the panel data are unbalanced and the number of observations depends on the choice of explanatory variables.

To test our hypotheses we estimate equations of the following form:

$$RIGHTS_{i,t} = \alpha RIGHTS_{i,t-1} + \beta Terror_{i,t} + \gamma Z_{i,t-1} + u_{i,t}, \tag{5.4}$$

where $RIGHTS_{i,t}$ represents our measure of human rights, and $Terror_{i,t}$ is the respective measure of terrorist attacks in country i at year t (i.e., the number of events, the number of suicide attacks and, respectively, the number of people killed). Z is a vector of control variables as introduced below. Note that we also include the lagged dependent variable, as human rights develop only slowly over time and the lagged dependent variable turns out to be highly significant (see, e.g., Dreher et al. 2006a). Following the previous literature, we estimate our model employing ordered probit, with clustering at the country level and robust standard errors. All regressions include dummies for each year, which are highly significant.

The ordered probit model is related to the multinomial probit model but takes into account the ordinal nature of the dependent variable. Just as in the binomial probit model, a latent dependent variable is assumed to underlie the empirical setup with a normally distributed error term. The cutoff points for the different categories are estimated with the coefficients of the model.

In selecting our control variables, we follow the robustness analysis in Dreher et al. (2006a). Based on a general-to-specific approach and extensive robustness tests employing extreme bounds analysis, Dreher et al. suggest the following variables as robust predictors of a country's level of physical integrity rights: the logarithm of a countries' population, its degree of democracy, dummies for income, and dummies for legal origin.

Our measure of democracy is from the Polity IV database as provided by Gurr et al. (2003). It represents the difference between a country's democracy and autocracy score running from -10 to 10 with higher numbers indicating higher levels of democracy.[19] Population is taken from the World Bank's (2006) World Development Indicators, legal origin follows Easterly and Sewadeh (2001), while the income classification is according to the World Bank (2006). For a complete description of the variables used and their sources we refer to Table 5.1 while the descriptive statistics are shown in Table 5.2. The next section reports the results.

5.6 Results

Column 1 of Table 5.3 replicates the baseline model of Dreher et al. (2006a) for the physical integrity index. As can be seen, the quantitative variables are significant at the one percent level. The sets of dummy variables indicating income and, respectively, legal origin, are both jointly significant at the one percent level. According to the results, government respect for human rights is higher with smaller population and greater democracy. The former finding is consistent with the notion that lower surveillance costs, e.g., because of a smaller population, allow the government to allocate more human rights. Moreover, higher levels of democracy reduce the government's possibility to increase security by restraining rights.

Relative to Scandinavian legal origin – the omitted variable – respect for human rights is smaller among all other categories – it is lowest in countries with German legal origin. As compared to low income countries, respect for human rights is greater in countries with upper middle income and in high income OECD countries.

Columns 2 and 3 report the same specification for the other two dependent variables, generally confirming the previous results. The exception is the dummies for legal origin which are not jointly significant at conventional levels.

[19]The index is based on competitiveness and openness of executive recruitment, competitiveness and regulation of political participation, and constraints on chief executives.

Table 5.1: Variables – definitions and sources

Variable	Description	Source
Physical integrity	Additive of torture, extrajudicial killings, political imprisonments, and disappearance, (0 = no rights; 8 = full rights).	Cingranelli and Richards (2006)
Empowerment index	Additive of freedom of movements, freedom of speech, workers rights, political participation, and freedom of religion indicators, (0 = no rights; 10 = full rights).	Cingranelli and Richards (2006)
Civil liberties	Index combining (a) Freedom of Expression and Belief, (b) Associational and Organizational Rights, (c) Rule of Law, and (d) Personal and Individual Rights (1= no rights; 7 = full rights).	Freedom House (2006)
Imprisonment	Absence of political imprisonments (0 = no rights; 2 = full rights).	Cingranelli and Richards (2006)
Torture	Absence of tortures (0 = no rights; 2 = full rights).	Cingranelli and Richards (2006)
Disappearences	Absence of disappearances (0 = no rights; 2 = full rights).	Cingranelli and Richards (2006)
Kills	Absence of extrajudicial killings (0 = no rights; 2 = full rights).	Cingranelli and Richards (2006)
Terror events	Number of terror events in respective country and year.	MIPT Terrorism Knowledge Base
Suicide attacks	Number of suicide attacks in respective country and year.	MIPT Terrorism Knowledge Base
Terror fatalities	Number of people killed in respective country and year.	MIPT Terrorism Knowledge Base
Terror events, dummy	Taking on the value 1 if at least one terror event occurred in respective country and year.	MIPT Terrorism Knowledge Base
Severe terror events	Number of terror events in which at least one person was killed or injured.	MIPT Terrorism Knowledge Base
Domestic terror events	Number of terror events in which both the attacker and the target were domestic.	MIPT Terrorism Knowledge Base
International terror events	Number of terror events in which either the attacker or the target was from abroad.	MIPT Terrorism Knowledge Base
Democracy	Polity IV democracy indicator (-10 = low; 10 = high).	Gurr et al. (2003)
Population (log)	ln (total population).	World Bank (2006)
Income, dummies	The groups are: *low* $825 or less; *lower middle*, $826-3,255; *upper middle*, $3,256-10,065; and *high (OECD)*, $10,066 or more.	World Bank (2006)
Legal origin, dummies	Dummies for British, French, Socialist, and German legal origin.	Easterly and Sewadeh (2001)

Table 5.2: Variables – descriptive statistics

Variable	**Mean**	**Minimum**	**Maximum**	**Std. Dev.**
Physical integrity	4.86	0	8	2.37
Empowerment index	5.88	0	10	3.28
Civil liberties	4.10	1	7	1.95
Imprisonment	1.09	0	2	0.85
Torture	0.80	0	2	0.75
Disappearences	1.65	0	2	0.65
Terror events	2.68	0	858	18.44
Suicide attacks	0.05	0	72	1.23
Terror fatalities	3.49	0	2987	50.32
Terror events, dummy	0.27	0	1	0.44
Severe terror events	0.97	0	551	8.68
Domestic terror events	1.39	0	611	15.09
International terror events	1.29	0	247	5.62
Kills	1.32	0	2	0.78
Democracy	0.22	-10	10	7.58
Population size (log)	15.15	9.89	20.98	2.10
Lower middle income	0.26	0	1	0.44
Higher middle income	0.19	0	1	0.39
High income OECD	0.12	0	1	0.32
High income	0.15	0	1	0.36
British legal origin	0.34	0	1	0.47
French legal origin, dummy	0.48	0	1	0.50
Socialist legal origin, dummy	0.10	0	1	0.30
German legal origin, dummy	0.04	0	1	0.20

Columns 4 to 6 add the number of terror events in a particular year and country. As the results show, physical integrity rights are restricted as a consequence of terror, strongly supporting our Hypothesis 5.1, and the same is true for civil rights measured by the Freedom House index. The former coefficient is significant at the one percent level, while the latter is significant at the five percent level. Empowerment rights do not seem to be affected by the number of terror events.

Table 5.4 replicates the analysis substituting the number of terror events by the number of suicide attacks and, respectively, the number of people killed in a certain country and year. Again, the results show some impact of terror on human rights. At the one percent level of significance, suicide attacks restrict human rights as measured by the physical integrity index (column 1) and the civil liberties index (column 3). Empowerment rights, to the contrary, are not significantly affected by suicide attacks (column 2). Turning to the number of people killed, there is a highly significant (and negative) impact on physical integrity rights (column 4), while empowerment rights and civil liberties are not significantly affected.

As our dependent variables are ordinal, quantitative interpretation of these results is not straightforward. One way of evaluating the magnitude of the estimated effect is to compare it to the estimated cutoff points. The estimated coefficients allow the calculation of the latent human rights variable. A shift in the observed index value will occur if the corresponding (estimated) cutoff point for the respective category will be exceeded. The smallest difference between two cutoff points in our case is 0.66, representing the difference for the upper and lower limit of an observed index value of 2. To quantify the impact of terror attacks, assume a shock of one standard deviation (i.e., 18.44, see Table 5.2). This would change the latent variable by $18.44 \cdot (-0.003) = -0.055$, ceteris paribus. This value represents roughly $1/10^{th}$ of the value needed to induce a change in the observed index score. To get a better grasp for the magnitude of the effect of terror, we calculate the marginal effects for the significant coefficients of the terror variables of Tables 5.3 and 5.4 (at the mean of all independent variables), as shown in Table 5.5. Due to the ordinal setup, the calculation and interpretation of the marginal

Table 5.3: Results terror events – dependent variable: human rights

	(1) PI	(2) ER	(3) CL	(4) PI	(5) ER	(6) CL
Lagged dependent	0.540	0.538	1.827	0.536	0.538	1.825
	(23.67)***	(20.54)***	(17.38)***	(23.44)***	(20.56)***	(17.33)***
Income lower middle	-0.034	-0.067	0.094	-0.033	-0.067	0.097
	(0.42)	(0.83)	(1.37)	(0.39)	(0.83)	(1.42)
Income upper middle	0.249	0.016	0.296	0.256	0.016	0.300
	(2.32)**	(0.15)	(2.99)***	(2.36)**	(0.15)	(3.03)***
Income high OECD	1.102	0.580	1.398	1.134	0.580	1.419
	(6.68)***	(2.74)***	(7.80)***	(6.91)***	(2.75)***	(7.96)***
Income high	0.031	-0.377	-0.017	0.078	-0.377	0.007
	(0.20)	(3.47)***	(0.15)	(0.56)	(3.37)***	(0.06)
Legal origin British	-0.909	-0.359	-0.285	-0.887	-0.359	-0.270
	(4.80)***	(1.04)	(0.73)	(4.70)***	(1.04)	(0.69)
Legal origin French	-1.009	-0.316	-0.400	-0.979	-0.316	-0.379
	(5.04)***	(0.90)	(1.03)	(4.92)***	(0.90)	(0.97)
Legal origin Socialist	-0.704	-0.464	-0.413	-0.682	-0.464	-0.402
	(3.08)***	(1.22)	(1.01)	(3.00)***	(1.22)	(0.98)
Legal origin German	-1.135	-0.490	-0.572	-1.141	-0.490	-0.571
	(3.71)***	(1.22)	(1.33)	(3.71)***	(1.22)	(1.32)
Population (log)	-0.202	-0.101	-0.080	-0.198	-0.101	-0.076
	(9.16)***	(4.04)***	(4.37)***	(8.99)***	(3.98)***	(4.18)***
Democracy	0.016	0.076	0.045	0.018	0.076	0.046
	(2.79)***	(10.23)***	(5.27)***	(3.09)***	(10.22)***	(5.34)***
Terror events				-0.003	-0.000	-0.002
				(4.73)***	(0.05)	(2.43)**
Observations	2,217	2,219	3,218	2,217	2,219	3,218
Countries	111	111	111	111	111	111
Years	21	21	31	21	21	31
Pseudo R^2	0.31	0.37	0.65	0.31	0.37	0.65

Notes: PI refers to the physical integrity rights indicator, ER to the empowerment rights indicator and CL to the civil liberties indicator. The former two are taken from Cingranelli and Richards (2006) while the latter stems from Freedom House (2006). The results are derived by running ordered probit regressions with clustering at the country level and including annual time dummies.
*/**/*** indicates significance at the 10/5/1-% level; robust absolute z-statistics are given in paranthe-ses.

Table 5.4: Results alternative terror measures – dependent variable: human rights

	(1) **PI**	**(2)** **ER**	**(3)** **CL**	**(4)** **PI**	**(5)** **ER**	**(6)** **CL**
Lagged dependent	0.539	0.538	1.826	0.540	0.538	1.827
	(23.63)***	(20.55)***	(17.37)***	(23.57)***	(20.54)***	(17.37)***
Income lower middle	-0.036	-0.067	0.094	-0.033	-0.067	0.094
	(0.44)	(0.83)	(1.37)	(0.40)	(0.83)	(1.38)
Income upper middle	0.249	0.016	0.296	0.252	0.016	0.296
	(2.31)**	(0.15)	(2.99)***	(2.33)**	(0.15)	(2.99)***
Income high OECD	1.102	0.581	1.398	1.113	0.579	1.399
	(6.66)***	(2.75)***	(7.80)***	(6.66)***	(2.74)***	(7.81)***
Income high	0.057	-0.382	-0.010	0.039	-0.375	-0.015
	(0.39)	(3.41)***	(0.08)	(0.26)	(3.41)***	(0.13)
Legal origin British	-0.910	-0.359	-0.285	-0.899	-0.359	-0.284
	(4.79)***	(1.04)	(0.73)	(4.71)***	(1.04)	(0.73)
Legal origin French	-1.010	-0.316	-0.401	-1.004	-0.317	-0.399
	(5.04)***	(0.90)	(1.03)	(4.98)***	(0.90)	(1.03)
Legal origin Socialist	-0.705	-0.464	-0.414	-0.700	-0.465	-0.413
	(3.07)***	(1.22)	(1.01)	(3.05)***	(1.22)	(1.01)
Legal origin German	-1.137	-0.490	-0.573	-1.141	-0.491	-0.573
	(3.71)***	(1.22)	(1.33)	(3.72)***	(1.22)	(1.33)
Population (log)	-0.202	-0.101	-0.080	-0.199	-0.100	-0.079
	(9.10)***	(4.04)***	(4.35)***	(9.01)***	(3.96)***	(4.37)***
Democracy	0.017	0.076	0.046	0.017	0.076	0.045
	(2.92)***	(10.21)***	(5.27)***	(2.84)***	(10.26)***	(5.27)***
Suicide attacks	-0.031	0.006	-0.013			
	(3.31)***	(0.73)	(3.35)***			
Terror fatalities				-0.001	-0.000	-0.000
				(4.47)***	(0.32)	(0.74)
Observations	2,217	2,219	3,218	2,217	2,219	3,218
Countries	111	111	111	111	111	111
Years	21	21	31	21	21	31
Pseudo R^2	0.31	0.37	0.65	0.31	0.37	0.65

Notes: PI refers to the physical integrity rights indicator, ER to the empowerment rights indicator and CL to the civil liberties indicator. The former two are taken from Cingranelli and Richards (2006) while the latter stems from Freedom House (2006). The results are derived by running ordered probit regressions with clustering at the country level and including annual time dummies.
*/**/*** indicates significance at the 10/5/1-% level; robust absolute z-statistics are given in parantheses.

effects is not straight forward.[20] Note that because of the assumed normal distribution the sign of the marginal effect changes. For all index values below the mean the marginal effect will have the opposite sign as the estimated coefficient, while all values above will have the same signs. Only for the mean value itself the effect is a priori undetermined.

The following example illustrates the interpretation of Table 5.5. The rows "probability at mean" represent the expected probabilities of obtaining a given index score when all independent variables are assigned their mean value. Therefore, the probability to obtain an index value of six is 26.69 percent. An increase in the amount of terror attacks by one standard deviation would, ceteris paribus, result in a reduction of the probability of 0.74 percent (i.e., $(-0.0004 \cdot 18.44) \cdot 100$). While this number is far from being negligible, the result also implies that the reduction in human rights is not dramatic. As can be seen, the marginal effects are always significant at the one percent level.

Table 5.5 also shows the marginal effects of suicide attacks and the number of persons killed. According to the results, the pattern is quite similar as compared to those of the number of events. As anticipated, the magnitude of the marginal effect of suicide attacks is substantially larger than those of a "normal" terror event (almost tenfold). One additional person killed reduces the probability of obtaining an index value of six (or seven) by 0.01 percent.

Regarding civil liberties, an increase of terror events by one standard deviation causes a reduction of the probability to obtain an index value of five by 1.29 percent.[21] However, while five marginal effects are significant at the five percent level, three are not significant at conventional levels. As before, the marginal effect of suicide attacks is much larger.

Table 5.6, finally, presents the results for the individual components of physical integrity – the dimension that turned out to be most clearly affected by terror attacks. As the individual dimensions vary from 0-2 only, variation among high income countries is extremely low. As a consequence, the results for those countries are completely deter-

[20] As Greene (2003, p.739) puts it, "without a fair amount of extra calculation, it is quite unclear how the coefficients in the ordered probit model should be interpreted."

[21] The probability of obtaining an index score of five is 43.9 percent (at the mean of all independent variables).

Table 5.5: Results marginal effects – dependent variable: human rights

Index value	**0**	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**E[y]**
Physical integrity rights										
Probability at mean	0.0004	0.0032	0.0175	0.0587	0.1962	0.2972	0.2669	0.1427	0.0171	5.22
Terror events										
Marginal effect	3.7E-06	2.6E-05	0.0001	0.0003	0.0005	0.0002	-0.0004	-0.0006	-0.0001	-0.0034
p-value	0.02	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01
Suicide attacks										
Marginal effect	4.1E-05	0.0003	0.0012	0.0030	0.0058	0.0018	-0.0046	-0.0062	-0.0013	-0.0379
p-value	0.03	0.01	0.00	0.00	0.00	0.01	0.00	0.00	0.00	0.00
Terror fatalities										
Marginal effect	9.8E-07	7.0E-06	3.0E-05	0.0001	0.0001	4.4E-05	-0.0001	-0.0001	-3.2E-05	-0.0009
p-value	0.01	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Civil liberties										
Probability at mean		1.1E-09	0.0001	0.0438	0.5179	0.4309	0.0073	1.1E-07		4.40
Terror events										
Marginal effect		1.3E-11	4.2E-07	0.0002	0.0006	-0.0007	-3.8E-05	-1.1E-09		-0.0010
p-value		0.57	0.22	0.03	0.02	0.02	0.08	0.45		0.02
Suicide attacks										
Marginal effect		8.9E-11	2.9E-06	0.0012	0.0039	-0.0049	-0.0003	-7.9E-09		-0.0066
p-value		0.56	0.19	0.01	0.00	0.00	0.04	0.43		0.00

Notes: This table depicts the marginal effects of the significant coefficients from Tables 5.3 and 5.4. "Probability at mean" gives the probability of observing a given index value when all independent variables are set to their corresponding sample mean. All marginal effects are calculated by assigning all independent variables the respective sample mean. The corresponding p-values are given below the coefficient. "E[y]" represents the expected value of human rights in the "probability at mean"-row and the marginal effect at that index value in the "marginal effect"-row.

mined in the ordered probit regressions, and some of the regressions do not converge. We therefore opted to exclude high income countries from the regressions. The dummy for legal German origin also had to be omitted. Note, however, that the main results are unchanged by the omission of high income countries.

Which dimensions drive the results? According to Table 5.6, almost all individual dimensions of physical integrity rights are negatively affected by terror. At least at the ten percent level of significance, the number of terror events increases the number of people disappearing, extrajudicial killings, and tortures. Governments restrict physical integrity rights among all dimensions as a consequence of suicide attacks, also at least at the ten percent level of significance. Again, the magnitude of the effect is much larger for suicide attacks. The number of people killed by terrorists increases the number of people disappearing, with a coefficient significant at the five percent level.

To summarize, there is clear evidence that – on average – governments respond to terrorism by restricting those very rights they want to protect in the first place. The next section tests whether the impact of our main variables of interest on physical integrity rights and civil liberties is robust to the inclusion of additional variables, the method of estimation, and sample period.

5.7 Tests for Robustness

We pursue various strategies to test the robustness of the impact of terror events on physical integrity rights and civil liberties. First, we replicate the regression with a dummy for the occurrence of terror in a given country and year in place of the number of terror events. Our results might be driven by the linear relationship we impose for the effect of the number of terror events on physical integrity rights. By assigning the value of one in cases when at least one terror event occurred we circumvent this problem. This comes at the cost of reduced information, however.

As our second test for robustness, we exclude those terror events that can be considered to be marginal. We excluded all events in which no person was physically harmed. More precisely, we excluded events in which the number of persons killed and the num-

Table 5.6: Results disaggregation – dependent variable: physical integrity subcomponents

	(1) disa	(2) kill	(3) impr	(4) tort	(5) disa	(6) kill	(7) impr	(8) tort	(9) disa	(10) kill	(11) impr	(12) tort
Lagged dpn.	1.061	1.121	1.108	0.904	1.076	1.133	1.106	0.914	1.070	1.131	1.108	0.915
	(15.04)***	(17.47)***	(16.10)***	(15.17)***	(15.06)***	(17.93)***	(16.01)***	(15.49)***	(15.06)***	(17.92)***	(16.04)***	(15.45)***
Inc. lwr mid	-0.071	0.053	-0.102	-0.104	-0.074	0.039	-0.100	-0.133	-0.073	0.040	-0.101	-0.124
	(0.60)	(0.50)	(0.92)	(0.84)	(0.64)	(0.37)	(0.91)	(1.06)	(0.62)	(0.38)	(0.91)	(0.99)
Inc. upr mid	0.248	0.353	0.127	0.253	0.239	0.334	0.135	0.234	0.231	0.330	0.128	0.232
	(2.01)**	(2.62)***	(1.08)	(1.64)	(2.00)**	(2.53)**	(1.14)	(1.49)	(1.90)*	(2.50)**	(1.07)	(1.48)
L.o. British	-0.224	-0.235	-0.290	0.009		-0.238	-0.285			-0.240	-0.289	0.005
	(1.07)	(1.49)	(1.51)	(0.05)		(1.51)	(1.50)			(1.51)	(1.50)	(0.03)
L.o. French	-0.404	-0.354	-0.391	-0.059	-0.196	-0.375	-0.396	-0.093	-0.196	-0.373	-0.393	-0.084
	(2.16)**	(2.48)**	(2.07)**	(0.38)	(1.75)*	(2.60)***	(2.09)**	(0.83)	(1.69)*	(2.59)***	(2.07)**	(0.53)
L.o. Socialist					0.223			-0.000	0.223			
					(1.08)			(0.00)	(1.07)			
Pop. (log)	-0.177	-0.190	-0.264	-0.198	-0.184	-0.200	-0.263	-0.211	-0.178	-0.198	-0.262	-0.208
	(5.58)***	(7.70)***	(5.54)***	(6.16)***	(5.89)***	(8.42)***	(5.57)***	(6.52)***	(5.70)***	(8.14)***	(5.49)***	(6.47)***
Democracy	-0.001	-0.007	0.050	0.016	-0.002	-0.008	0.051	0.015	-0.002	-0.008	0.050	0.014
	(0.16)	(1.05)	(6.77)***	(2.06)**	(0.30)	(1.19)	(6.77)***	(1.85)*	(0.29)	(1.20)	(6.74)***	(1.85)*
Terror events	-0.006	-0.008	-0.001	-0.017								
	(1.76)*	(3.30)***	(0.28)	(2.42)**								
Suicide attacks					-0.141	-0.131	-0.183	-1.020				
					(1.96)*	(2.66)***	(2.86)***	(1.80)*				
Terror fatalities									-0.003	-0.002	-0.001	-0.004
									(2.47)**	(1.35)	(0.94)	(1.28)
Observations	1,650	1,641	1,646	1,648	1,650	1,641	1,646	1,648	1,650	1,641	1,646	1,648
Countries	83	83	83	83	83	83	83	83	83	83	83	83
Years	21	21	21	21	21	21	21	21	21	21	21	21
Pseudo R^2	0.27	0.30	0.37	0.24	0.27	0.30	0.37	0.24	0.27	0.30	0.37	0.24

Notes: This table shows results for the four subindicators of the physical integrity rights indicator: "disa" is disappearances, "kill" is kills, "impr" is imprisonment and "tort" is torture. "Inc." represents income, "L.o." legal origin, "Pop." population and "dpn." the endogenous variable. The results are derived by running ordered probit regressions with clustering at the country level and including annual time dummies. */**/*** indicates significance at the 10/5/1-% level; robust absolute z-statistics are given in parantheses.

ber of persons injured were both zero and/or missing. Of course, it is debatable which threshold constitutes a severe event. We choose the lowest threshold possible. While this is the most objective threshold from our point of view, we are clearly aware that events in which no one is physically harmed may still have a major impact, e.g., the London bombings of July 21, 2005.

Third, we distinguish between domestic and international terror. A domestic incident is defined as an event in which both the target and the attackers are domestic. To some extent, the consequences of these forms of terrorism might be different. A priori, domestic terror might be more likely to increase governments' respect for human rights (Hypothesis 5.2), while there is no reason to expect the same for international terror. To the contrary, when it comes to international terrorism, Hypothesis 5.1 seems more likely to hold.

Fourth, we split our sample in two sub-periods and investigate whether the negative impact of terror events holds. As Figure 4 shows, terrorism increased markedly after 1997. We choose this year as cut-off. The increase is to some extent driven by the explicit inclusion of domestic events from 1998 onwards as noted in the definitions of the MIPT Terrorism Knowledge Base. However, domestic terror events are also reported for several cases prior to 1998.

To some extent, fifth, our result might be due to reversed causality. When people engage in terrorist activities as a consequence of the human rights situation in their country, low human rights might not be the consequence of terror, but its cause. Tests for endogeneity show, however, that the exogeneity of terror can not be rejected at conventional levels of significance. In choosing instruments for terror, we follow Dreher and Gassebner (2007) showing that countries voting in line with the US in the United Nations General Assembly and countries with more fractionalized governments are more frequently the target of terrorist attacks. When we lag terror events by one year instead of using contemporaneous values, the results also remain unchanged. Still, we deal with the issue of potential endogeneity in some detail and report results estimated with 2SLS (where terror is instrumented with voting behavior in the General Assembly and government fractionalization) below.

Given that our estimation setup includes the lagged dependent variable, 2SLS estimations may, however, suffer from dynamic panel bias. Therefore, we employ the GMM estimator as suggested by Arellano and Bover (1995) and Blundell and Bond (1998) in addition. We present results employing the two-step estimator implemented by Roodman (2006) in Stata, including Windmeijer's (2005) finite sample correction. We treat the lagged dependent variable and the terror events as endogenous and the additional covariates as strictly exogenous. As before, we include time dummies in the regression. We report results of the Sargan-Hansen test on the validity of the instruments used (amounting to a test for the exogeneity of the covariates), and the Arellano-Bond test of first and second order autocorrelation. While autocorrelation of first order has to be present in order for the estimator to be consistent, second-order autocorrelation must be absent. In order to minimize the number of instruments in the regressions we collapse the matrix of instruments as suggested in Roodman (2006). Doing so reduces the instruments to 85 (physical integrity) and, respectively, 94 (civil liberties).[22]

In empirical research, an important difficulty is that several models may all seem reasonable given the data, but yield different conclusions about the parameters of interest. Hence, as our final test for robustness, we employ the so-called extreme bounds analysis (EBA) to examine to what extent the number of terror events in a country is a robust determinant of physical human rights. The EBA has been widely used in the economic growth literature and became recently one of the standard tools for robustness analysis in the political economy literature.[23] The EBA aims to examine how sensitive the estimation results are to the inclusion of additional variables. We estimate equations of the following general form:

$$RIGHTS_{i,t} = \alpha M_{i,t} + \beta F_{i,t} + \gamma Z_{i,t} + u_{i,t} \tag{5.5}$$

where M is a vector of 'standard' explanatory variables; F is the variable of interest; Z is a vector of up to three (here we follow Levine and Renelt, 1992) possible additional

[22]It is necessary to limit the number of instruments because the power of the Sargan-Hansen test is low when many instruments are used (see Bowsher, 2002).

[23]For instance, de Haan and Sturm (2000), Sturm et al. (2005) and Inklaar et al. (2007).

explanatory variables, which according to the literature may be related to the dependent variable; and u is an error term. The extreme bounds test as suggested by Leamer (1983) for variable F states that if the lower extreme bound for β – i.e., the lowest value for β minus two standard deviations – is negative, while the upper extreme bound for β – i.e., the highest value for β plus two standard deviations – is positive, the variable F is not robustly related to Y. In our case, the variable of interest is the number of terror events in a certain year and country. The M vector contains the base variables as introduced above. In the Z vector, we include the 52 variables suggested in the robustness analysis of Dreher et al. (2006a). Among them is a dummy for years of war, political variables like the ICRG indicator of political risk, and economic variables like trade and government revenue. Table 5.10 in the Appendix lists all variables with their definitions and sources.

It is rare in empirical research that we can say with certainty that some model dominates all others in all possible dimensions. In these circumstances, it makes sense to check how sensitive the findings are to alternative modelling choices. The EBA provides a relatively simple means of doing exactly this. We report the percentage of the regressions in which the coefficient of the variable F is significantly different from zero at the 5 percent level as well as the outcomes of the cumulative distribution function (CDF) test. The CDF test as proposed by Sala-i-Martin (1997) is based on the fraction of the cumulative distribution function lying on each side of zero. CDF(0) indicates the larger of the areas under the density function either above or below zero; in other words, regardless of whether this is CDF(0) or 1-CDF(0). So CDF(0) will always be a number between 0.5 and 1.0. We consider a variable to be robust if the CDF(0) test statistic $>$ 0.95, following Sturm and de Haan (2005).[24]

[24]Recently, Sala-i-Martin et al. (2004) proposed a so-called Bayesian Averaging of Classical Estimates (BACE) approach to check the robustness of different explanatory variables in growth regressions. This approach builds upon the approach as suggested by Sala-i-Martin (1997) in the sense that different specifications are estimated (by OLS) to check the sensitivity of the coefficient estimate of the variable of interest. The major innovation of BACE as compared to Sala-i-Martin's approach is that there is no set of fixed variables included and the number of explanatory variables in the specifications is flexible. The biggest disadvantages of the BACE approach are the need of having a balanced data set, i.e., an equal number of observations for all regressions (due to the chosen weighting scheme), the restriction of limiting the list of potential variables to be less than the number of observations and the computational burden. For a recent application see Lamla (2007).

Turning to the results of our robustness tests, Table 5.7 shows that the impact of terror on physical integrity rights is extremely robust as to how terror events are measured, and the same is true regarding the choice of sample period. In all cases terror reduces governments' respect for human rights at the one percent level of significance (columns 1 to 6). We see that splitting among the time dimension and differentiating between domestic and international terror does not qualitatively change the previous results. Furthermore, the significantly smaller (at the one percent level) coefficient of domestic terror gives mild support to our conjecture that Hypothesis 5.2 is "more" valid for domestic forms of terror. Note, however, that the net effect is still negative.

Column 7 shows the 2SLS results. Our instruments – voting in line with the US in the United Nations General Assembly and government fractionalization – are not rejected by the Sargan-Hansen test at the five percent level of significance (while they are at the ten percent level). The first stage F-test indicates the power of these instruments, as they easily pass the threshold of 10 proposed by Staiger and Stock (1997). As can be seen, the (negative) impact of terror on human rights remains significant at the ten percent level.

Column 8 reports results from the GMM estimator. Again, the results remain qualitatively unchanged, with the impact of terror being significant at the one percent level.[25] Both the Sargan-Hansen test and the Arellano-Bond test do clearly not reject the specification. We take this as evidence that endogeneity is not an issue here and that our previous results are valid. The GMM approach facilitates the interpretation of the coefficient as compared to the ordered probit specifications. An increase in terror attacks by one standard deviation reduces the physical integrity index by 0.1.

Table 5.8 replicates the analysis for civil liberties. According to the estimates, the impact of terror on civil liberties is less robust as compared to those on physical integrity rights. Still, the impact stays significant in the two sub-samples. The same is true when we focus on severe terrorism only, and distinguish domestic from international terror. In the instrumental variables and GMM regressions, and when employing a dummy for

[25]We also estimated the GMM setup for the number of suicide attacks and the number of people killed. All previous findings remain unchanged.

Table 5.7: Results tests of robustness – dependent variable: physical integrity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Method	**Probit**	**Probit**	**Probit**	**Probit**	**Probit**	**Probit**	**2SLS**	**GMM**
Lagged dependent	0.532	0.536	0.538	0.531	0.532	0.495	0.644	0.306
	(23.17)***	(23.41)***	(23.60)***	(22.98)***	(22.02)***	(9.77)***	(27.59)***	(6.29)***
Income lower middle	0.001	-0.033	-0.034	-0.028	-0.008	-0.034	-0.019	-0.128
	(0.02)	(0.40)	(0.41)	(0.34)	(0.09)	(0.24)	(0.24)	(0.50)
Income upper middle	0.280	0.252	0.253	0.257	0.226	0.466	0.252	0.483
	(2.70)***	(2.31)**	(2.35)**	(2.31)**	(1.94)*	(2.65)***	(2.60)***	(1.65)
Income high OECD	1.170	1.112	1.118	1.172	1.158	1.501	1.024	2.060
	(7.30)***	(6.76)***	(6.75)***	(7.42)***	(6.57)***	(5.96)***	(6.84)***	(7.64)***
Income high	0.125	0.077	0.051	0.165	0.143	0.267	0.370	0.352
	(0.86)	(0.56)	(0.35)	(1.27)	(1.22)	(1.39)	(1.77)*	(1.14)
Legal origin British	-0.897	-0.903	-0.898	-0.859	-0.782	-0.999	0.046	0.299
	(5.02)***	(4.76)***	(4.74)***	(4.66)***	(3.36)***	(3.61)***	(0.26)	(1.70)*
Legal origin French	-0.979	-1.002	-0.996	-0.929	-0.835	-1.074	0.035	0.035
	(5.16)***	(5.01)***	(4.96)***	(4.81)***	(3.44)***	(3.82)***	(0.20)	(0.19)
Legal origin Socialist	-0.695	-0.701	-0.694	-0.650	-0.603	-0.711	0.387	0.837
	(3.21)***	(3.07)***	(3.03)***	(2.93)***	(2.25)**	(2.26)**	(1.94)*	(2.53)**
Legal origin German	-1.126	-1.143	-1.140	-1.133	-1.040	-1.496	-0.387	-0.403
	(3.65)***	(3.72)***	(3.71)***	(3.68)***	(3.16)***	(3.40)***	(1.55)	(1.16)
Population (log)	-0.186	-0.198	-0.200	-0.195	-0.177	-0.270	-0.229	-0.446
	(8.49)***	(8.97)***	(9.08)***	(8.74)***	(7.55)***	(6.71)***	(8.75)***	(8.22)***
Democracy	0.020	0.018	0.017	0.021	0.023	0.016	0.020	0.033
	(3.34)***	(3.09)***	(2.92)***	(3.61)***	(4.10)***	(1.80)*	(3.19)***	(2.81)***
Terror events, dummy	-0.188							
	(2.80)***							
Severe terror events		-0.007						
		(3.69)***						
Domestic terror events			-0.002					
			(2.99)***					
International terror events				-0.019				
				(4.86)***				
Terror events					-0.025	-0.003	-0.014	-0.005
					(5.62)***	(4.73)***	(1.80)*	(4.58)***
Observations	2,217	2,217	2,217	2,217	1,676	541	1,846	2,217
Countries	111	111	111	111	111	111	111	111
Years	21	21	21	21	16	5	21	21
Pseudo R^2	0.31	0.32	0.31	0.31	0.31	0.33	0.72	
Sargan test (p-value)							0.07	0.55
First stage F-statistic							24.87	
AR(1) test (p-value)								0.00
AR(2) test (p-value)								0.33

Notes: This table shows results for the physical integrity rights indicator. Columns 1 to 8 are derived by running ordered probit regressions with clustering at the country level. Column 7 results from a two stage least squares regression with government fractionalization and percentage of votes cast in line with the U.S. in the UN General Assembly as the instruments. Column 8 is obtained by running the Blundell-Bond system GMM estimator. All regressions include annual year dummies.
*/**/*** indicates significance at the 10/5/1-% level; robust absolute z-statistics are given in paranthe-ses.

terror events, however, the impact of terror is no longer significant at conventional levels. To some extent this is not surprising. The civil liberties index is sometimes used as an indicator of democracy. Our results also support Abadie (2006) showing democracy to influence terror. If civil liberties also capture democracy and causality runs from democracy to terror, it is not surprising that the correlation between terror and civil liberties does not hold in the instrumental variables regression.

Table 5.9 reports the results of the extreme bounds analysis. As can be seen in the upper part of the table, the CDF(0) of the three base variables easily exceeds the threshold of 0.95 when we focus on physical integrity rights. Lagged physical integrity and population size have a CDF(0) of almost one, while that of democracy is 0.95. The former two variables are significant at the five percent level in almost all of the approx. 23,000 regressions run; democracy is significant in 80 percent of these regressions. Turning to our variable of main interest – the number of terror events in a given country and year – the table shows that the CDF(0) also easily exceeds the critical threshold of 0.95. At the five percent level of significance, the number of terror events is significant in 96 percent of the regressions run. We conclude that our result is indeed robust to the inclusion of other variables suggested in the human rights literature.

Turning to civil liberties, the lower part of the table shows that there is no robust impact of terror. While the results for the other variables are more or less similar to those described above, the number of terror events exert a significant impact on civil liberties in only 58 percent of the regressions run. The CDF(0) of 0.88 confirms that the impact of terror events on civil liberties can not be considered to be completely robust according to standard criteria.

5.8 Conclusions

We analyzed the link between terror and human rights. Our theoretical model points to the subjectively perceived danger of a terror attack and the trade off between security and human rights as major determinants of the level of human rights. Experiencing an increased threat of terror, citizens update their subjective probability of a terror

Table 5.8: Results tests of robustness – dependent variable: civil liberties

Method	(1) Probit	(2) Probit	(3) Probit	(4) Probit	(5) Probit	(6) Probit	(7) 2SLS	(8) GMM
Lagged dependent	1.827	1.825	1.826	1.825	1.724	2.779	0.836	0.819
	(17.41)***	(17.34)***	(17.35)***	(17.33)***	(15.86)***	(296)***	(61.57)***	(8.28)***
Income lower middle	0.088	0.097	0.095	0.101	0.103	0.172	0.030	0.084
	(1.25)	(1.41)	(1.39)	(1.48)	(1.44)	(5.27)***	(1.10)	(1.28)
Income upper middle	0.292	0.298	0.298	0.303	0.281	0.481	0.092	0.157
	(2.95)***	(3.01)***	(3.01)***	(3.03)***	(2.68)***	(12.64)***	(2.73)***	(1.62)
Income high OECD	1.390	1.404	1.405	1.452	1.416	1.687	0.262	0.407
	(7.61)***	(7.85)***	(7.81)***	(8.42)***	(7.82)***	(34.30)***	(5.33)***	(2.28)**
Income high	-0.030	0.007	-0.010	0.053	0.037	0.330	-0.018	-0.029
	(0.26)	(0.06)	(0.08)	(0.43)	(0.30)	(9.32)***	(0.25)	(0.34)
Legal origin British	-0.293	-0.280	-0.281	-0.233	-0.201	-4.502	-0.009	0.046
	(0.76)	(0.72)	(0.72)	(0.60)	(0.49)	(147)***	(0.16)	(1.17)
Legal origin French	-0.412	-0.393	-0.395	-0.324	-0.285	-4.618	-0.035	0.010
	(1.07)	(1.01)	(1.01)	(0.83)	(0.70)	(124)***	(0.60)	(0.25)
Legal origin Socialist	-0.419	-0.414	-0.410	-0.370	-0.386	-4.619	0.011	0.024
	(1.03)	(1.01)	(1.00)	(0.91)	(0.89)	(134)***	(0.16)	(0.22)
Legal origin German	-0.580	-0.575	-0.575	-0.535	-0.473	-5.101	0.031	-0.003
	(1.35)	(1.33)	(1.33)	(1.24)	(1.03)	(111)***	(0.36)	(0.05)
Population (log)	-0.083	-0.076	-0.079	-0.070	-0.064	-0.064	-0.039	-0.029
	(4.45)***	(4.16)***	(4.31)***	(3.88)***	(3.33)***	(25.01)***	(4.27)***	(1.62)
Democracy	0.045	0.046	0.046	0.048	0.047	0.073	0.023	0.023
	(5.22)***	(5.32)***	(5.28)***	(5.47)***	(4.99)***	(19.70)***	(6.86)***	(1.33)
Terror events, dummy	0.027							
	(0.54)							
Severe terror events		-0.005						
		(2.23)**						
Domestic terror events			-0.001					
			(1.68)*					
International terror events				-0.012				
				(2.57)**				
Terror events					-0.013	-0.002	0.002	-0.000
					(2.83)***	(11.46)***	(0.78)	(0.51)
Observations	3218	3218	3218	3218	2671	547	2325	3218
Countries	111	111	111	111	111	111	111	111
Years	30	30	30	30	25	5	30	30
Pseudo R2	0.65	0.65	0.65	0.65	0.63	0.78	0.93	
Sargan test (p-value)							0.37	0.13
First stage F-statistic							30.60	
AR(1) test (p-value)								0.00
AR(2) test (p-value)								0.10

Notes: This table shows results for the civil liberties indicator. Columns 1 to 8 are derived by running ordered probit regressions with clustering at the country level. Column 7 results from a two stage least squares regression with government fractionalization and percentage of votes cast in line with the U.S. in the UN General Assembly as the instruments. Column 8 is obtained by running the Blundell-Bond system GMM estimator. All regressions include annual year dummies.
*/**/*** indicates significance at the 10/5/1-% level; robust absolute z-statistics are given in paranthe-ses.

Table 5.9: Results EBA – dependent variable: human rights

	Avg. Beta	**Avg. S.E.**	**% Sign.**	**CDF(0)**	**lower Bound**	**upper Bound**
Physical integrity						
Lagged dependent	0.524	0.043	99.97	0.9999	-9.38	24.93
Population (log)	-0.256	0.049	99.36	0.9985	-26.77	27.98
Democracy	0.025	0.011	80.11	0.9549	-28.56	9.06
Number of terror events	-0.009	0.005	96.03	0.9861	-9.15	24.08
Civil liberties						
Lagged dependent	3.060	0.157	100.0	0.9999	-1.39	735.45
Population (log)	-0.074	0.037	92.99	0.9688	-70.35	119.47
Democracy	0.051	0.018	72.71	0.8972	-35.12	32.89
Terror events	-0.016	0.003	57.60	0.8795	-6.40	0.64

Notes: This table shows results for the extreme bounds analysis. All results are obtained by running ordered probit regressions with clustering at the country level and including annual year dummies as well as dummies for income and legal origin. "Avg. Beta" and "Avg. S.E." represent the unweighted averages of the coefficient and the standard error, respectively. "% Sign." gives the percentage of regressions in which the coefficient is significant at the five percent level. "CDF(0)" represents the unweighted average of the CDF. Results for physical integrity (civil liberties) are based on 22,724 (16,877) combinations with 1,140 (1,771) observations, on average. Table 5.10 in the Appendix lists the 52 variabes employed in the EBA.

attack, and hence demand more security at the expense of human rights. Politicians – in competition with each other – follow this shift of perception and restrict human rights in the aftermath of a terror attack in political equilibrium. Our empirical results indicate that governments' answer to terror indeed is – on average – to restrict freedom. Our study supports anecdotal evidence suggesting that governments – under the threat of terror – violate those very rights they want to protect from terror in the first place.

Our empirical analysis suggests a rise in the level of terror to significantly reduce governments' respect for basic human rights. According to our results, terror increases the probability of extrajudicial killings, political imprisonment, and torture. This result is extremely robust, as extreme bounds analysis for almost 23,000 regressions shows. However, the magnitude of the effect is rather small. Specifically, an increase of the number of terror attacks of one standard deviation reduces the probability of obtaining a physical integrity score of 6 (out of 8) by 0.74 percent. To some extent, civil liberties (measured by the Freedom House index) are also restricted as a consequence of terrorism. However, these results are not completely robust to the specification of the model. We find no effect of terror on "positive rights" such as political participation, freedom of religion, freedom of speech, or freedom of movement.[26]

Which policy conclusions do arise from this? Clearly, if human rights restrictions really increase security, people might be willing to trade some of their rights for greater security. However, whether and to what extent human rights violations actually increase security is not at all obvious. This is even more true, as our analysis shows that governments reduce physical human rights as a consequence of terror, rather than civil liberties or empowerment rights. According to Frey and Luechinger (2003) and Freytag et al. (2006), restricting human rights in reaction to terror attacks may reduce the individual opportunity costs of potential terrorists, and thus rather increase terrorism. Frey and Luechinger argue that there may be superior strategies than deterrence in fighting terrorism. The same could hold true for restricting human rights as an answer to terrorism.

[26]Note that our data do not cover the major time of the "war against terror" in the aftermath of 9/11. The results do thus not depend on this exceptional event.

5.9 Appendix

Table 5.10: Variables used in the EBA

Variable	Description	Source
Age of democracy	Defined as: AGE = (2000 - DEM_AGE)/200 and varying between 0 and 1, with US being the oldest democracy (value of 1).	Persson and Tabellini (2003)
Age of parties	Average age of political parties.	Beck et al. (2001)
Area	Land area (square kilometer).	World Bank (2006)
Catholic Share	Share of catholics in population.	Persson and Tabellini (2003)
Constituency	Indicates whether the constituencies of the senators are states/provinces.	Beck et al. (2001)
Debt service	Public and publicly guaranteed debt service (% of GNI).	World Bank (2006)
Dominant religion	Percent of dominant religion.	Alesina et al. (2003)
Election year, legislative	Dummy for legislative elections.	Beck et al. (2001)
Ethnic fractionalization	Index of ethnic fractionalization.	Alesina et al. (2003)
FDI	Foreign direct investment, net inflows (% of GDP).	World Bank (2006)
Federalism, dummy	Dummy for federal states.	Elazar (1996)
Fractionalization	Index of ethnolinguistic fractionalization, approximating the level of lack of ethnic and linguistic cohesion within a country, ranging from 0 (homogeneous) to 1 (strongly fractionalized) and averaging 5 different indexes.	Persson and Tabellini (2003)
Gap in schooling	Difference between years of schooling male and years of schooling female.	Barro and Lee (2000)
GDP	GDP in current US$.	World Bank (2006)
GDP per capita	Real GDP per capita in constant dollars (in international prices, base 1985).	Penn World Tables
Government debt	Central government debt, total (% of GDP).	World Bank (2006)
Government employees	Share of government employees in total employment.	World Bank (2001)
Government Fractionalization	"The probability that two deputies picked at random from among the government parties will be of different parties."	Beck et al. (2001)
Government transfers	Transfers to sub-national from other levels of Government (% of total sub-national revenues and grants).	IMFs Government Finance Statistics
Growth	GDP growth (annual, percent).	World Bank (2006)
Human Development Indicator	Composite index based on measures of life expectancy, literacy, education, and standards of living.	UNDP (2005)
IMF program, dummy	IMF program is at least five months in effect in a given year.	Dreher (2006b)
Infant mortality	Mortality rate, infant (per 1,000 live births).	World Bank (2006)
Investment	Gross capital formation (% of GDP).	World Bank (2006)

continued on next page...

Variable	Description	Source
Investment growth	Gross capital formation (annual percent growth).	World Bank (2006)
Language fractionalization	Index of language fractionalization.	Alesina et al. (2003)
Left government, dummy	Indicates whether the main government party is left-wing.	Beck et al. (2001)
Life expectancy	Life expectancy at birth, total (years).	World Bank (2006)
New state, dummy	Dummy for new states.	Gallup et al. (2001)
Number of human rights organizations	Number of human rights related NGOs being represented in a country.	UIA (2000)
Number of wars	Number of wars/conflicts in specific year and country.	Ghosn et al. (2004)
Political risk	Political Risk Rating, annual averages, ranging from 0-100.	International Country Risk Guide
Post election, executive	Share of the year within after 12 months of an executive election.	Dreher and Vaubel (2005)
Post election, legislature	Share of the year within after 12 months of a legislative election.	Dreher and Vaubel (2005)
Pre-election, executive	Share of the year within 12 months of an executive election.	Dreher and Vaubel (2005)
Pre-election, legislature	Share of the year within 12 months of a legislative election.	Dreher and Vaubel (2005)
Primary schooling	Average years of primary schooling in the total population.	Barro and Lee (2000)
Protestant share	Share of protestants in population.	Persson and Tabellini (2003)
Religious fractionalization	Index of religious fractionalization.	Alesina et al. (2003)
Revenue decentralization	Sub-national Revenues (% of total revenues)	IMFs Government Finance Statistics
Special interests	Dummy for special interest executive parties.	Beck et al. (2001)
Sub-national Tax Revenue	Sub-national Tax Revenue (% of total sub-national revenues and grants).	IMFs Government Finance Statistics
Tiers	Number of government tiers.	Treisman (2000)
Tiers, average	Average area first tier units (thousands square kilometers per unit).	Treisman (2000)
Trade	Exports and Imports (% of GDP).	World Bank (2006)
Urban population	Urban population (% of total).	World Bank (2006)
Vertical imbalance	Intergovernmental transfers as a share of sub-national expenditures.	IMFs Government Finance Statistics
World Bank projects	Number of World Bank projects at least five months in effect in a given year.	Boockmann and Dreher (2003)
Years in office	Indicates the number of years the government chief executive has been in office.	Beck et al. (2001)
Years left	Number of years the government chief executive remains in office.	Beck et al. (2001)
Years of independence	Ranging from 0 to 250 (the latter value is used for all non-colonized countries).	Persson and Tabellini (2003)

Chapter 6

Terrorism and Cabinet Durability: Empirical Evidence

6.1 Introduction

Since the terrorist attacks on September 11, 2001 (9/11), there has been increased academic interest into the consequences of terrorism. By now, there is abundant empirical evidence indicating that terrorism affects society in many ways – ranging from increased mental health problems (e.g., Schuster et al., 2001) to sub-optimal macroeconomic outcomes (e.g., Abadie and Gardeazabal, 2003; Blomberg et al., 2004; Blomberg and Hess, 2006; Frey et al., 2007).[1]

However, these consequences are generally beyond the interest of terrorists as they use violence or the threat of violence primarily to achieve social or political goals. Their actions – most often directed against randomly chosen civilians – are intended to create an atmosphere of fear and to make political decision makers respond to their demands

This chapter is an adapted version of Gassebner et al. (2007c).

[1]There is of course also ample research on the causes of terrorism (e.g., Krueger and Malečková, 2003; Abadie, 2006; Dreher and Gassebner, 2007) and counter-terrorism (e.g., Enders and Sandler, 1993; Frey and Luechinger, 2003; Frey and Luechinger, 2004; Sandler, 2005).

– or ultimately to topple existing political regimes.[2] The chaos and instability that arises as a result of terrorist attacks is likely to affect the public opinion regarding the current government and, in particular, its counter-terrorism policies (Downs-Le Guin and Hoffman, 1993). If the perception of the public changes with respect to the competence of the incumbent government, then the position of the government may be at risk. A relevant question to consider, therefore, is whether terrorism affects the duration of governments.

There are several political economy models that provide guidance why there may be a relationship between terrorism and cabinet duration. For instance, Lupia and Strøm (1995) argue that critical events (e.g., terrorism) affect the public opinion causing a shift in the power distribution of political parties, which may enforce early elections. However, there is also a popular belief that the electorate starts to 'rally around the flag' when national security is at stake (Mueller, 1970). Thus, whether terrorism affects the duration of governments remains an empirical question.

We examine the impact of terrorism on the duration of governments using a panel data set consisting of more than 150 countries over the period 1968-2002. Our measure for cabinet duration is taken from Databanks International (2005), while we extract different terrorist indicators from the MIPT Terrorism Knowledge Base. We use different hazard models in our econometric analysis to take duration dependence into account.

Our chapter relates to several studies focusing on the political effects of terrorism. Chari (2004) studies the impact of the Madrid train bombings on the 2004 parliamentary elections in Spain. Furthermore, Berrebi and Klor (2006) analyze the interaction between Palestinian terrorist attacks and election outcomes in Israel. Time series evidence is provided by Chowanietz (2007), who examines the presence of rally effects in five industrialized countries after terrorist attacks.

The paper that comes closest to our work is Gassebner et al. (2007d), who use panel data to examine the impact of terrorism on the re-election probability of governments. Contrasting Gassebner et al. (2007d), we are not primarily interested in the reaction of the electorate in response to terrorist attacks, but aim to address a more general

[2]See definitions of terrorism by, e.g., Enders and Sandler (2006) or Encyclopaedia Britannica (2007).

question: can terrorism explain why some governments last longer than others? The broader perspective of this chapter implies that we do not solely examine election outcomes. Instead, we examine all years in which a particular government is in power and, as said, explicitly model duration dependence.

Our main finding is that terrorism decreases the average time in office of cabinets and hence increases cabinet instability. Although this result is very robust for different terrorism measures and different model specifications, we find that the effect varies across sub-samples. That is, terrorism hardly influences cabinet duration in OECD countries, nor does it in very autocratic countries. Instead, we observe that especially cabinets in countries that (frequently) switch from democracy to autocracy (and/or vice versa) are vulnerable in the presence of terrorism. Finally, we find that the effect of domestic terrorism is larger than the effect of trans-national terrorism.

The remainder of this chapter is organized as follows. In section 6.2 we derive our main hypothesis using the theoretical literature on cabinet duration. In section 6.3 we discuss our data and empirical model. Section 6.4 presents our estimation results. The final section discusses our results and concludes the chapter.

6.2 Related Literature

There are many studies that examine the question why some cabinets last longer than others. We opt not to discuss this voluminous literature but refer to surveys by Warwick (1994) and Grofman and van Roozendaal (1997). The latter provide a typology of five groups of variables which are proposed to be relevant determinants of cabinet stability.

The first category consists of characteristics of party strength in the legislature and variables measuring attributes of the cabinet related to party balance. Variables that fall into this category are, for example, party fractionalization, cabinet size or the effective number of parties in a cabinet. The second category refers to the ideological structure of party competition and cabinet composition. Ideological polarization between cabinet members and dominant central parties in a cabinet belong to this category. Thirdly, the institutional framework in which the political process takes place is dubbed to be impor-

tant. Institutional features that are particularly relevant are constitutional procedures and aspects of legislative organization. The fourth group of variables that affect cabinet instability are time dependent factors external to the cabinet and the legislature. Variables in this category are inflation and unemployment. Finally, the fifth group consists of factors related to the anticipated consequences of dissolution such as the probability of winning the next election.

We argue that terrorist attacks are events external to the cabinet and the legislature. These external events (also known as critical events) are the focal point of the events approach proposed by Browne et al. (1984). The events approach contrasts the so-called structural approach (encompassing the first three categories listed above), since preferences of cabinet members may well change over time. Lupia and Strøm (1995) develop a model that relates critical events to cabinet terminations. Their model starts from the premise that three parties bargain over a cabinet formation. Once a cabinet is installed, some unexpected event occurs that alters the power distribution in the coalition through a public opinion shock. If a coalition member perceives the marginal costs of remaining in the coalition to be higher than the marginal benefits it is optimal to end coalition participation.[3] When one coalition party decides to opt out, several scenarios are feasible. It is possible that the former coalition partners renegotiate and form a new coalition. Likewise, it could be that part of the former coalition forms a new coalition with one or more opposition parties. Finally, it is possible that new elections are held. In any case, changes in the composition of the cabinet are a likely consequence of the critical event.

As terrorists strive for maximum publicity and mainly target civilians, it is likely that the public opinion regarding the current government alters in the aftermath of an attack (Lerner et al., 2003). However, it is a priori not clear in which direction the public

[3]Although the Lupia-Strøm model refers to multiparty systems with an explicit cabinet coalition, we argue that the ruling party in a two-party system consists of implicit coalitions. These implicit coalitions are the interest groups that operate within the party. Upon a Lupia-Strøm critical event the power of these interest groups within the party changes. To maintain unity within the party, party leaders are likely to dissolve the cabinet and/or replace cabinet members according to the demands of the interest groups whose power has increased after the critical event. Hence, for the purpose of this chapter we do not need to differentiate between two-party and multi-party systems.

opinion will shift. The popularity of the incumbent government can be affected either positively or negatively. Hetherington and Nelson (2003), for instance, document that after the 9/11 terrorist attacks, the Gallup Poll approval ratings for U.S. President Bush improved from 51 percent on September 10 to 86 percent on September 15. According to Mueller (1970), this so called 'rally around the flag' phenomenon occurs when events happen that: (1) are international, (2) involve the United States and in particular the U.S. President and (3) are specific, dramatic and sharply focused. More recent studies argue that rally effects are not specific to the United States only, but also occurred in, for instance, Great Britain (Lai and Reiter, 2005) and Israel (Arian and Olzaeker, 1999). Although rally effects are mostly observed in cases of international (armed) conflict, there is some evidence that rally effects may also occur in the aftermath of terrorist attacks (Chowanietz, 2007).

It is also possible that the public opinion shifts away from the incumbent government. Gassebner et al. (2007d) argue that national security is arguably one of the most important public goods. Hence, the electorate may perceive a successful terrorist attack as a failure of one of the governments rudimentary processes (Holmes, 2001). This view can be traced back the models of Barro (1973) and Ferejohn (1986), in which the electorate holds the incumbent government accountable for the provision of public goods. Focusing on cabinet changes in election years, they conclude that the probability that the government will be ousted from office increases with approximately twelve percent after a terrorist attack.

It is ex ante not clear whether terrorism affects the tenure of cabinets positively or negatively. Therefore, we turn to an empirical analysis to establish a definitive answer on our research question.

6.3 Data and Method

To examine whether terrorism can explain why some governments remain longer in office than others, we employ time series cross section (TSCS) data for more than 150 countries over the period 1968-2004. An important issue in the modeling of duration data is to

take account of temporal dependence. Therefore, we incorporate the suggestions of Beck et al. (1998) in a conditional fixed effects logit (CFEL) model (see Chamberlain, 1980).[4] It turns out that in each specification the inclusion of the duration dependence parameters are jointly significant at the 1 percent level.[5] We thus estimate equations of the following form:

$$c_{it} = \sum_j \delta_{jt} + \alpha_i + \beta T_{it} + \gamma X_{it} + \epsilon_{it} \tag{6.1}$$

where c_{it} is a binary variable equal to 1 if the cabinet of country i is replaced in a given year t and 0 if the cabinet of country i in year t remains in office. $\sum_j \delta_{jt}$ takes the temporal dependence into account and consists of the years since the last cabinet change and three temporal splines. α_i is a country specific effect that accounts for all characteristics specific to country i. T_{it} denotes our terror measure. X_{it} is a vector of control variables and ϵ_{it} is an error term.

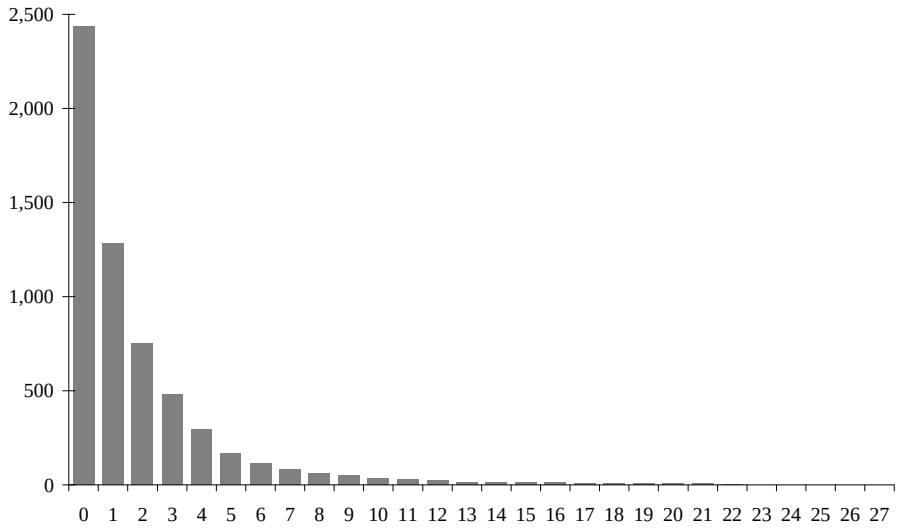
Our dependent variable, cabinet duration, is taken from Databanks International (2005). This source provides data on the number of cabinet changes per year. As the data limits our analysis to yearly observations, we discard multiple cabinet changes per year in our analysis and assume that for those years only one cabinet change has occurred.[6] Figure 6.1 shows the empirical distribution of the time in office of the cabinets in our data set. It shows that there are 5,419 cabinets in our sample of which only 52 percent last more than one year. The average duration of a cabinet is 1.9 years (not

[4]Our choice for a CFEL conditional fixed effects logit model is based on Hausman tests (Hausman, 1978). We test the null-hypothesis that all country fixed effects equal zero by comparing the estimates of a conditional fixed effects logit (CFEL) model and the unrestricted (pooled) logit model. The null-hypothesis of no country specific effects is rejected for all model specifications. Hence, CFEL should be preferred.

[5]In addition, we have also estimated other duration models such as a Cox proportional hazard model with time varying covariates. As these results are very similar to the results presented in the next section, we exclude them for readability reasons (results are available upon request). For a discussion on the equivalence of duration models and their solution, see Beck et al. (1998). For a general discussion of duration models, see Kiefer (1988). For a discussion of using Hazard models in the context of cabinet duration see Carmignani (2002).

[6]In our view, this is only of limited influence on our estimates as only 3.5 percent of all cases exhibit more than one cabinet change per year (208 out of 5,419 observations).

Figure 6.1: Empirical distribution of cabinet duration



Notes: The figure gives the empirical distribution of the time in office (measured in years) of the cabinets for our sample (N=5,419 observations).

shown), while the longest duration observed in our sample is the government of Bhutan, which remained in office for 27 years (1971-1998).

We construct different indicators measuring terrorism on the basis of information assembled by the MIPT Terrorism Knowledge Base.[7] This source provides data on transnational terrorism – cases in which the attacker and/or target are of foreign nationality – for individual countries over the period 1968-2004.[8] Our main indicator is the number of terror events for each country and year.[9] To probe the robustness of our results, we

[7]Available at: http://www.tkb.org/. The Terrorism Knowledge Base defines terror as "violence, or the threat of violence, calculated to create an atmosphere of fear and alarm." Terror is used to discourage the adversaries from acting at their free will. The motives for engaging in terror are political while the acts themselves are generally conducted in a way that will achieve maximum publicity, mainly by attacking civilians. Moreover, terrorist acts are mostly intended to create more than immediate physical damage – a long-time situation of fear and intimidation.

[8]It also provides data on domestic terrorism for the period 1998-2004. We use the data on domestic terrorism in our robustness analysis.

[9]Territories are assigned to the country formally governing the territory. Kashmir and the Persian Gulf are excluded as it is not obvious to which country they should be assigned to.

also use data on the number of suicide attacks, the number of casualties and the number of casualties per terrorist attack. The last two measures have the additional advantage that they allow us to examine whether terrorist attacks with many casualties have a larger impact on cabinets than terror events without casualties.

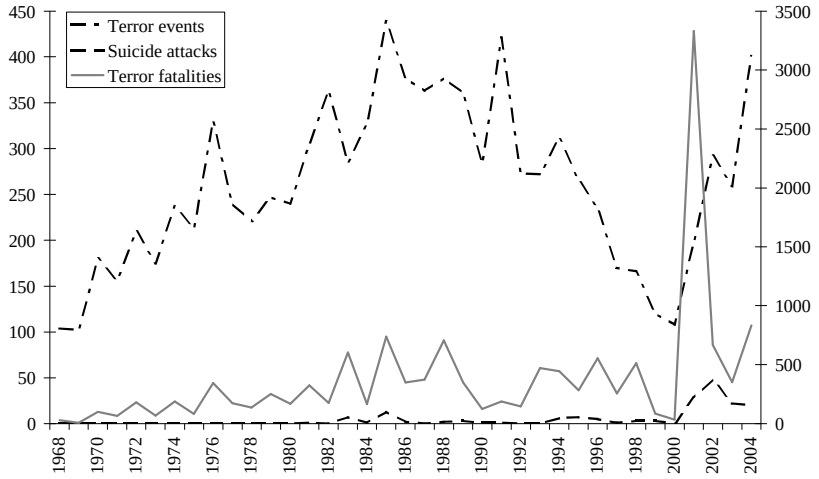
Figure 6.2 shows the variation over time for our different terror measures. It shows an upward trend of terror events in the 1970s and 1980s, followed by a steady decline in the 1990s and – again – a rise from 2001 onwards. Furthermore, it shows that the number of casualties and suicide attacks have been relatively stable over time, but have increased sharply since 2001. Besides, terrorism is also a world wide phenomenon. Out of 208 countries in our dataset, only in 27.4 percent (57) of the countries no terrorist attack was recorded over our sample period. Whereas large part of the world did face some terror, however, most attacks have happened in only a few countries. That is, the top ten countries that had most terrorist attacks account for 48.7 percent of all terrorist attacks in the world.[10]

We use an extensive set of control variables in our model. The selection of these variables is based on Grofman and van Roozendaal (1997). Before discussing these variables, it is important to note that all our model specifications contain fixed country effects. This implies that we need not to include variables that have no variation over time as they are fully absorbed by the country fixed effects.

To proxy for the ideological differences within cabinets we include the polarization variable of the Database of Political Institutions (Beck et al., 2001) in our regressions. This variable indicates to which extent government parties have the same 'political color.' From the same data source, we include two proxies for political coherence. That is we include a fractionalization variable (i.e., the probability that two randomly drawn members of the cabinet are from the same political party) and a variable that measures the number of years that the largest party is part of the cabinet. The latter is a proxy for the stability of coalitions.

[10]The top ten countries are (number of events in parenthesis): Israel (818), Lebanon (608), France (537), Germany (471), United States (446), Greece (403), Colombia (379), Iraq (357), Turkey (351) and Peru (326). The total number of recorded events is 9,624.

Figure 6.2: Transnational terror events and fatalities



Notes: The figure visualizes our measures of terror from 1968 to 2004. The left hand scale quantifies the number of transnational terror events and the number of transnational suicide events. The right hand scale measures fatalities caused by transnational terror attacks.

We include two variables that proxy for the political regime. That is, we include the Polity IV score (Marshall and Jaggers, 2000) as a measure for democracy.[11] The reason is that cabinets in more democratic settings are more likely to be held accountable by the electorate than cabinets installed by a dictator. A different reason to include this variable is that the polity variable is based on criteria that relate to the legislative organization of elections such as the constraints that are put on the chief executive (political leaders). In addition to the inclusion of a democracy variable, we include the number of years the political regime is in place (Marshall and Jaggers, 2000). We believe that cabinets in infant political systems are more likely to fall than cabinets in more mature systems as new political systems may not have much experience with electoral processes.

[11]We have run several models using alternative measures for democracy such as the measure by Przeworski et al. (2000) and Vanhanen (2000). It turns out that the results are robust for these alternative measures. All results are available on request.

In accordance with the literature, we also include variables related to the economic situation in our model. More specifically, we include the inflation rate as well as the level and the growth rate of GDP per capita. To ensure that the causality runs from the economic situation to cabinet changes and not vice versa, we use the lag of these variables.[12]

Finally, we include other 'critical' events in the model as they are a priori as likely as terrorism to affect cabinet duration. These variables are: the number of mass (non-violent) demonstrations, the number of strikes and the number of riots. These three variables are taken from Databanks International (2005) and reflect mass civil protest (see Jong-A-Pin, 2006). We use political violence variables like the number of purges the number of guerrilla warfare attacks (both taken from Databanks International, 2005) as well as a dummy variable that indicates the presence of a civil war (Gleditsch et al., 2002). Furthermore, we include different 'crisis' variables, i.e., the number of major government crises (Databanks International, 2005), the presence of a banking crisis (Caprio and Klingebiel, 1999) and the presence of a currency crisis (Dreher et al., 2006b). All variables, their exact definition and sources are presented in Table 6.1.

Table 6.1: Variables – definitions and sources

Variable	**Definition**	**Source**
Cabinet change	Replacement of the premier minister and/or the replacement of at least half of the ministers in the cabinet.	Databanks International (2005)
Terror events	Number of terror events per country and year.	MIPT Terrorism Knowledge Base
Suicide attacks	Number of terror events involving suicide attackers.	MIPT
Assassinations	Any politically motivated murder or attempted murder of a high government official or politician.	Databanks International (2005)
Terror events w/o 0	Number of severe terror events. Excluding events in which neither persons were killed nor injured.	MIPT
Sum of fatalities	Number of persons killed by terrorist attacks.	MIPT
Fatalities per event	Number of persons killed divided by the number of terror events.	MIPT
Terror dummy	Dummy variable taking on the value 1 if at least one transnational terror event occurred.	MIPT

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[12]The results, however, remain unchanged we use the contemporaneous values in the analysis.

Variable	Definition	Source
Major government crises	Any rapidly developing situation that threatens to bring the downfall of the present regime, excluding situations of revolt aimed at such overthrow.	Databanks International (2005)
Currency crisis	Dummy variable, 1 if currency index value exceeds mean by one standard deviation.	Dreher et al. (2006b)
Banking crisis	Dummy variable, 1 if in a period of systemic banking crisis.	Caprio and Klingebiel (1999)
Democracy	Polity IV score, represents the difference between a countries democracy and autocracy score. Ranges from -10 to 10, higher numbers indicate more democracy.	Marshall and Jaggers (2000)
Regime duration	The number of years that a political regime is in place.	Marshall and Jaggers (2000)
Economic growth per capita	Annual percentage growth rate of GDP per capita based on constant local currency.	World Bank (2006)
GDP per capita	GDP (in constant 2000 US$) divided by midyear population.	World Bank (2006)
Inflation	GDP deflator.	World Bank (2006)
Civil war	Dummy variable, 1 if at least 1,000 battle related deaths per year in a conflict between the government of a state and internal opposition groups without foreign intervention.	Gleditsch et al. (2002)
Purges	Number of systematic repressions (or eliminations) by jailing or execution of political opposition within the rank of the regime or the opposition.	Databanks International (2005)
Guerilla warfare	Any armed activity, sabotage, or bombings carried on by independent bands of citizens or irregular forces and aimed at the overthrow of the present regime.	Databanks International (2005)
Riots	Any violent demonstration or clash of more than 100 citizens involving the use of physical force.	Databanks International (2005)
Strikes	Any strike of 1,000 or more industrial or service workers that involves more than one employer and that is aimed at national government policies or authority.	Databanks International (2005)
Demonstrations	Any peaceful public gathering of at least 100 people for the primary purpose of displaying or voicing their opposition to government policies or authority, excluding demonstrations of a distinctly anti-foreign nature.	Databanks International (2005)

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Variable	Definition	Source
Political fractionalization	The probability that two deputies picked at random from the legislature will be of different parties.	Beck et al. (2001)
Polarization	Maximum polarization between the executive party and the four principle parties of the legislature.	Beck et al. (2001)
Years of party in government	Number of years that the party of the chief executive has been in office.	Beck et al. (2001)

6.4 Empirical Results

Table 6.2 shows our estimation results. As our dataset is unbalanced, we examine in columns 1-6 the impact of terrorism on cabinet dissolution in the presence of different explanatory variables. We have clustered the explanatory variables in different groups and as such the columns show whether the effect of terrorism is robust for the inclusion of variables from this particular group. We distinguish between variables representing economic and political crisis, institutional variables, economic variables, indicators of political violence, indicators reflecting mass civil protest and variables measuring attributes of the incumbent government. It can be seen that no matter what control group is included in the model, terrorism is always statistically significant at the one percent level. Furthermore, we find that the coefficient of the terrorism variable is hardly affected by the different sets of control variables.

Concerning the other explanatory variables, all variables except inflation are of the expected sign. Here, we briefly comment on their significance. First, we find that the majority of these variables is significant. In column 1, we find that both major government crises and banking crises increase the probability of cabinet dissolution. Currency crises, however, are not related to cabinet dissolution. In column 2, we find that both institutional variables are highly significant and of the expected sign: cabinet changes are more likely in more democratic countries while they occur less frequently in stable political systems. In the group of economic indicators, we find that low economic growth in particular is related to cabinet termination. Inflation is also marginally significant. The negative sign might be a little bit surprising as this means that inflation decreases

Table 6.2: Results conditional fixed effects logit – dependent variable: cabinet change

Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Terror events (log)	0.173	0.184	0.223	0.219	0.240	0.196	0.111
	(2.68)***	(3.30)***	(3.85)***	(4.01)***	(4.47)***	(2.72)***	(1.78)*
Major government crises	0.957						0.629
	(9.80)***						(7.66)***
Currency crisis	0.080						
	(0.72)						
Banking crisis	0.278						
	(2.44)**						
Democracy		0.038					0.047
		(4.78)***					(5.19)***
Regime duration		-0.013					-0.009
		(3.76)***					(2.45)**
Lagged economic growth			-0.015				-0.013
per capita			(2.79)***				(2.18)**
Lagged inflation			-9.3E-05				-1.1E-04
			(1.66)*				(1.96)*
Lagged GDP per capita (log)			-0.186				
			(1.44)				
Civil War				0.589			0.603
				(3.55)***			(3.23)***
Purges (log)				0.416			0.565
				(2.71)***			(2.93)***
Guerilla warfare (log)				0.269			
				(2.16)**			
Riots (log)					0.239		0.312
					(2.62)***		(3.56)***
Strikes (log)					0.058		
					(0.45)		
Demonstrations (log)					0.328		
					(4.09)***		
Political fractionalization						1.270	
						(5.05)***	
Polarization						0.008	
						(0.10)	
Years of party in government						0.001	
						(0.25)	
Observations	4,246	4,784	4,789	5,344	5,835	3,170	4,099
Countries	175	157	175	170	191	152	152
Pseudo R^2	0.04	0.01	0.01	0.01	0.02	0.02	0.05

Notes: The estimation technique used is conditional fixed effect logit. The Hausman test for specification rejects a random effects model for all specifications at the 5%-significance level. All specifications include the years since the last cabinet change and three temporal splines to account for duration dependence in the data.

*/**/*** indicates significance at the 10/5/1-% level; absolute value of z-statistics is given in parentheses.

the probability of cabinet terminations. This might be driven by a short-run Phillips curve effect in developing countries. In any case, the effect is very small. In columns 4 and 5, all indicators for political violence and mass civil protest, except the number of strikes, are significant. Out of the group of variables reflecting attributes of the incumbent government, only the degree of fractionalization turns out to be significant.

As we have included only one control group in each specification, the results in columns 1-5 could be biased due to omitted variables. Therefore, we also follow a different modeling strategy. That is, we start with the general unrestricted model including all explanatory variables simultaneously. Next, we drop the least significant variable from the regression and estimate the model again. We repeat this so-called general-to-specific procedure until only significant variables remain. Again, most of the covariates that are significant in columns 1-5 end up in the final model specification. Only the number of demonstrations, the presence of guerrilla warfare and the fractionalization measure are not robust. Our measure of terrorism is significant at the ten percent level and the magnitude of the coefficient is smaller than in previous specifications.

To test the robustness of our results further, we replace our terrorism variable for various alternative terrorism measures. We use the general-to-specific model specification of column 7 as our benchmark specification. The results are shown in Table 6.3. First, we include the lagged number of terrorist attacks as it may take some time before a cabinet reacts to a critical event.[13] This variable is significant at the five percent level. Next, we include the number of suicide attacks and the number of political assassinations as especially these events are likely to affect the public opinion. We find that both terror measures are highly significant. To examine whether the intensity of terror influences the duration of cabinets, we include different indicators related to the number of victims. We include the number of terror events with at least one person harmed, the sum of all fatalities due to terrorism in a particular year, the average number of fatalities per event and a dummy indicating the presence of terror. In our view, a remarkable pattern stands out. That is, the average number of fatalities and the presence of terror are insignificant, while all the other variables are significant. This lends support to the

[13]Including lagged values also mitigates potential endogeneity problems.

view that it is not the presence of terrorism as such that affects the duration of cabinets. However, it is the severity of terrorism that matters. In particular, terrorist attacks that involve relatively many casualties are likely to affect the probability that the cabinet dissolves.

To get a grasp at the magnitude of our estimated relationships we calculate the marginal effects for our terror measures. We focus on specification 7 of Table 6.2 and Table 6.3. The results are displayed in Table 6.4.[14] Given our logarithmic set-up one cannot directly see the effect of one additional terror attack. We therefore calculate the effect of a one unit change of the respective terror measure.[15] The result is that, at the mean of all variables, one additional terror attack increases the likelihood of a cabinet change by one percent at a significance level of ten percent. Using lagged terror events increases both the magnitude of the effect and its significance level. This implies that it might indeed need some time for the political process to react to terror. Turning to suicide attacks we can confirm the findings of chapter 5. Again, we find that terrorists who voluntarily give their life for their cause trigger bigger reactions. Here, we find that an additional suicide attack increases the probability of a cabinet dissolution by almost 15 percent. When turning to the next three measures, all of which proxy the severity of terror attacks, we find that more severe attacks increase the likelihood of a cabinet change. The biggest effect stems from the political assassination variable. But the same conclusion can be drawn from more severe terror attacks (the marginal effect is doubled as compared to taking all terror attacks into account) and the number of people killed by terrorist activity. The sole existence of terror does not matter, however. This is demonstrated by the insignificance of fatalities per event and a simple terror dummy.

In order to further test the robustness of our findings we re-run our analysis using different sub-samples. The results of these additional tests of robustness are suppressed for readability reasons but are available upon request. First we separated OECD and non-OECD countries. We find that OECD countries do not dissolve their cabinets

[14] In order to calculate the marginal effect all variables are assigned their mean value and the fixed effects are set to zero.

[15] We do so by calculating the marginal effect of the change in logarithms and transfer this effect into the effect of a one unit change of the underlying variable.

Table 6.3: Results alternative terror measures – dependent variable: cabinet change

Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Major government crises	0.637	0.640	0.618	0.628	0.630	0.634	0.635
	(7.74)***	(7.77)***	(7.49)***	(7.63)***	(7.66)***	(7.71)***	(7.72)***
Democracy	0.047	0.047	0.046	0.047	0.047	0.047	0.048
	(5.16)***	(5.10)***	(5.04)***	(5.18)***	(5.15)***	(5.19)***	(5.22)***
Regime duration	-0.009	-0.010	-0.009	-0.009	-0.010	-0.010	-0.010
	(2.40)**	(2.74)***	(2.48)**	(2.46)**	(2.57)**	(2.58)***	(2.56)**
Lagged economic growth	-0.013	-0.014	-0.013	-0.013	-0.014	-0.014	-0.014
per capita	(2.14)**	(2.20)**	(2.08)**	(2.18)**	(2.22)**	(2.22)**	(2.21)**
Lagged inflation	-1.2E-04	-1.1E-04	-1.1E-04	-1.1E-04	-1.1E-04	-1.1E-04	-1.1E-04
	(2.01)**	(1.93)*	(1.99)**	(1.93)*	(1.93)*	(1.93)*	(1.94)*
Civil War	0.584	0.643	0.569	0.602	0.605	0.629	0.640
	(3.13)***	(3.48)***	(3.03)***	(3.23)***	(3.25)***	(3.39)***	(3.45)***
Purges (log)	0.558	0.567	0.536	0.567	0.568	0.567	0.569
	(2.89)***	(2.94)***	(2.75)***	(2.94)***	(2.95)***	(2.94)***	(2.95)***
Riots (log)	0.314	0.328	0.284	0.316	0.321	0.323	0.323
	(3.59)***	(3.75)***	(3.22)***	(3.62)***	(3.68)***	(3.70)***	(3.70)***
Terror events (log), t-1	0.166						
	(2.66)***						
Suicide attacks (log)		1.038					
		(2.47)**					
Assassinations (log)			0.388				
			(3.43)***				
Terror events w/o 0 (log)				0.204			
				(2.35)**			
Sum of fatalities (log)					0.113		
					(2.10)**		
Fatalities per event (log)						0.088	
						(1.10)	
Terror dummy							0.006
							(0.06)
Observations	4,099	4,099	4,099	4,099	4,099	4,099	4,099
Countries	152	152	152	152	152	152	152
Pseudo R^2	0.05	0.05	0.05	0.05	0.05	0.05	0.04

Notes: The estimation technique used is conditional fixed effect logit. The Hausman test for specification rejects a random effects model for all specifications at the 5%-significance level. All specifications include the years since the last cabinet change and three temporal splines to account for duration dependence in the data. 'w/o 0' indicates that all terror events in which neither fatalities nor injuries occurred are excluded.

*/**/*** indicates significance at the 10/5/1-% level; absolute value of z-statistics is given in parentheses.

Table 6.4: Marginal effects of terror – dependent variable: cabinet change

Variable	Marginal Effect	p-value	Mean
Terror events	0.010	0.073	0.497
Terror events, t-1	0.014	0.007	0.496
Suicide attacks	0.149	0.013	0.007
Assassinations	0.050	0.001	0.114
Terror events w/o 0	0.023	0.018	0.234
Sum of fatalities	0.012	0.035	0.262
Fatalities per event	0.011	0.270	0.131
Terror dummy	0.001	0.953	0.114

Notes: The table contains the marginal effects of terror based on a one unit change of the respective measure. It utilizes the conditional fixed effects logit regressions of specification (7) of Table 6.2 (Terror events) and Table 6.3. The marginal effect is calculated for the value of each independent variable set to its mean and the fixed effect set to zero. The column 'Mean' reports the mean of the respective terror variable that is used to calculate the marginal effect. 'w/o 0' indicates that all terror events in which neither fatalities nor injuries occurred are excluded.

as a result of terror. Our findings for the overall sample are mainly driven by the non-OECD countries. Digging deeper in this matter we distinguish between political stable countries and unstable countries. We measure political stability by changes in the Przeworski et al. (2000) democracy dummy. We find that stable autocracies, i.e., countries that never score the value of one on the democracy dummy, do not react to terror. This means that rulers of such countries obviously have obtained the knowledge to withstand all forms of uprising. As our final test of robustness we turn to domestic rather than transnational terror. This has the disadvantage of a very short time span (from 1998 onwards). Keeping that caveat in mind we find that reactions to domestic terror are more pronounced. Domestic terror is most likely caused by factors specific to each country. It is probable that discontent with the political landscape is among these factors.

6.5 Conclusions

This chapter analyzes the impact of terrorism on cabinet durability for a panel of more than 150 countries for the period between 1968 and 2002. We employ conditional fixed

effect logistic regression to analyze the factors influencing the probability of a cabinet change to occur. We find that terrorism and political violence in general greatly undermines the stability of governments. This implies that governments are highly aware of critical events that change the political force field and that they react to these events by reforming the cabinet. Besides terrorism and political violence we find that economic success in the past significantly enforce the position of governments in office. Furthermore, the institutional setup of the political system greatly affects cabinet stability. We test the sensitivity of our findings by replicating our analysis using different sub-samples. We find that terror provokes cabinet dissolutions mainly in non-OECD countries and in political unstable societies.

Given that our methodology allows for the analysis of time varying events, a whole new field of possible critical events may be studied. One could think of economic and social crises and so on. Another interesting field of research from a political perspective would be to analyze whether the cabinets that follow those ousted out of office more closely follow government policies that abide to the terrorists wishes or that a terrorist attack literally backfires.

Chapter 7

Terrorism and Electoral Accountability: One Strike, You're Out!

7.1 Introduction

After examining the effect of transnational terror on cabinet durability in general, we now turn our focus to a more specific question: does terror influence election outcomes? The motivation for this analysis is taken from the recent past. On March 11, 2004 several bombs exploded in four commuter trains around Madrid killing almost two-hundred people. Only three days later general elections were held in which the Aznar administration was replaced. While both events could have been driven separately by the Spanish participation in the Iraq war, we argue that there is a systematic linkage between terrorism and electoral accountability. Barro (1973) and Ferejohn (1986) show that the electorate holds the government accountable for the provision of public goods. As national security is arguably one of the most important public goods, terrorist attacks

This chapter is an adapted version of Gassebner et al. (2007d).

are likely to affect the government's re-election probability.[1] We examine the relationship between terrorism and electoral accountability using a conditional fixed effects logit model and a dataset containing more than 800 elections in 115 countries.

The chapter proceeds as follows: the ensuing section derives our hypothesis. Section 7.3 provides our estimation results. Section 7.4 concludes.

7.2 Theoretical Background

The political economy literature provides two different entries to investigate the effect of terrorism on the probability of government replacement. These are the literature on electoral accountability originating in the work of Barro (1973) and the game theoretical approach on coalition termination by Lupia and Strøm (1995).

The electoral accountability approach of Barro (1973) and Ferejohn (1986) is based on the idea that incumbent governments face a trade-off between rent extraction and public goods provision. In order to obtain enough votes at the next election, the incumbent needs to provide a minimal amount of public goods to appear competent in the eyes of the electorate. If this minimal amount of public goods is not provided, the incumbent is judged to be incompetent and will be replaced at the next election. Regarding terrorism, the electorate generally does not observe how much public good is provided. That is, voters do not, or at least not completely, observe the counter terrorism activities of the government. However, they do observe the number of terror events that materialize. As the number of terror events is believed to be decreasing in both the amount of resources spent by the government on terror protection and the competence of the government, the amount of terrorism provides a signal about the competence of the incumbent. If the electorate believes that the level of terror under the current government is too high (relative to the expected level of terror under a different government), the incumbent government is more likely to be ousted from office.

[1] While election outcomes and cabinet changes are mainly of interest to political scientists, recent studies find evidence that changes in government affect macroeconomic outcomes such as economic growth (Jones and Olken, 2005) and inflation (Aisen and Veiga, 2006).

The game theoretical approach of Lupia and Strøm (1995) explains why single events might cause the fall of a cabinet. Their model starts from the premise that three parties have bargained about a cabinet coalition. After the government was formed, an unexpected event (such as a terrorist attack) occurs that alters the power distribution within the coalition through a public opinion shock. After the shock it may become beneficial for one of the coalition members to opt out of the coalition to enforce early elections. As a consequence of the public opinion shock, it is very likely that a different coalition will be installed after the elections.
Both theoretical approaches lead to the following hypothesis:

Hypothesis 7.1. *Terrorism increases the probability that the incumbent government is replaced at the next election.*

7.3 Empirical Analysis

We examine the effect of terrorism on the probability of government replacement using a conditional fixed effects logit model as proposed by Chamberlain (1980). We focus on more than 800 elections in about 115 countries over the period 1969-2002. Our terrorism indicators are from the Memorial Institute for the Prevention of Terrorism (MIPT) Terrorism Knowledge Base (2006).[2] Our main terrorism indicator is the yearly number of total terror events in a country. As a test for robustness, we also present results using alternative indicators based on the number of casualties as well as various dummy variables. The pairwise correlation coefficients between all used indicators are shown in Table 7.1.

Our regression model is as follows:

$$c_{it} = \alpha_i + \delta_t + \beta T_{it} + \gamma X_{it} + \epsilon_{it} \tag{7.1}$$

[2]The MIPT defines an act of terror as "violence calculated to create an atmosphere of fear and alarm." Terror is used to discourage others from acting at their free will. The motives for engaging in terror are political while the acts themselves are generally conducted in a way that will achieve maximum publicity, mainly by attacking civilians. Moreover, terrorist acts are mostly intended to create more than immediate physical damage – a long-time situation of fear and intimidation. See http://www.tkb.org/.

Table 7.1: Correlation matrix terror indicators

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
(1)	Transnational terror event	1.00									
(2)	Transnational terror event, t-1	0.75	1.00								
(3)	Transnational terror event dummy	0.83	0.58	1.00							
(4)	Transnational terror event w/o 0	0.84	0.65	0.63	1.00						
(5)	Transnational terror event w/o 0 dummy	0.76	0.55	0.73	0.87	1.00					
(6)	Fatal transnational terror event dummy	0.30	0.18	0.58	-0.12	-0.13	1.00				
(7)	Transnational terror event dummy, no bodily harm	0.69	0.52	0.63	0.82	0.86	-0.11	1.00			
(8)	Sum of fatalities	0.65	0.49	0.51	0.81	0.70	-0.09	0.81	1.00		
(9)	Fatalities per event	0.37	0.24	0.43	0.52	0.59	-0.08	0.68	0.86	1.00	
(10)	Fatalities per event w/o 0	0.12	0.12	0.12	0.14	0.13	0.02	0.13	0.15	0.12	1.00

Notes: Correlation coefficients are based on all available observations (N=7488). We use the logarithm of the variables that are based on events to mitigate the effect of outlying observations. 'w/o 0' indicates that all terror events without fatalities or injuries are excluded. Terror event dummy no bodily harm is a dummy taking on the value of 1 if at least one terror attack occurred but no person was killed or injured and 0 otherwise. Fatal terror event dummy is a dummy taking on the value of 1 if at least one fatal terror attack occurred and zero otherwise.

where c_{it} is a binary variable equal to 1 if the cabinet of country i is replaced in an election year t and 0 if the cabinet of country i in year t remains in office after elections.[3] Data on elections and cabinet changes are taken from Databanks International (2005). α_i is a country specific effect that accounts for all characteristics specific to country i and δ_t is a time fixed effect that accounts for all variation common to year t.[4] T_{it} denotes our terror measure. X_{it} is a vector of control variables and ϵ_{it} is an error term. We control for a broad set of variables, which have been suggested to affect cabinet change (see Grofman and van Roozendaal, 1997, for an extensive overview). These variables can be categorized as follows: (i) macroeconomic variables, (ii) crises variables (both economic and political), (iii) political regime variables, (iv) indicators of political violence and (v) variables capturing mass civil protest.[5]

Our estimation results are reported in Table 7.2. The terror indicator is included in each regression. Columns (1)-(5) contain the estimates when the different sets of control variables are included. In column (6) all control variables are included simultaneously. As this specification decreases our sample and many of the controls are insignificant, we subject our model to a general-to-specific procedure in which we dropped the least significant variable at every stage of the test-procedure until only significant variables remain. The result of this analysis is displayed in column (7). In the remainder of our analysis we use this parsimonious model specification.[6]

Our main finding is that the number of terror events is significant at the 5%-level in all specifications. The positive sign indicates that the presence of terror increases

[3]A cabinet change is defined as the replacement of the premier and/or the replacement of at least 50 percent of the ministers in cabinet.

[4]Using Hausman tests, we checked for the inclusion of common effects as well as random effects. Both types of effects are rejected at the 5 percent significance level for all specifications. The presence of time effects are examined using Wald tests. The null-hypothesis of no time effects is rejected for all specifications.

[5]The economic variables are taken from the World Bank (2006). The data on critical political events are from Databanks International (2005). The political regime variables are from the Polity IV dataset of Marshall and Jaggers (2002). The data on banking crises are from Caprio and Klingebiel (1999) and currency crises data are from Dreher et al. (2006b).

[6]We also conducted the extreme bounds analysis of Sala-i-Martin (1997), in which the same variables turn out to be robustly related to cabinet changes. The results of both test procedures are available upon request.

Table 7.2: Results conditional logit – dependent variable: cabinet change

Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Transnational terror	0.434	0.476	0.366	0.409	0.474	0.389	0.439
events (log)	(2.68)***	(2.40)**	(2.32)**	(2.64)***	(3.16)***	(1.68)*	(2.75)***
Lagged inflation	7.1E-05					0.002	
	(0.25)					(1.35)	
Lagged economic growth	-0.029					-0.016	
p.c.	(1.55)					(0.50)	
Lagged GDP p.c.	-0.620					0.226	
	(1.59)					(0.31)	
Major government crises		0.539				0.604	
		(1.87)*				(1.76)*	
Currency crises		0.350				0.304	
		(0.85)				(0.62)	
Banking crises		0.374				0.517	
		(0.96)				(1.04)	
Democracy			0.091			0.136	0.120
			(2.91)***			(2.79)***	(4.41)***
Regime duration			-0.019			-0.015	
			(1.72)*			(0.91)	
Purges (log)				1.468		2.371	1.767
				(2.38)**		(1.86)*	(2.89)***
Civil War				0.940		1.118	1.075
				(1.69)*		(1.37)	(1.96)**
Assassinations (log)				0.393		-0.098	
				(1.24)		(0.23)	
Guerrilla warfare (log)				0.451		0.504	
				(1.04)		(0.62)	
Riots (log)					-0.267	-0.760	
					(1.04)	(1.99)**	
Demonstrations (log)					0.290	0.482	
					(1.29)	(1.47)	
Strikes (log)					0.209	-0.026	
					(0.53)	(0.05)	
Observations	824	624	814	930	1,017	479	842
Countries	120	104	113	125	140	78	115
Pseudo R^2	0.12	0.14	0.15	0.14	0.11	0.24	0.17

Note: The results are based on conditional fixed effect logit regressions. Time dummies are included in all specifications. We use the logarithm of the variables that are based on events as well as for GDP per capita to mitigate the effect of outlying observations. Due to data availability the number of countries per specification differs.

*/**/*** indicates significance at the 10/5/1-% level; absolute value of z-statistics is given in parentheses.

the probability of a cabinet change at the election. Furthermore, it can be seen that the estimated coefficient of our terror measure is hardly affected by the different sets of control variables.

Concerning our control variables, we find that only the level of democracy, the number of purges and the presence of civil war are robustly related to electoral change. All three variables increase the likelihood of a cabinet change. The duration of the political regime and major government crises only marginally affect the probability of a change of the cabinet.

Table 7.3 shows the results when each of the other terror measures is included in specification (7) of Table 7.2. Here, we also report the marginal effect of each indicator on the likelihood of a cabinet change after the election. First, we include lagged terror events to examine our imposed direction of causality.[7] Second, we include a dummy equal to 1 if there was at least one act of terror and 0 otherwise. Although inclusion of this indicator comes at the cost of not utilizing all available information, it gives the estimated marginal effect a clear interpretation.[8] That is, the presence of at least one terror event increases the likelihood of a cabinet change after the election with 12.0 percent. Third, we include the number of terror events again, but exclude cases without casualties or injuries, since such terror events are expected to have less impact on the public opinion. Similarly, we construct three dummy variables to differentiate between the severity of different terrorist attacks. We make a distinction between events with at least one injury or casualty, events without any injuries or casualties and events with at least one casualty. The estimated marginal effects (12.4 percent, 3.8 percent and 18.0 percent, respectively) show that the probability of a cabinet change depends on the severity of the terrorist attack. While terrorist attacks without casualties or injuries hardly affect the probability of a cabinet change, the impact drastically increases when only terrorist attacks with casualties (and injuries) are considered. Finally, we further test the robustness of our results using several terror measures based on the number

[7]We also examined terror events at $t-2$ and find that they do not influence the election outcome. This result suggests that the electorate is short-sighted with respect to its voting behavior.

[8]This also mitigates the effect of outliers in the terror variable.

Table 7.3: Results tests of robustness – dependent variable: cabinet change

Variable	coefficient	z-stat	marginal effect
Transnational terror events	0.439	2.75***	0.109
Transnational terror events, t-1	0.395	2.17**	0.097
Transnational terror event dummy	0.489	2.13**	0.120
Transnational terror events w/o 0	0.399	1.66*	0.097
Transnational terror event w/o 0 dummy	0.501	1.85*	0.124
Transnational terror event dummy, no bodily harm	0.153	0.61	0.038
Fatal transnational terror event dummy	0.728	2.38**	0.180
Sum of fatalities	0.452	2.43**	0.111
Fatalities per event	0.538	1.96**	0.134
Fatalities per event w/o 0	0.547	2.26**	0.137

Note: The table contains point estimates and marginal effects of specification (7) of Table 6.2. The marginal effects are calculated at the mean of all explanatory variables with the fixed effects set to 0. For all indicators (apart from the dummy variables) we use the logarithm to mitigate the effect of outlying observations. 'w/o 0' indicates that all terror events without fatalities or injuries are excluded. 'No bodily harm' is a dummy taking on the value of 1 if at least one terror attack occurred but no person was killed or injured and 0 otherwise. 'Fatal transnational terror event dummy' is a dummy taking on the value of 1 if at least one fatal terror attack occurred and 0 otherwise.

*/**/*** indicates significance at the 10/5/1-% level.

of casualties. We include the sum of all casualties per year, the number of casualties per event and the casualties at the median event. The alternative measures confirm our findings above. Terrorism significantly increases the likelihood of a cabinet change, while this likelihood is positively affected by the severity of the terrorist attack.

7.4 Conclusions

In this paper we analyze the impact of terrorism on the probability of government replacement. We find strong evidence that terrorist attacks increase the probability that the cabinet will be replaced after an election. Furthermore, we find that the magnitude of this effect increases with the severity of a terrorist attack. Our empirical evidence supports both the electoral accountability model of Barro (1973) as well as the Lupia and Strøm (1995) critical events model. As recent studies find that government changes

affect macroeconomic variables such as economic growth (Jones and Olken, 2005) and inflation (Aisen and Veiga, 2006), our results indicate that terrorism indirectly influences economic outcomes.

Chapter 8

Does political proximity to the U.S. cause terror?

8.1 Introduction

After starting this work with the determinants of democracy we conclude the circle by analyzing the determinants of terror. America is in the midst of a "War on Terror." The determinants of terror, however, have not yet been fully understood. Economists, in particular, started researching this topic only very recently. Still, first evidence exists.[1] Absence of political rights, high inequality and fractionalization have been identified as causes of terror, while low GDP per capita does not seem to determine terror (Krueger and Malečková, 2003; Abadie, 2006; Piazza, 2006).

However, the literature so far relied on cross-sectional analysis only. Results are thus potentially driven by fixed factors that correlate with the variables of interest. This is the first gap in the literature that this paper fills. For the first time, the relevance of the variables most prominently suggested to determine terror is analyzed in a panel context

This chapter is an adapted version of Dreher and Gassebner (2007).

[1]Note that most previous studies analyze the sources of terrorism and thereby focus on the country where terror originates. A recent exception is Piazza (2006). Frey et al. (2007) survey the literature on the economic effects of terrorism.

– employing a new database on terrorism drawing from a multitude of sources. In doing so, we focus on the targets of transnational events of terror.

As our second – and main – contribution, we examine the effects of a country's political proximity to the U.S. on the frequency and severity of terror in that country. We employ the standard measure of political proximity, the share of roll call votes cast in line with the U.S. in the United Nations General Assembly.[2] To anticipate our results, countries voting in line with the U.S. are indeed victims of more and deadlier terror.

The next section motivates our main hypothesis. Section 8.3 presents the data and estimation method, while the results are shown in the final section.

8.2 Hypothesis

"What worries people around the world above all else is living in a world shaped and dominated by one country – the United States" (Zakaria 2004). While U.S. dominance seems to alienate people across the globe, most Western countries are still on good terms with the U.S., and so is the majority of their citizens. To the contrary, in some countries with mostly Muslim population, terror groups have emerged, aiming at destroying U.S. culture and dominance. However, not only the U.S. itself became the target of terrorism – its allies have also been hit by severe attacks. Arguably, terror in these countries might also indirectly target U.S. dominance and culture. Friends of the U.S. might thus be more prone to terror than other countries, all else equal.

As one example, consider America's recent "War on Terror" in Iraq. Following their participation, the UK and Spain – among the closest allies of the U.S. – suffered major terror attacks themselves. These attacks can be linked directly to these countries' support of the U.S. in Iraq: the two London bombings in July 2005 and the Madrid bombing in March 2004 were declared to be retaliation for participation in the U.S.-led war.

We expect this pattern to hold more generally. Our considerations imply:

Hypothesis 8.1. *Political proximity to the U.S. increases terror.*

[2]See, e.g., Thacker (1999), Barro and Lee (2005), Kilby (2006), Dreher and Jensen (2007).

8.3 Data and Method

We employ panel data for 116 countries over the period 1975-2001.[3] Since some of the data are not available for all countries or years, the panel data are unbalanced and the number of observations depends on the choice of explanatory variables. As the data on terror events are strongly skewed to the right and display significant overdispersion we estimate our regressions employing the fixed effects Negative Binomial estimator.[4]

The equations take the following form:

$$terror_{jit} = \alpha + \beta_1 US_{it} + \beta_2' X_{i,t-1} + \lambda_t + \varepsilon_{it}, \tag{8.1}$$

where $terror_{jit}$ represents our jth measure of terror for country i in period t, and US_{it} measures political proximity to the United States. $X_{i,t-1}$ is the vector of (lagged) control variables, λ_t are fixed time effects, while ε_{it} represents the disturbance.

Next, we need to establish how to measure political proximity to the U.S. We follow the bulk of literature and employ data on voting coincidence in the UN General Assembly as provided by Voeten (2004). In particular, we follow Thacker (1999), coding votes in agreement with the U.S. as 1, votes in disagreement as 0, and abstentions or absences as 0.5. The resulting numbers are then divided by the total number of votes in each year. This results in a variable ranging from zero to one, with zero indicating total disagreement with the U.S., and one showing full agreement.

Our terror measures are taken from the MIPT Terrorism Knowledge Base, integrating data from various sources. For each country and year we employ the number of transnational terror events as our main variable of interest.[5] In addition, we use the

[3] Available data extends to 2002. However, due to the special effects of September 11, we exclude the year 2002 from the analysis. Estimating the full sample yields similar results. The exception is column 1 of Table 1 where the coefficient for the UN voting variable becomes marginally insignificant (p-value = 0.11).

[4] The results reported below are qualitatively unchanged when estimated with OLS, while the quantitative impact of UN voting on terror is substantially larger. Random effects Negative Binomial regressions also show similar results.

[5] A transnational terror event is an event in which either the attacker and/or the target is foreign. Including domestic events does not change the main results.

median as well as the average number of persons killed per attack in a given country and year, and the number of suicide attacks.[6]

Note that our analysis focuses on the targets of terrorism rather than its sources. In choosing our control variables, we thus mainly follow Piazza (2006) who also analyzes the targets of terror. We employ GDP per capita (measured in constant 2000 US$). On the one hand, richer countries are more attractive targets for terrorists, as terror creates more attention. On the other hand, richer countries have stronger police and intelligence agencies, potentially being able to prevent terror. The impact of GDP is thus not obvious a priori.

A second variable suggested to be important for terror is political freedom. The relation between political freedom and terrorism is also ambiguous a priori. Repressive states could foster terrorism (as foreign minorities might conceive terror as the only "effective communication device" against state repression) or be detrimental to it (as repressive states might be better able to suppress terrorism). In line with Piazza (2006), we include both the level of and changes in freedom.

Third, we include population size, as more potential victims promise to create more news in case of successful attack. Furthermore, the costs of state surveillance might rise with population size (Piazza 2006).

Finally, we include government fractionalization. According to Piazza (2006), the number of parties in power to some extent proxies "social cleavage," potentially giving rise to terror. Conflicting political interests might in particular extend to the foreign based population.

Data for per capita GDP and population are taken from World Bank (2006). Government fractionalization is from Beck et al. (2001) and measures the probability that two randomly drawn members from among the government are of different parties. The level and change in political freedom are based on the average of political rights and civil liberties from Freedom House (2005). We transform the original scales of both indices, so that higher values represent more liberty, on a scale from 1 to 7.

[6]Territories are assigned to the country formally governing the territory. Kashmir and the Persian Gulf are excluded as it is not clear to which country they should be assigned.

8.4 Results

Table 8.1 reports first results for the number of transnational terror events. Overall, our results mirror those reported in Piazza (2006). The number of terror events increases with greater government fractionalization, at the one percent level of significance. At the ten percent level, countries with larger population and decreasing political freedom are struck by terror more frequently. GDP per capita and the level of freedom have no significant impact on terror. With respect to our variable of primary interest, terror is more frequent with greater voting coincidence with the U.S. in the General Assembly. An increase in voting coincidence from zero to one implies approximately one additional terror attack.

Table 8.1: Results Negative Binomial estimation – dependent variable terror events

	(1)	**(2)**	**(3)**
GDP per capita, t-1	-7.50E-06	-6.98E-06	-7.17E-06
	(0.60)	(0.55)	(0.57)
Political freedom, t-1	0.018	0.022	0.013
	(0.49)	(0.59)	(0.36)
Political freedom, change	-0.116	-0.120	-0.119
	(1.65)*	(1.69)*	(1.69)*
Population, t-1	1.84E-09	1.92E-09	1.83E-09
	(1.86)*	(1.90)*	(1.87)*
Government fractionalization, t-1	0.433	0.436	0.424
	(2.98)***	(2.98)***	(2.92)***
Voting with U.S.	0.968	0.951	0.948
	(1.89)*	(1.84)*	(1.85)*
Economic growth, t-1		-0.009	
		(1.45)	
Population growth, t-1			-0.025
			(0.79)
Countries	116	116	116
Observations	2,263	2,250	2,262
Wald test (Prob>chi2)	0.00	0.00	0.00

Notes: Results in this table are derived using Negative Binomial fixed effects estimations. Annual year dummies are included in all regressions. The voting with U.S. variable measures the voting behavior in the UN General Assembly. It follows the definition of Thacker (1999), coding votes in agreement with the U.S. as 1, votes in disagreement as 0, and abstentions or absences as 0.5.
*/**/*** indicates significance at the 10/5/1-% level; absolute value of z-statistics is given in parentheses.

In columns 2 and 3, we test for the stability of our results by including two additional variables that have been suggested in Piazza (2006). Column 2 adds economic growth, while population growth is included in column 3. As the results show, the coefficient of voting coincidence remains virtually unchanged, while both variables are not significant at conventional levels.[7]

Table 8.2 replicates the analysis with our three alternative dependent variables. As can be seen, voting with the U.S. increases the average and the median number of people killed in a single terror attack at the one and, respectively, five percent level of significance. To the contrary, suicide attacks are not more frequent in countries voting in line with the U.S. (a result based on data for 17 countries only, however). According to the estimates, changing voting behavior from voting fully against to completely in line with the U.S. increases the average number of people killed by 3 and the number of fatalities in the median attack by 2.6.

8.5 Conclusions

In this chapter, we analyze determinants of terror. In particular we focus on the effect of political proximity to the U.S. Our results show that voting in line with the U.S. in the UN General Assembly is costly in terms of additional terror, both regarding frequency and severity of attacks. With respect to other determining factors our findings are in line with the existing literature. GDP per capita is not a good predictor of the occurrence of terror. More populous countries experience excess terror. Last but not lease, political factors play an important role in determining terror attacks. Political fractionalization and reforms that restrict political freedoms trigger more frequent and more severe terror attacks. This concludes the main part of this work. In the next chapter we summarize all contributions.

[7]As additional tests for robustness, we excluded observations with extreme values. Our results are qualitatively unchanged. We also tested for the endogeneity of UN voting to terror, using the number of new International Monetary Fund programs concluded in a certain country and year. While IMF programs have been shown to be highly correlated with UN voting (e.g., Thacker, 1999), they are not significantly related to terror. Note that there is no support for the endogeneity of UN General Assembly voting according to the Durbin-Wu-Hausman test.

Table 8.2: Results alternative terror measures – dependent variable terror

	(1) **Average Kills**	**(2)** **Median Kills**	**(3)** **Suicide Attacks**
GDP per capita, t-1	3.03E-05	2.96E-05	-3.91E-05
	(1.50)	(1.29)	(0.28)
Political freedom, t-1	0.075	0.027	0.949
	(1.47)	(0.40)	(1.18)
Political freedom, change	-0.255	-0.246	-0.605
	(2.21)**	(1.71)*	(0.87)
Population, t-1	1.80E-10	6.10E-10	-1.07E-08
	(0.50)	(1.38)	(1.11)
Government fractionalization, t-1	0.409	0.352	3.979
	(1.80)*	(1.21)	(2.47)**
Voting with U.S.	3.057	2.610	5.176
	(3.96)***	(2.54)**	(0.96)
Countries	86	79	17
Observations	1,762	1,588	319
Wald test (Prob>chi2)	0.00	0.00	0.00

Notes: Results in this table are derived using Negative Binomial fixed effects estimations. Annual year dummies are included in all regressions. The voting with U.S. variable measures the voting behavior in the UN General Assembly. It follows the definition of Thacker (1999), coding votes in agreement with the U.S. as 1, votes in disagreement as 0, and abstentions or absences as 0.5. The different dependent variables are presented in the table header.
*/**/*** indicates significance at the 10/5/1-% level; absolute value of z-statistics is given in parentheses.

Chapter 9

Conclusion

This final chapter briefly summarizes the findings of this work. This thesis analyzes two central concepts of political economy: democracy and terror. Our analysis focuses on a wide array of topics. We examine the driving forces behind both phenomena, we analyze their effects on the economy and, finally, we focus on their interrelationship. After concluding our analysis we can confirm the importance of studying both democracy and terror. We have expanded the knowledge of the political economy literature in several dimensions.

We find that predicting the emergence of democracy is not as straight forward as the abundant literature proposes. Only five variables out of 55 survive our extensive robustness analysis for the emergence of democracy while just four do so for the survival of democracy. The policy conclusion that can be drawn from our analysis is that if one wants to foster democratization processes, the attention should be turned to poor countries that have recently experienced a democratic regime change. We engage the dispute between modernization theorists, claiming GDP per capita to cause democratization, and the group of scholars spearheaded by Adam Przeworski from the New York University, objecting this finding. Our analysis strongly supports the latter group. Increases in wealth does increase the probability of remaining democratic, however.

Examining the effect of democracy on macroeconomic outcomes, we can confirm its relevance. Analyzing the influence of the political system on trade flows, we find

strong evidence for the detrimental effects of autocracies. We demonstrate this effect by deriving two new theoretical channels: an accountability channel and a bureaucracy channel. The former furthermore demonstrates the importance of holding political leaders accountable for their actions (e.g., via elections) while the latter shows how the political-institutional setup shapes human behavior which ultimately determines economic outcomes. Both theoretical channels are shown to be highly relevant empirically.

We also clarify the driving forces of a specific policy agenda. Exploring the determinants of environmental policy we find a potential "silver-lining" with respect to two phenomena which are present in most modern day developed economies: inequality and de-industrialization. Both turn out to be fostering more stringent environmental policies which in turn reduce pollution. Our finding is rooted in theoretical considerations of the policy formation process. Parties with more weight shape the policy outcome.

The interrelationship between democracy and terror is demonstrated in chapter 5 of this thesis. On the one hand, we find that democratic countries show great respect for human rights. Again, political accountability might be the key issue for this finding. On the other hand, we find that governments reduce the human rights of their citizens in response to terror attacks. In a way, they reduce parts of the very rights they want to protect in the first place. However, this response is quantitatively relatively minor. Furthermore, the effect is not present for empowerment rights such as freedom of movement, religion, etc.

In chapters 6 and 7 we strengthen the examination of the interrelation of democracy and terror even further. We analyze whether and to what extent terrorism affects one of the core actors of most societies: cabinets. In particular we focus on the question whether terror attacks affect the durability of cabinets. We find evidence that terror acts increase instability. In the presence of terror, cabinets are more frequently dissolved. We find a moderate effect for a general setup. However, the truly noteworthy finding is achieved for election years. Again we find support for the electoral accountability hypothesis: voters hold politicians accountable for not providing them the most basic public good, namely security. As the presence of terror gives rise to the speculation that incumbents are incompetent, they are voted out of office.

Examining the determinants of terror rounds of this analysis. In particular we look at the repercussions of political proximity to the U.S. with respect to terror in the own country. We find that friendship to the U.S. is costly as it increases the amount and severity of terror attacks. Finally, we see once more the close relationship of democracy and terror: political reforms that reduce democracy trigger additional terror attacks.

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Education

10/05 to 11/07	Doctoral Studies in Economics at the University of Zurich, Switzerland.
01/04 to 09/05	Doctoral program "Quantitative Economics and Finance" at the University of Konstanz, Germany.
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10/98 to 08/03	Studies of International Economics at the Eberhard-Karls-University Tübingen, Germany. Thesis: "New Approaches to Measure the Costs of Protection." Degree: "Diplom-Volkswirt" (Masters' equivalent). Among the top 10% of the students of the class of 2003.

Work experience

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