Income growth and democratization—An instrumental-variable approach

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Abstract

There is wide agreement among academics and policy makers that democracy is a universal value—it brings with it a more secure, stable and prosperous society. One should therefore try to identify causes of democracy. In this thesis I study the short term effect of economic growth on changes in democracy. A quick glance at data tells us that wealthy countries are democratic, and common wisdom suggests these countries are democratic because of their wealth. The main contribution of my thesis is to question this view. I make progress to existing literature by taking time series persistence and simultaneity issues in the data into account. Formal tests are established, and they suggest that both problems are present. I therefore first difference the data and use an instrument variable approach during estimation. The econometric framework provides a couple of interesting results. First, once time series persistence is accounted for, the estimated effect of income on democracy changes from positive to negative and statistically significant. Second, when economic growth is instrumented the point estimate drops even further, making it quantitatively important as well. These findings might be important for policy making, as they imply that one should be careful when designing money transfers to poor countries.

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All remaining flaws in this thesis are my own.

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[1] See http://www.sv.uio.no/isv/english/research/projects/conceptualization-and-measurement/index.html

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Chapter 1

Introduction

1.1 Motivation and background

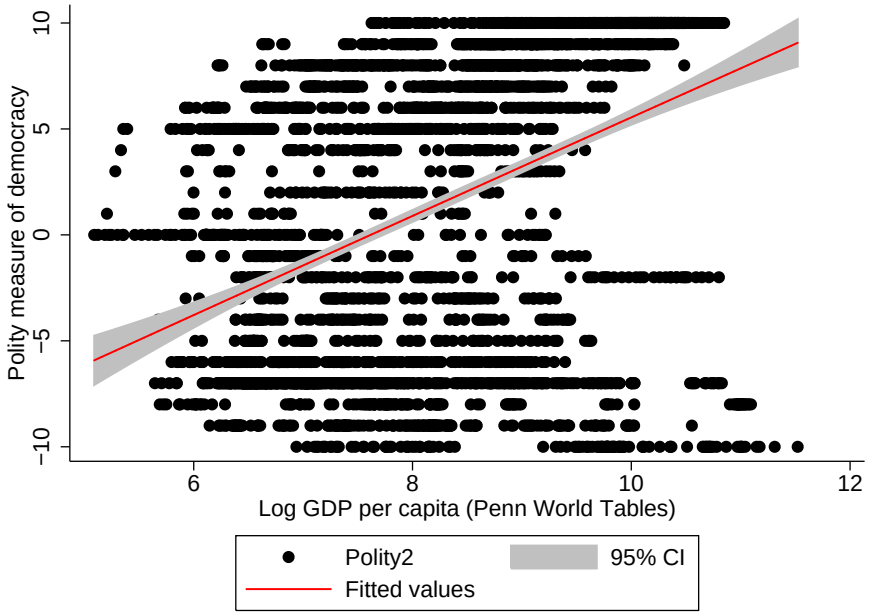
> *"Democratically governed nations are more likely to secure the peace, deter aggression, expand open markets, promote economic development, protect American citizens, combat international terrorism and crime, uphold human and worker rights, avoid humanitarian crises and refugee flows, improve the global environment, and protect human health."*
>
> U.S. Department of State (2013)

Promoting democracy is not only on the U.S. policy agenda. Most academics and policy makers agree that democracy has a number of positive implications. Rulers rule on behalf of the population, and depend on their support to stay in office. Democratic processes will therefore provide outcomes more consistent with the will of citizens. Basic human and political rights are respected. The democratic peace thesis upholds that democracies do not go to war against each other (Bueno de Mesquita, Smith, Siverson and Morrow, 2003, 218). In line with this view, one can think that international democratization processes will reduce both the frequency and severity of conflicts around the world. Democracy is furthermore argued to have a positive effect on economic development and welfare, at least in the long run (Papaioannou and Siourounis, 2008). The overall picture is that democratic systems undoubtedly seem better than any other alternative. Given that democracy has so many positive implications for societies, it is crucial to identify the mechanisms leading up to this form of government. In this thesis I will study the effect of income on democracy.

At first sight, it might seem obvious that higher income leads to democratization.

Indeed, one of the most robust patterns when looking at cross-country data is the positive relationship between the two. This stylized fact is reproduced in Figure 1.1, which plots (log) per capita income against democracy. Each point in the figure represents an income-democracy observation at the country-year level. A simple least squares line is included to highlight the sign of the relationship. The regression line demonstrates once and for all a positive relationship between the two variables, and uncertainty bands reveal that the null hypothesis (no correlation) is rejected at any reasonable confidence level.

Figure 1.1: Income and democracy, 1980-2007



The stylized fact reproduced in the figure seems to have established some kind of premise for the way many researchers approach a potential *causal* relation between income and democracy. In fact, there is a substantial number of studies, including Lipset (1959)'s influential modernization theory, that readably take the positive income-democracy correlation as evidence for a causal effect of income on democracy.[1] With this as a starting point, they set out to explain *why* increased income leads to democracy. Even those who do not take for granted a positive effect seem to constrain

[1]For other studies that claim a positive association see; Dahl (1971), Huntington (1991), Rueschemeyer, Stephens and Stephens (1992), Barro (1999), Epstein, Bates, Goldstone, Kristensen and O'Halloran (2006) and Moral-Benito and Bartolucci (2011).

their analysis to a one-sided hypothesis—the null of no effect is tested against the alternative—higher income causes higher levels of democracy.[2]

However, Figure 1.1 demonstrates nothing else than a positive raw correlation. In particular, it is silent about any causal relationships, including the timing of causation. Does income causes democracy, or is it the other way around? What about other factors not part of the plot, which explain both high income and high democracy? Does income and democracy co-move once these factors are taken into account? Are causal effects different in the short run than in the long run? Figure 1.1 is silent about these issues. Thus, one cannot infer causality based on the regression slope in the figure. This is important to realize if one wants to provide policy advices, as the validity of such advises hinges critically upon causal relationships. With this lesson in mind I set out to determine the causal *short term* effect of income on democracy. In contrast to the large bulk of existing empirical work, I argue that income can actually hamper democratic development. At least in the short run.

Let me explain how. First, national income represents a potential economic prize for those who are able to gain monopoly power over the state. This is particularly true for income that can be reached by institutions controlled by the government. When national income is high, then there is more to grab for an autocratic state leader who consider diverting resources. Thus, increased income will, everything else equal, strengthen the ruling elite's incentive to push for autocracy.[3] Second, a wealthy government has more resources to secure its position and fight off opponents. Revenues allow the ruling elite to build up military and police capacity, to monitor its citizens, and to chase down any potential threat. It is undoubtedly very costly to enforce monopoly power. Third, an alternative to brute force is economic buy-off. Autocratic regimes can invest in infrastructure to please the population, or buy off bureaucrats or opposition groups. In the wake of the Arab Spring for instance, Saudi Arabian leaders strategically increased investments in public goods. Compared to the rulers in Libya and Syria, these leaders also remained relatively effortless in power. All of these strategies can reduce the risk that citizens raise against the rulers, and thereby reduce the probability of democratization. At least in the short run. Thus, increased governmental income might not only give the elite an increased incentive to push for autocracy, it can also give them increased opportunities to do so.

[2]A recent example is Acemoglu, Johnson, Robinson and Yared (2008), who report negative and significant income coefficients, but fail to comment upon them. The main conclusion in this paper is that income does not affect democracy.

[3]Increased governmental income will also alter the incentives of the citizens. I take this into account in the theoretical model presented in this thesis.

1.2 Main contribution and findings

To formalize the arguments above, I construct a simple game theoretical model. The model demonstrates how increased governmental income changes the players' incentives. A key result is that governmental income, when it reaches a certain level, shifts the equilibrium from democracy to autocracy. Readers should note that the model does not offer any complete or general theory about the link between income and democracy. Not even near. Rather, its purpose is to formalize one possible mechanism (of many) that can hamper democratic development in the wake of higher income.

This thesis' main objective is to investigate whether the data can inform us about a causal effect of income on democracy. Two econometric concerns make this task extra challenging. First, both income and democracy posit high persistence over time in most countries. If the levels of these time series are sufficiently persistent (that is, if they consist of a unit root), then they violate the standard assumption of stationarity. If not controlled for, this will lead to biased estimates. Most studies in the literature do not address the problem of unit root. In contrast, I formally test the hypothesis of a unit root and find this to be the case in both series. I then solve the issue by first differencing income and democracy in the econometric analysis. Consequently, it is the effect of economic *growth* on *changes* in democracy that is estimated. However, if the level equation is correctly specified, then the estimate from the first differenced equation will effectively reflect level effects. The reason is that the relationship between economic growth and democratic change on the one hand, and between economic income level and the level of democracy on the other, should be qualitatively similar. Intuitively, higher economic growth *must* give higher income levels. This is true by the very definition of these concepts. The same goes for the link between level and change in democracy. It is therefore hard to see how the effect of income growth on changes in democracy can differ qualitatively from the effect of the level of income on the level of democracy.[4] After income and democracy have been first differenced, the link between the two is found to be *negative*.

Second, and equally important, there is the issue of reversed causality. In particular, it is possible that income growth and democratization affect each other simultaneously. If democratization has a causal effect on income growth, then failing to take this into account will result in biased estimates. This is true regardless of the causal effect of income growth on democratization. Interestingly, most studies of the link between

[4] An alternative argument can be formed using a standard difference equation. Consider for instance the linear equation $y_{it} = \beta x_{it-1}$. The first difference is $\Delta y_{it} = \beta \Delta x_{it-1}$, where the coefficient in the second equation by definition, is identical to the one in the first equation. Thus, if the first equation is true, one can always estimate the second and obtain the same result.

income and democracy seem to either abstract from the simultaneity problem all together, or to hope that lagged variables solve the problem. An important contribution of this thesis is the attempt to address the simultaneity problem in a proper manner. To this end I take an instrumental variables approach. Instrumental variables are used to filter out endogenous variation in explanatory variables, and thereby obtain a measure with exogenous variation only. This variation is then used to identify a causal effect on the dependent variable. Reversed causality is therefore controlled for in my case *if* the instruments chosen are exogenous (Wooldridge, 2009). Two different instrumental estimators are applied. The first one is the two stage least squares (2SLS) estimator. I use climatic natural disasters and global economic growth as instruments for (country-level) economic growth. The second estimator is the one constructed by Arellano and Bond (1991). It is a GMM-type estimator, and allows me to instrument all explanatory variables. After simultaneity problems in the data are addressed, the negative effect of income growth on democratization turns *quantitatively important*.

In sum, the main conclusion from the econometric analysis is that controlling for unit roots and simultaneity alters the well-established positive effect of income on democracy completely. A negative effect of economic growth on democratization is in itself hard to reconcile with the conventional wisdom, which is that wealthy countries are democratic because they are wealthy. While this finding is in contrast to the results in most existing income–democracy studies (Fayad, Bates and Hoeffler (2012) being the exception), my results are in line with findings from the resource-curse literature. This literature emphasizes how higher income can pose a challenge rather than a blessing to countries. Among the arguments used are those I have presented above.

An interesting question arises. If increased income has a negative effect on democratization, why then are so many wealthy countries democratic? First, it must be kept in mind that the result is conditional upon *all else being equal*. Thus, I am not claiming that Norway will turn autocratic following high growth. Rather, for certain values on other factors, an income increase will have a negative effect on democracy. Second, even though I do not find any strong evidence for a long term effect of income on democracy. The results from an error correction model (see chapter 8) instead suggest that if anything, the effect turns positive with time. This offers a possible explanation for why most wealthy countries are democratic, even though the short term effect of income growth on changes in democracy is negative. However, it is hard to reconcile a negative short term effect with a positive long term effect—these two have an embedded contradiction. A more credible explanation is that a number of other factors than income render nations democratic. Acemoglu et al. (2008) for instance,

point at historical factors as the explanation.

Even though I look into short term effects, my results should have substantial policy implications. Governments in developed countries, international financial agencies and aid organizations, seem to base their policies on an underlying assumption that increased income has positive implications for democracy, peace and development more generally. If the effect of income on democracy instead is negative, then providing financial aid and encouraging economic development in poor autocratic countries can come at the expense of hampering democratization. The solution to this problem should not be to stop money transfers to poor countries all together. Rather, one should ask *how* money transfers can be designed to prevent negative effects on the democratic processes. It is possibly better to commit to long term aid programs rather than one time transfers, given that the negative effect of income on democracy only seems to hold in the short term. Furthermore, payments should be conditioned upon being used for the common good, rather than diverted for personal benefits for the few. Even though there are weaknesses with the econometric analysis presented in this thesis, the results nevertheless imply that one cannot take the conventional view of income as positive for democracy for granted.

1.3 Defining democracy

There is no consensus in the literature regarding how democracy should be defined. Furthermore, no two democratic regimes are exactly equal. The Norwegian parliamentary system differs substantially from the presidential system in Finland. The Finnish system is very different from the presidential system in South Africa, where the President gets elected by the legislature rather than by the population. Democracies do however, have some important commonalities. In essence, democracy is "government by the people for the people". The question is what this statement amounts to in practice.

The literature is divided between those who define democracy according to the existence of particular political institutions (institutional definition), and researchers who base their definition on some underlying principles (substantial definition) (Knutsen, 2011, 46). To understand democracy, it is not enough to point at a list of institutions that needs to be in place for it to function. Rather, a deeper understanding of why the institutions are democratic is needed. Accordingly, I follow the second approach and define democracy based on three interdependent criteria I find decisive for democracy. First, there must be institutions and procedures through which citizens can express

their preferences about political leaders and alternative politics.[5]

Second, there must be institutional constraints on the power of the executive. In other words, there must be checks and balances on the power of the executive to prevent rule based on personal interests (extending own powers etc.).

Third, citizens must be guaranteed civil liberties (including rule of law) and political rights. Rule of law is an important aspect in democratic societies. All citizens, including the elite, are equal in front of the law (O'Donnell, 2001, 24-25). Besides being important determinants for democracy in itself, factors such as freedom of expression, organization, the right to join political parties and run for office are also necessary for the first criterion to be valid (Beetham, 1994, 28-29).

Researchers do not only disagree about how democracy should be defined, they also have different opinions about how it should be measured. In particular, it is debated whether democracy should be conceptualized as a dichotomous or graded phenomenon (Collier and Adcock, 1999). Alvarez, Cheibub, Limongi and Przeworski (1996) uphold that while some regimes might be more democratic than others, they should not be considered democratic unless offices are contested. They argue that "while democracy can be more or less advanced, on cannot be half-democratic: there is a natural zero point" (1996, 21). Thus, the researchers make the case for a dichotomous measure of democracy. Bollen and Jackman (1989, 618) on the other hand, take a graded approach and argue that democracy is "always a matter of degree". Collier and Levitsky (1997) emphasize the extensive number inconsistent (hybrid) regimes in the world today. The regimes have some democratic features, but are not considered to be (fully) democratic (see also Diamond (1999)). In using a dichotomous measure of democracy, much of this information would not be taken into account (Hadenius and Teorell, 2005, 91). For instance, one would lose the information that Norway is more democratic than Bolivia, even though both countries have elections for political office. The chosen democracy threshold in dichotomous measures furthermore leads to potential reliability and validity problems (Elkins, 2000, 293).

I want to utilize as much information as possible about the extent of democracy and avoid the potential reliability and validity problems described above. I therefore view democracy as a regime form in which a country can approximate more or less well, at any given point in time. In the empirical analysis I study changes in level

[5]This criterion covers the aspects that Dahl (1971) and Dahl (1998) label *contestation* and *participation*. Contestation requires that political offices are filled through competition. Thus, elections for office positions must be held. Participation implies that citizens are guaranteed the right to participate in the political process. In representative democracies an important aspect of this is that all citizens above a certain age have the right to vote in political elections. Furthermore, there must be channels through which the citizens can express their preferences regarding specific policies.

of democracy from one year to the next. Democratization is therefore referred to as changes in democracy over time. Note that democratization can take negative values (as is the case when there are movements towards autocracy). My main measure of democracy is the variable Polity2 from the Polity IV dataset (Marshall, Gurr and Jaggers, 2012). I explain why in section 5.2.

This thesis does not distinguish between democratic transition and democratic consolidation (see e.g. Schmitter (1994) and Schedler (1998)). Instead, *all* movements in democracy over time are labeled democratization (also if the country already is considered to be democratic). Thus, it makes little sense in this thesis to distinguish between the phase when a country makes the shift from autocracy to democracy (transition), and *further* improvement of democratic quality for a country that is already considered to be democratic (consolidation).[6] A further distinction in the literature is between studies of transitions towards democracy (democratization) and democratic survival. I focus on the first aspect and set out to determine the effect of income growth on democratization. Income has however also been found important for democratic survival (see e.g. Przeworski and Limongi (1997)).

1.4 Summary of content

This thesis is divided into nine chapters. Chapter two reviews a number of relevant studies of the link between income and democracy. In particular, it illustrates that the debate regarding income and democracy over the years largely has turned into a debate regarding the relevance of Lipset's (1959) modernization theory. In chapter three I develop a simple game theoretical model that illustrates a possible mechanism that links income negatively to democracy. Assuming that people act strategic, it is reasonable to think that income growth changes the players' incentives. Concretely, economic growth gives the elite an increased incentive to push for autocracy as there is more to grab. The elite can also use the increased revenues to buy off the population and make them accept autocracy (at least in the short term). In chapter four I review econometric methods. Special emphasis is put on the instrumental variables approaches, which address the issue of simultaneity (amongst others). Chapter five outlines the dataset and variable definitions, and also presents descriptive statistics. Chapter six provides an introductory analysis. I first replicate the most common finding in the literature, namely that of a positive effect of income on democracy.

[6] Teorell (2010, 32) points out that such a distinction can be more fruitful for researchers that operate with a dichotomous conceptualization of democracy.

Both variables are used in levels. Then I perform panel data tests for unit roots and simultaneity. These tests suggest that neither the presence of unit roots nor simultaneity in the data can be ruled out. This renders the results from the level model unreliable. Chapter seven reports the results from the main analysis. Once presence of unit roots are controlled for, the effect of income on democracy turns *statistically* negative. Furthermore, once simultaneity problems in the data are addressed, the effect also becomes of *quantitative* importance. A battery of robustness checks is conducted and the results are found to be relatively robust. In chapter eight I dig deeper into the results and ask what kind of observations that drive the results. I find that income seems to have a stronger negative effect on democracy in democracies and inconsistent regimes, than it does in autocracies. Furthermore, income seems to hamper democratic development more for countries that are on a path *towards* democracy. Finally, I do not find strong evidence of a *long term* effect of income on democracy. If anything however, this effect is positive. Chapter nine concludes.

Chapter 2

Previous research

Over the years, the link between income and democracy has received considerable attention which has resulted in a vast body of research.[1] This is reasonable as the association addresses the effect of one of the most important means in international politics; money transfers, on one of the superior goals; the expansion of democracy. Despite the extensive number of studies however, researchers have not been able to come to an agreement regarding the link between the two. The aim of this chapter is not to give an extensive review of the income–democracy literature. Instead, some key contributions that are representative for the larger debate are outlined. I also review a couple of studies that argue that economic growth in some instances can be a curse rather than a blessing.

2.1 Income and democracy

Interest regarding the association between income and democracy dates all the way back to Aristotle, who claimed that only in a wealthy society with relatively few people living in poverty, could democracy arise and survive (Moral-Benito 2011:1). The observed correlation between income and democracy (reproduced in Figure 1.1), is furthermore the cornerstone of Lipset's (1959) influential modernization theory. Lipset and other modernization theorists claimed that democracies were created and preserved through a process of modernization. This process was argued to involve changes in industrialization, urbanization, wealth and education (Lipset, 1959, 80). Lipset claimed that these factors were so closely interrelated that they formed one common factor; economic development. In particular, the modernization process was

[1]Furthermore, in political science few questions have been given more attention than the query of what leads to democracy. Thus, there is an enormous democracy literature. However, as this thesis studies the effect of income on democracy I only review this part of the literature.

assumed to strengthen the middle class, who no longer would tolerate repressive political regimes. As a result democracy would evolve. Using simple statistical analysis, Lipset found that economic development correlated with democracy.

The modernization theory seems to have created a premise in the study of income and democracy. As a consequence, subsequent studies addressing the link between the two, make themselves relevant by either supporting or rejecting the theory. Most of these studies have argued for the modernization theory (see e.g. Dahl (1971), Huntington (1991), Burkhart and Lewis-Beck (1994), Londregan and Poole (1996), Barro (1999), Boix and Stokes (2003), Epstein et al. (2006), Kennedy (2010), Boix (2011), Moral-Benito and Bartolucci (2011), Treisman (2012)).[2] In fact, for a long time the positive effect of income on democracy was widely accepted as a stylized fact in comparative politics (Geddes, 1999, 117).

A number of more recent studies however, have argued that economic development does not create democracy per se. Przeworski and Limongi (1997, 167) for instance, argue that regimes do not transition to democracy as per capita income increase.[3] Instead, once countries have become democratic, for whatever reason, higher levels of GDP per capita increase the probability of *remaining* democratic. Thus, as history evolves, wealthy democracies will increase in numbers, because whenever dictatorships dies in wealthy countries, democracy will be there to stay (Przeworski and Limongi, 1997, 159). The researchers claim that there are two reasons for why democracy is more likely to die in poor countries. First, the marginal utility of consumption is higher in poor countries because they have little to consume. This implies that the utility gain of becoming a dictator and thereby taking all wealth is high. Second, poor countries, which typically have little capital, have much to gain by increasing the capital stock. Thus, the capital stock will increase faster in poor countries. This implies that the speed in catching-up following conflict and capital destruction is higher in poor countries. In sum, the struggle for dictatorship is less attractive in wealthy countries, because the gain from getting all rather than part of the total income is smaller. Furthermore, the recuperation from destruction is slower (Przeworski and Limongi, 1997, 166).

Boix and Stokes (2003) on the other hand, criticize Przeworski and Limongi (1997) for only including country-year observations after 1950. When extending the time series further back, they find economic development to have a significantly positive effect

[2]Several of these studies however, argue for a conditional income effect and emphasize different mechanisms that links income to democracy. For qualitative studies on the link between income and democracy see Moore (1966) and Rueschemeyer, Stephens and Stephens (1992).

[3]See also Przeworski, Alvarez, Cheibub and Limongi (2000).

on democracy. However, Boix and Stokes also criticize the modernization theory for assuming that democratization is triggered by income development in itself. Instead they argue that democratization is caused by other changes that accompany development. In particular, lower levels of *income inequality*. When countries develop, income becomes more equally distributed. Thus, the ruling elite will find a democratic tax structure less expensive as the country gets wealthier. Consequently, democratization becomes more likely (Boix and Stokes, 2003, 539-540).[4] Kennedy (2010) on the other hand, defends the modernization theory. He claims that there is a positive link, but that it is conditional. Kennedy argues that the results from the studies that reject the modernization theory are driven by a conceptual issue regarding how they measure democracy. Democratic transitions should not be regarded as a one-time, unique and exclusive phenomenon (Kennedy, 2010, 797). When changing this assumption, Kennedy finds that there is a positive link between economic development and democratization. There is however, an inherent contradiction. Income development has an opposite effect on *onset* and *outcome* of institutional change. Since economic development is generally stabilizing for all regime types, at the same time as it increases the costs for the ruler to bring about institutional change, economic development decreases the probability of major institutional change. However, because economic development makes it harder to establish a new autocratic regime and increases the attractiveness for the masses to gain power, regime changes that does follow economic development, are more likely to be towards democracy than autocracy (Kennedy, 2010, 797). Thus, Kennedy concludes that the effect of income on democracy is positive but conditional.

Acemoglu et al. (2008) on the other hand, side with Przeworski and Limongi (1997) and argue that the established association between income and democracy is spurious. When controlling for country specific factors they show that the positive effect of GDP per capita on democratization disappears. The same happens when they use instrumental variable approaches to control for the possible simultaneity problems in the data. The authors argue that many developed countries are democratic due to *historic factors;* countries have chosen different development paths.[5] Political and economic development paths are furthermore interwoven. While some countries embarked upon development paths that were associated with both democracy and economic growth, others pursued a path based on dictatorships, repression and more limited economic

[4]See also Boix (2003).

[5]Acemoglu, Johnson and Robinson (2001), point at differences in colonization strategies as an important determinant for different development paths of colonial societies. For other studies that point at the impact of certain historical factors see e.g. Jones (2003) and Acemoglu, Johnson and Robinson (2002).

growth (Acemoglu et al., 2008, 836).[6]

The literature review so far, illustrates that the income-democracy debate has been dominated by a one-sided hypothesis. Researchers have either supported the modernization theory or argued that there is no effect of income on democracy. Fayad, Bates and Hoeffler (2012) on the other hand, find a *negative* effect of income on democracy for inconsistent regimes (i.e. countries that are neither democratic nor autocratic). This finding is furthermore robust against controlling for country specific factors and simultaneity. The researchers also point out that Acemoglu et al. (2008) report negative income effects but fail to comment upon them as such.[7] Fayad et al. (2012) furthermore find that it is the resource proportion of income per capita that is negatively and significantly related to democracy. Using a pooled mean estimator they also find that the long term effect of increased income is a drop in democracy.

Similarly, Bueno de Mesquita and Downs (2005, 78), argue, in sharp contrast to the modernization theory, that economic development strengthens autocracies in the short term. They claim that authoritarian regimes have learned to use economic development to their advantage. By increasing investments in public goods such as public transportation, national defense, primary education and health care, the rulers become more popular with the population. At the same time the rulers prevent investments in coordination goods, which make it hard for the population to coordinate themselves and challenge the ruling regime. In particular, coordination is prevented by limiting political rights, general human rights, press freedom and the population's access to higher education. In this way the autocratic leaders increase their probability of survival. China and Russia are illustrative examples of countries where economic development cannot be said to have triggered democratization. They are furthermore examples of countries where the ruling regime strictly limit coordination goods by monitoring media coverage and strongly penalizing criticism towards the regime.

2.2 Is there an economic growth curse?

In a broader perspective, higher income has in many studies been found to be damaging for development. Take for instance the literature on the *natural resource curse*.

[6]Moral-Benito and Bartolucci (2011) argue that a weakness in the Acemoglu et al. (2008) study is that they do not account for relative importance of income. Using the same data as Acemoglu et al. (2008), they find a significant but non-linear effect of income on democracy even after controlling for country-specific effects. There is a positive effect of income in poor countries, but this effect vanishes as countries become wealthy.

[7]See e.g. Acemoglu et al.'s (2008) table 3 columns 2, 3, 4, 5, 8 and 9. Several of these estimates are furthermore statistically significant. The conclusion in this paper however, is that income does not affect democracy.

It refers to the paradox that many natural resource abundant countries have lower economic growth than resource-poor countries (Sachs and Warner, 2001).[8] One of the common explanations given for the curse is that rent-seeking elites in resource abundant countries often divert a large portion of the resource revenues to themselves. They are able to do so because of weak, ineffective and corrupt institutions. Furthermore, resource abundant countries often see a decline in the competitiveness of other economic sectors. This is also labeled the Dutch disease.[9]

The resource curse has also been linked up to foreign aid. Natural resources and foreign aid revenues are both windfalls, in the sense they are almost exogenous to the economy. This makes it easier for corrupt politicians to grab resources for personal use, without having to resort to unpopular (and often less profitable) means like taxation (Djankov, Montalvo and Reynal-Querol, 2008, 4). Accordingly, Boone (1996, 322) finds that foreign aid has no effect on economic growth nor investments in developing countries. Burnside and Dollar (2000) modify this finding some. They argue that aid can have a positive effect on growth and investments if the institutional quality is high. In countries without good fiscal, monetary and trade policies however, aid transfers are found to have little effect. Djankov, Montalvo and Reynal-Querol (2008), link the aid curse directly up to democratic quality. They argue that politicians' rent-seeking behavior is hampering for democracy because it gives them an incentive to weaken the political institutions. The reason is that weak institutions are a a necessary condition for the elite to be able to grab foreign aid for personal use. Furthermore, when revenues do not depend on taxes from the citizens, there is less incentive for democratic accountability. The corrupt government official will also try to sustain their rent-seeking activity by reducing the probability of losing power. All these factors are hampering for democracy (Djankov, Montalvo and Reynal-Querol, 2008, 5-6).

Accordingly, several studies find general economic growth, i.e. not only growth connected to natural resources and aid, to negatively affect democracy. Epstein et al. (2006) for instance, find that higher economic growth within a year decreases the probability democratization. However, in a Markov analysis, they find that this result only holds for countries that started out as autocracies (Ibid, 560). Kennedy (2010, 792-793) accordingly finds that GDP per capita growth within a year decreases the probability of general institutional change. This indicates that economic growth has a positive effect on the stability of both authoritarian and democratic regimes.[10]

[8] See also Gelb (1988), Sachs and Warner (1999) and Auty (2001).

[9] The resource curse has also been linked to a higher probability of conflict (see e.g. Collier and Hoeffler (2005), Le Billon (2001) and Colgan (2013)).

[10] This is in accordance with findings from the economic crises literature (see e.g. Gasiorowski (1995) and Geddes (1999)). Here economic crises are found to be destabilizing for both democratic

This literature review has illustrated that the income-democracy literature is dominated by a one-sided hypothesis. Researches either support Lipset's (1959) modernization theory, or argue that income does not have a causal effect on democracy. Fayad, Bates and Hoeffler (2012) however, find economic level to have a negative effect on democracy in inconsistent regimes. This is consistent with findings from the curse literature that emphasize that economic growth in certain instances can have a hampering effect on development. Fayad, Bates and Hoeffler (2012) however, do not offer a theoretical explanation for why income can have a hampering effect on democracy. This makes the story less credible, especially in a field dominated by the one-sided hypothesis described above. Thus, in the next section I develop a simple theoretical model which illustrates one possible mechanism that can link income negatively to democracy. The model demonstrates how income growth changes the players' incentives. In particular, it illustrates that rent-seeking elites get an increased incentive to push for autocracy following economic growth. This underlying logic is similar to the arguments in the article by Djankov, Montalvo and Reynal-Querol (2008), who find that aid transfers can hamper democratic development.

and autocratic leaders. The mirroring argument from this debate is that positive economic growth strengthens both democratic and autocratic leaders.

Chapter 3

Theory

In this chapter I develop the theoretical framework for this thesis. Readers should note that the model is not a complete or general theory about the link between income and democracy. Not even close. The objective of the theoretical framework is simply to demonstrate that a positive effect of income on democracy is not a law of nature. In order to do so, I formalize one possible mechanism (out of many) that can explain a negative short term effect of higher income on democracy. Thus, the theoretical model only captures some aspects of a dynamic and complex process. Consequently, the model conclusion only holds *everything else being equal*.

3.1 Introduction

For this thesis I post the following working hypothesis: *In the short term, higher income in a country can come with risk of less democracy, everything else being equal.* I base my claim on four simple observations, which I will take for granted throughout: First, most individuals derive utility from economic wealth, in particular when that wealth accrues to themselves. Second, economic wealth seems to be a limited resource in the sense that not everyone can have unlimited wealth simultaneously. Third, virtually any human societies, in particular countries, are organized hierarchically. Those at the top of the hierarchy are in some cases controlled by the people through democratic processes such as elections. In other cases these leaders secure their power using brute force, or by buying off government officials. In any case, an arguably small group of people is placed at the very top of most societies. In some political systems this group has a disproportional large influence of the distribution of wealth. Fourth, the wealth seems to be more equally distributed (between leaders and the rest) in

democratic countries compared to autocratic ones.[1]

These observations lead quite naturally to the working hypothesis stated above. The first two observations imply some sort of a tradeoff between different groups of people when it comes to wealth. In particular, the struggle for economic wealth is not totally different from a null sum game, where someone's income gain is someone else's income loss.[2] When adding the third observation (that societies are organized hierarchically), one realizes that there is a potential tension between the leaders of a society and the rest. In many countries this tension is of limited relevance. These countries typically have high quality legal systems, deep democratic traditions, and so on. In others, diverting publicly owned resources for private use is much easier, making the expected return of such behavior high.[3] The ruling elite in this latter group of countries have especially much to gain when income is high, as this implies there is more to grab. This is consistent with the fourth observation. Thus, higher income as a result of economic growth, increases the incentive for the ruling elite to divert resources for personal use. For that reason they should be willing to take stronger measures to secure power when income is high. Those measures can include a number of non-democratic actions, for example bribes to officials, to interest groups among the people, or violent suppression of oppositional groups. It should also be said that higher national income means that the public looses more when the elite diverts all the resources. Still, a wealthy autocrat can afford to spend considerable resources to please potential opponents, and he has more to loose by not doing so. Saudi Arabia following the Arab spring is a good example of this. Here, the leaders had both the will and economic power to invest strategically in public goods during the Arab Spring. They also remained in power relatively effortless compared to their colleagues in e.g. Libya and Syria.

Motivated by an econometric concern for unit roots in the time series, this thesis analyzes empirically the effect of economic growth (first difference of logged income level) on democratic development (first difference of democracy level).[4] However, the underlying theory is about level effects. This is justified by two observations: First, the relationship between economic growth and democratic development on the one

[1] It could of course be discussed whether this is a cause or an effect of the regime type. See e.g. Boix (2003).

[2] Strictly speaking it does not have to be a null sum game. However, there is always potential for distributional conflicts.

[3] One cannot disregard the working hypothesis based on the observation that rich, western countries are more democratic than others. These countries differ in many other aspects than the income level. This is why the end of the working hypothesis, "everything else being equal", is so important.

[4] The problem of unit roots and why first differencing controls for the problem is discussed in detail in sections 4.3 and 6.2.

hand, and between economic income level and the level of democracy on the other, should be qualitatively similar. To see why, consider the linear equation $y_{it} = \beta x_{it-1}$. The first difference is $\Delta y_{it} = \beta \Delta x_{it-1}$, where the coefficient in the second equation is, by definition, identical to the one in the first equation. Thus, if the first equation is true, one can always estimate the second and obtain the same result. In other words, whenever the growth rate is positive in this example, the level has to rise by definition. Second, income growth matters to people only because it raises the income level. The growth rate does not give utility per se. Analyzing the growth rate based on a theory about income levels is fairly standard in the literature, in particular the literature on economic growth (see e.g. Solow (1956), Cass (1965) and Koopmans (1965)).

The purpose of the econometric part of this thesis is to test whether statistical results are consistent with the story portrayed above. Next I formalize the arguments made so far in a stylized model of autocracy. The constructed model is undoubtedly simple. It does however, clearly illustrate a possible mechanism that can link income negatively to democracy in the short term.

3.2 A simple model of autocracy

Consider a country with a population of size n who discounts the future at a rate δ. The population can be divided into two types of players, the elite (who governs the country) and citizens (the remaining population). The number of elite members is normalized to 1, so that the citizen group consists of $n-1$ individuals. A change of government implies that one individual is drawn from the pool of citizens to replace the existing elite.

I assume that the country can be in two different states, democracy and autocracy. Furthermore, both the elite and the citizens can either accept the current state or try to push the country into the other state. Whether players prefer democracy or autocracy depends on expected utilities in the two states. In a democracy, national income y is divided equally among all members of the population, and everybody (including the elite) get the same share $\frac{y}{n}$ of total income.[5] This amount is referred to as per capita national income. When the regime is autocratic, I assume that the elite gets to keep the entire income net of a transfer κ_e that satisfies $\kappa_e \geq \underline{\kappa}_e$, where $\underline{\kappa}_e$ is a positive constant. The transfer is handed out to the citizens, and captures the

[5]This can be thought of as the outcome of perfect competition among political candidates. Utility maximizing citizens always prefer the candidate that promises more distribution of income to each voter, so the only way to get elected is by transferring $\frac{y}{n}$ to each voter. Any other income sharing would imply that some political candidate could suggest a different sharing and be elected on that political platform.

notion that every individual needs at least some income in order to survive.[6] More generally, κ_e represents any cost that is specific to the ruling elite in an autocracy, e.g. corruption payments, expenditures on extra security measures, etc. Therefore κ_e will be a choice variable for the elite that needs to be solved for (the elite will set κ_e in order to maximize its own utility). Furthermore, I make the assumption that citizens pay an additional cost $\kappa_c > 0$ if they choose to oppose the ruling elite in an autocratic regime. This cost represents the disutility related to public repression of individuals, personal losses in civil war, etc.

Suppose the elite, the essential player in the game, is the one that can initiate an attempt to replace democracy by autocracy. As long as the elite chooses not to do so, the country remains democratic. If on the other hand, the elite pushes for autocracy, the citizens have to either a) try to overthrow the elite (possibly using violent measures) and re-establish democracy, or b) accept the autocratic elite and save the costs of opposition. When democracy is re-established after an autocratic regime, the elite is eliminated by the citizens (who then choose a new government) and gets zero utility forever after.

First, consider a democracy where the elite accepts the state of the country, i.e. it does not try to establish autocracy. Then everyone including the elite get the same share $\frac{y}{n}$ of total income. Thus, in an everlasting democracy the discounted lifetime utility for both the elite and the citizens, denoted U_E^D and U_C^D, is

$$U_E^D = U_C^D = \sum_{t=0}^{\infty} \delta^t \frac{y}{n} = \frac{y}{n} + \delta\frac{y}{n} + \delta^2\frac{y}{n} + \ldots = \frac{y}{n(1-\delta)}. \tag{3.1}$$

The last line follows from (A.3) in Appendix A.

In order to proceed I make the assumption that changes in income are persistent, so that any one-time increase or decrease in y is expected to last forever.[7] This assumption simplifies the analysis considerably because it implies that all future elites (in expected terms) will try to achieve autocracy, given that it is preferable today. To see this, suppose the current y is such that the elite finds autocracy optimal and therefore tries to introduce autocracy. Suppose this attempt is overthrown so the country becomes democratic again. Then the new elite will make the same analysis as

[6] Assume that citizens are responsible for production in the country. If $\kappa_e < \underline{\kappa}_e$, then the citizens die of hunger and production becomes $y = 0$. That can never be optimal for the elite as it would get zero income (which is less than it gets in a democracy) in this case. Therefore, the elite will always give something to the citizens.

[7] This is consistent with empirical analyses which suggest that log GDP is close to a random walk. This assumption is furthermore supported by unit root tests of the variable in my dataset. See section 6.2 for more details.

the previous one. Obviously, without any change in y also the new elite finds it optimal to implement autocracy, and it will immediately try to do so. More generally, if y is such that a party finds autocracy (democracy) optimal today, then the persistence in y implies that this party is expected to always find autocracy (democracy) optimal.

Now, suppose the elite finds it optimal to establish autocracy. A relevant question then is whether one can prevent the citizens to rise against the elite. In other words, can the elite secure its leadership in an autocratic state? To answer this question, remember that citizens in autocracy get a transfer κ_e from the autocratic elite. If they accept the elite their expected lifetime per capita utility U_C^B becomes

$$U_C^B = \sum_{t=0}^{\infty} \delta^t \frac{\kappa_e}{n} = \frac{\kappa_e}{n}\left(1 + \delta + \delta^2 + \ldots\right) = \frac{\kappa_e}{n(1-\delta)}. \tag{3.2}$$

However, if the citizens try to prevent autocracy, the outcome becomes uncertain. Let any attempt to implement autocracy against the will of citizens, but also to keep it from one period to the next, succeed with probability p.[8] The corresponding probability that citizens are able to prevent autocracy is $1-p$ (given that they oppose it).[9] When citizens rise against the autocratic elite, they also face a cost κ_c due to repression from the rulers. Furthermore, in that case the elite should never transfer more than the minimum amount $\kappa_e = \underline{\kappa_e}$ (they gain nothing by increasing the transfer). The individual citizen's expected utility when citizens rise against the autocratic elite, denoted U_C^A, is therefore[10]

$$\begin{aligned} U_C^A &= \sum_{t=0}^{\infty} \delta^t \left[(1-p)\frac{y}{n} + p\left(\frac{\underline{\kappa_e}}{n} - \frac{\kappa_c}{n}\right)\right] \\ &= (1-p)\frac{y}{n} + p\left(\frac{\underline{\kappa_e}}{n} - \frac{\kappa_c}{n}\right) + \delta\left[(1-p)\frac{y}{n} + p\left(\frac{\underline{\kappa_e}}{n} - \frac{\kappa_c}{n}\right)\right] \\ &\quad + \delta^2\left[(1-p)\frac{y}{n} + p\left(\frac{\underline{\kappa_e}}{n} - \frac{\kappa_c}{n}\right)\right] + \ldots \\ &= \frac{(1-p)y}{n(1-\delta)} + \frac{p(\underline{\kappa_e} - \kappa_c)}{n(1-\delta)}. \end{aligned} \tag{3.3}$$

Comparing (3.2) to (3.3) it is clear that citizens accept an autocratic state if $U_C^B \geq U_C^A$,

[8] Thus, the probability that the autocracy will survive for two periods (after implementation) is p^2, for three periods p^3, etc.

[9] I make the simplifying assumption that p is independent of income. In reality however, this is most likely not the case, see e.g.Boix (2003).

[10] The assumption of persistent changes in y makes this infinite sum far simpler than if income changes was transitory.

or

$$\frac{\kappa_e}{n(1-\delta)} \geq \frac{(1-p)y}{n(1-\delta)} + \frac{p(\underline{\kappa}_e - \kappa_c)}{n(1-\delta)}$$
$$\kappa_e \geq (1-p)y + p(\underline{\kappa}_e - \kappa_c) \tag{3.4}$$

Intuitively, when growth is positive so that y rises, it becomes more difficult to buy off the citizens as their alternative cost (the utility in democracy) then increases. The result implies that the citizens prefer autocracy when growth is sufficiently negative so that the income level declines below the critical value in (3.4). Equation (3.4) can be thought of as a participation constraint for the citizens that need to hold if the elite is to ensure autocracy. The analysis is summarized so far in Proposition 1:

Proposition 1. *When income growth is positive, citizens require a higher transfer from the elite to accept autocracy.*

I have assumed throughout that n, p and δ are exogenous. It is however reasonable to allow κ_e to be endogenous. In particular, one might think that the elite sets κ_e in order to maximize its utility in autocracy. In order to solve for κ_e, suppose that the elite decides to implement autocracy and at the same time pay the citizens so that they accept this decision. The lifetime utility for the autocratic elite in this case, denoted U_E^B, is the discounted sum of national income less of the transfer to citizens:

$$\begin{aligned} U_E^B &= \sum_{t=0}^{\infty} \delta^t (y - \kappa_e) \\ &= (y - \kappa_e) + \delta(y - \kappa_e) + \delta^2(y - \kappa_e) + \dots \\ &= (y - \kappa_e)\left[1 + \delta + \delta^2 + \dots\right] \\ &= \frac{y - \kappa_e}{1 - \delta}. \end{aligned} \tag{3.5}$$

When is this outcome better for the elite than democracy? Comparing (3.5) to (3.1) we see that the elite prefers autocracy with transfers to democracy when $U_E^B > U_E^D$, or

$$\frac{y - \kappa_e}{1 - \delta} > \frac{y}{n(1-\delta)}$$
$$y\frac{n-1}{n} > \kappa_e$$
$$y > \frac{n}{n-1}\kappa_e. \tag{3.6}$$

However, for this to be an equilibrium also (3.4) has to hold. Notice that it is sufficient

for the elite to let (3.4) hold with equality to secure an autocratic state (this makes the citizens indifferent between accepting autocracy and opposing it). Thus, they will pay $\kappa_e = (1-p)y + p(\underline{\kappa}_e - \kappa_c)$. Inserting this into (3.6) and solving for y we get

$$\begin{aligned} y &> \frac{n}{n-1}\left[(1-p)y + p(\underline{\kappa}_e - \kappa_c)\right] \\ \frac{n-1-n(1-p)}{n-1}y &> \frac{pn}{n-1}(\underline{\kappa}_e - \kappa_c) \\ y &> \frac{pn}{pn-1}(\underline{\kappa}_e - \kappa_c). \end{aligned} \tag{3.7}$$

An important result immediately arises. Whenever (3.7) holds the elite prefers autocracy with transfers to democracy, and the citizens accept this claim. This leads way to the main hypothesis of this thesis, namely Proposition 2:

Proposition 2. *When income growth is sufficiently positive so that* (3.7) *becomes satisfied, then the equilibrium regime is not democratic.*

Next, suppose the elite prefers an autocratic state and that the citizens choose to oppose it. Even if autocracy is implemented, it will eventually be overthrown. The expected length of an autocracy in this case is $p + p^2 + p^3 + \ldots = \frac{p}{1-p}$. When the autocracy is overthrown, the elite is eliminated by the citizens who choose a new elite. Remember that the autocratic elite should never transfer more than $\kappa_e = \underline{\kappa}_e$ when citizens are in opposition. Accordingly, the elite's expected discounted utility in this case, denoted U_E^A, is

$$\begin{aligned} U_E^A &= \sum_{t=0}^{\infty} \delta^t p^{t+1}(y - \underline{\kappa}_e) \\ &= p(y - \underline{\kappa}_e) + \delta p^2(y - \underline{\kappa}_e) + \delta^2 p^3(y - \underline{\kappa}_e) + \ldots \\ &= p(y - \underline{\kappa}_e)\left[1 + \delta p + \delta^2 p^2 + \ldots\right] \\ &= p(y - \underline{\kappa}_e)\frac{1}{1 - p\delta} \\ &= \frac{py}{1 - p\delta} - \frac{p\underline{\kappa}_e}{1 - p\delta}. \end{aligned} \tag{3.8}$$

When will the elite prefer autocracy with an opposing citizen group to democracy? Comparing (3.1) and (3.8), we see that the elite prefers autocracy if $U_E^A > U_E^D$, i.e. if

$$\frac{py}{1 - p\delta} - \frac{p\underline{\kappa}_e}{1 - p\delta} > \frac{y}{n(1-\delta)}.$$

Solving for y gives:

$$\begin{aligned}\left[\frac{p}{1-p\delta}-\frac{1}{n(1-\delta)}\right]y &> \frac{p\underline{\kappa}_e}{1-p\delta}\\ \frac{pn(1-\delta)-1+p\delta}{n(1-p\delta)(1-\delta)}y &> \frac{p\underline{\kappa}_e}{1-p\delta}\\ y &> \frac{pn(1-\delta)}{pn(1-\delta)-(1-p\delta)}\underline{\kappa}_e. \qquad (3.9)\end{aligned}$$

Thus, the higher the income, the stronger incentive will the elite have to enforce autocracy. In particular, if national income is sufficiently high so that equation (3.9) holds, then the ruling elite will try to convert the country into autocracy. Notice that the existence of an opposition to the autocratic elite implies that the country will be autocratic with probability p, and democratic with probability $1-p$. This holds for every period, so a country in this situation will move back and forth between autocracy and democracy. Proposition 3 summarizes the result:

Proposition 3. *When income growth is sufficiently positive so that* (3.9) *becomes satisfied, then the elite prefers autocracy with risk of being overthrown by the opposition to democracy.*

The analysis so far has derived two conditions, namely equations (3.7) and (3.9), that are sufficient to move a regime away from democracy. That is, as long as at least one of the two conditions are satisfied, democracy cannot be an equilibrium. The reason is that the elite, motivated by the potential income award, will try to impose autocracy. However, even though (3.9) is satisfied, it does not mean that the elite will choose the autocratic regime with opposition. That is, even if (3.9) is satisfied, it does not imply that Proposition 3 represents an equilibrium. To fully characterize equilibrium we need to determine whether the elite can prefer autocracy with transfers to autocracy with opposition. Evidently it does so whenever $U_E^B > U_E^A$. Substituting for (3.5) and (3.8) we get

$$\frac{y-\kappa_e}{1-\delta} > \frac{py}{1-p\delta}-\frac{p\underline{\kappa}_e}{1-p\delta}$$

To check whether $U_E^B > U_E^A$ can be a solution we insert for (3.4) into the left hand side and then solve for y. The result is

$$\begin{aligned}\frac{y-(1-p)y-p(\underline{\kappa}_e-\kappa_c)}{1-\delta} &> \frac{py}{1-p\delta}-\frac{p\underline{\kappa}_e}{1-p\delta}\\ py\left[\frac{1}{1-\delta}-\frac{1}{1-p\delta}\right] &> p\underline{\kappa}_e\left[\frac{1}{1-\delta}-\frac{1}{1-p\delta}\right]-p\kappa_c\frac{1}{1-\delta}\end{aligned}$$

$$y\frac{\delta(1-p)}{(1-\delta)(1-p\delta)} > \underline{\kappa}_e\frac{\delta(1-p)}{(1-\delta)(1-p\delta)} - \kappa_c\frac{1}{1-\delta}$$
$$y > \underline{\kappa}_e - \frac{1-p\delta}{\delta(1-p)}\kappa_c \tag{3.10}$$

Now we are ready to revisit the question of whether Proposition 3 represents an equilibrium or not. Notice that the right hand side of (3.9) satisfies

$$\frac{pn(1-\delta)}{pn(1-\delta)-(1-p\delta)}\underline{\kappa}_e > \underline{\kappa}_e,$$

while the right hand side of (3.10) satisfies

$$\underline{\kappa}_e - \frac{1-p\delta}{\delta(1-p)}\kappa_c < \underline{\kappa}_e.$$

Thus, whenever (3.9) holds, so will (3.10). The conclusion is that the elite will always prefer to buy off the citizens to risking opposition, given that they prefer autocracy to democracy in the first place. Combining results, we get Proposition 4, a refinement of Proposition 2:

Proposition 4. *When income growth is sufficiently positive so that*

$$y > \frac{pn}{pn-1}(\underline{\kappa}_e - \kappa_c),$$

then the equilibrium regime is autocratic. Under this condition the elite will pay a transfer

$$\kappa_e = (1-p)y + p(\underline{\kappa}_e - \kappa_c)$$

to the citizens, who will accept the autocracy. The expected lifetime utility of the elite and the citizens are then given by

$$U_E^B = \frac{py - p(\underline{\kappa}_e - \kappa_c)}{1-\delta} \text{ and } U_C^B = \frac{(1-p)y + p(\underline{\kappa}_e - \kappa_c)}{n(1-\delta)} \text{ respectively.}$$

If the condition above does not hold, then the equilibrium regime is democratic. In that case the elite and the citizens get the same utility given by

$$U_E^D = U_C^D = \frac{y}{n(1-\delta)}.$$

In summary, a simple model which predicts that positive income growth might lead

to a regime shift from democracy to autocracy has been established. The main idea in this thesis is to test whether this prediction is consistent with data. Before the econometric analysis can be performed however, the methods and data applied must be reviewed. This is brought about in the following two chapters.

Chapter 4

Research design

To test whether income affects democracy in the data, I estimate a linear regression model. This chapter presents and discusses the econometric framework that underlies the empirical analysis. First, I present the standard assumptions that are needed for the estimates and inference to be valid. Second, I review the problem of time series persistence in the variables. Third, I discuss the simultaneity problem. Finally, two different instrumental variables estimators are suggested to take simultaneity into account.

4.1 Introduction

I will use econometric methods to investigate the *causal* effect of income on democracy. To fix ideas, consider the following dynamic and linear panel data model:

$$y_{it} = \beta_0 + \beta_1 x_{it-1} + \sum_{k=1}^{K} \gamma_k c_{kit-1} + u_{it}, \text{ where} \tag{4.1}$$

$$u_{it} = \alpha_i + \varepsilon_{it}$$

For each country i in period t, y_{it} denotes the dependent variable democracy, x_{it-1} is economic level (lagged by one period), and c_{kit-1} denotes control variable $k \in [1, \ldots, K]$. u_{it} is a residual term. In panel data, it is typically assumed that the residual term can be decomposed into a constant panel-specific factor α_i and a white noise residual $\varepsilon_{it} \sim IID(0, \sigma_\varepsilon^2)$.[1] The coefficients β_0, β_1, and $\{\gamma_k\}_{k=1}^K$ are unobserved population parameters to be estimated. β_1 is the main parameter of interest, as it represents the effect on y_{it} of a marginal change in x_{it-1}. Estimation of equation (4.1)

[1] $IID(0, \sigma_\varepsilon^2)$ denotes that the variable is *independent* and *identically distributed*, with mean zero and constant variance (denoted by σ_ε^2).

by ordinary least squares (OLS) methods will yield unbiased estimates and correct inference only if a number of assumptions are met (Wooldridge, 2009, 105, 503-505). The set of assumptions is reported below:

(A1) For each panel i, the regression equation is linear in the parameters. That is, equation (4.1) represents the true population model.

(A2) Random sampling, i.e. the data sample is representative for the underlying population.

(A3) There are no perfect linear relationships between the explanatory variables, i.e. no collinearity.

(A4) The expected value of the residual u_{it} is zero, conditional upon all the explanatory variables: $E(u_{it}|x_{it-1}, \{c_{kit-1}\}_{k=1}^{K}) = 0$.

(A5) The residual u_{it} displays constant conditional variance σ_u^2: $Var(u_{it}|x_{it-1}) = \sigma_u^2$. This is referred to as no heteroskedasticity.

(A6) There is no autocorrelation in the residuals: $Cov(u_{it}, u_{it-1}) = 0$.

Assumption (A1) is a trivial implication of estimating and making inference based on a linear model. One should note however, that x_{it-1} always has the same marginal effect on y_{it} in a linear model, regardless of the initial value of x_{it-1}. Assumption (A2) is also quite simple. Needless to say, the data used for inference should reflect the population. I discuss potential problems with missing data in chapter 5. Assumption (A3) of no collinearity is needed to identify the regression parameters. However, the correlation matrix Table B.3 in appendix B leaves little room for concerns about multicollinearity problems in this analysis. The key assumption is (A4). This assumption implies two things. First it implies that none of the explanatory variables in equation (4.1) co-vary with the residual, i.e. that

$$Cov(x_{it-1}, u_{it}) = Cov(c_{1it-1}, u_{it}) = \ldots = Cov(c_{Kit-1}, u_{it}) = 0.$$

Second it implies that the expected unconditional residual is zero, i.e. that

$$E(u_{it}) = 0.$$

These two restrictions are crucial. To see this, consider a simple version of equation (4.1) without control variables (these are abstracted from for the moment to ease the

notation):

$$y_{it} = \beta_0 + \beta_1 x_{it-1} + u_{it}$$

β_1, the population parameter of interest in this equation, can be expressed as follows (see e.g. Wooldridge (2009) for derivations of the expressions below):

$$\beta_1 = \frac{Cov(x_{it-1}, y_{it})}{Var(x_{it-1})} \tag{4.2}$$

The OLS estimator on the other hand, denoted $\hat{\beta}_1$ can be derived as

$$\hat{\beta}_1 = \beta_1 + \frac{Cov(x_{it-1}, u_{it})}{Var(x_{it-1})}. \tag{4.3}$$

When comparing (4.2) and (4.3), we see that the OLS estimator is unbiased only if $Cov(x_{it-1}, u_{it}) = 0$. This is the sense in which assumption (A.4) is crucial for the regression results to be valid.

If assumptions (A1)-(A4) are met, then estimation of (4.1) by means of OLS yields unbiased estimates of the parameters in the model. If assumptions (A5)-(A6) hold, then estimation of (4.1) by means of OLS yields unbiased standard errors as well. I try to take these two latter requirements into account by estimating Huber-White robust standard errors clustered around countries. The rest of this chapter presents strategies that aim at fulfilling (A.4).

4.2 Fixed-effects regression

A concern when using country-year data is that country-specific factors which do not vary over time, affect explanatory variables as well as the dependent variable. Theoretically it can exist a vast number of such factors, making it impossible to include them all in the estimated model. The sum of all country-specific factors that do not vary over time is represented in equation (4.1) by α_i. From this equation we realize that (A.4) is satisfied only if

$$E(\alpha_i | x_{it-1}, \{c_{kit-1}\}_{k=1}^{K}) = 0,$$

which implies that the fixed effect α_i must be uncorrelated with all the explanatory variables. This is unlikely. For instance, geographical location might affect both a country's level of democracy and its economic level. If this is the case, then leav-

ing geographical location out of the regression equation (4.1) might lead to omitted variable bias. However, country-specific factors can be controlled for by means of the *fixed-effects* estimator (Verbeek, 2008, 359). One approach is to include a dummy variable for each country i. However, this substantially reduces the degrees of freedom as the number of regressors increases. Transforming the data prior to the estimation to eliminate α_i all together is therefore an easier way to control for the problem (Wooldridge, 2009, 481). Again, consider equation (4.1), this time without control variables and with the decomposed error term:

$$y_{it} = \beta_0 + \beta_1 x_{it-1} + \alpha_i + \varepsilon_{it} \tag{4.4}$$

Remember that α_i is the constant country-specific factor while ε_{it} is the white noise residual. For country i, the average of this equation over time is

$$\bar{y}_i = \beta_0 + \beta_1 \bar{x}_i + \alpha_i + \bar{\varepsilon}_i, \tag{4.5}$$

where $\bar{y}_i = T^{-1} \sum_{t=1}^{T} y_{it}$, $\bar{x}_i = T^{-1} \sum_{t=1}^{T} x_{it}$ and $\bar{\varepsilon}_i = T^{-1} \sum_{t=1}^{T} \varepsilon_{it}$ denote the (time) demeaned variables. Because α_i is constant over time, it is present in both equations (4.4) and (4.5).[2] The fixed-effects transformation implies subtracting equation (4.5) from equation (4.4). For each t the transformation gives

$$y_{it} - \bar{y}_i = \beta_1 (x_{it-1} - \bar{x}_i) + \varepsilon_{it} - \bar{\varepsilon}_i \tag{4.6}$$

It is evident in equation (4.6) that the unobserved country-specific effect α_i has disappeared. Also, assumption (A.4) is now reduced to

$$E\left((\varepsilon_{it} - \bar{\varepsilon}_i) \mid (x_{it-1} - \bar{x}_i), \{c_{kit-1} - \bar{c}_{ki}\}_{k=1}^{K} \right) = 0.$$

Accordingly, equation (4.6) can now be estimated consistently by ordinary least squares methods (given that ε_{it} is uncorrelated with the explanatory variables) (Wooldridge, 2009, 482). This is the sense in which the fixed-effects estimator allows *arbitrary* correlation between α_i and the explanatory variables.[3] The parameters in fixed effects regressions are identified only through the within dimension of the data (Verbeek, 2008, 362). However, when interpreting the results, it is important to realize that the

[2] Note that $\bar{\alpha}_i = T^{-1} \sum_{t=1}^{T} \alpha_i = \alpha_i$.

[3] Note that variables that do not vary over time, such as colonial past, cannot be included in fixed-effects regressions as these are transformed away.

linearity of the model allows us to interpret coefficients in exactly the same way as the OLS estimates.

Factors that are time specific, but do not vary across countries, might also affect democratization and explanatory variables simultaneously. I will control for such factors by including time dummy variables in the regression equations, effectively estimating *time fixed-effects* regressions.

Running fixed-effects regressions sometimes comes at the cost of considerable loss of variation in the data, since the between dimension of the data is removed. Less variation leads to higher estimated standard errors, making it harder to prove significant relationships (Green, Kim and Yoon, 2001). A Hausman specification test can give an indication of whether fixed effects should be controlled for. It tests' whether estimates from one estimator are significantly different from another (Hausman, 1978). H_0 being that the estimates do not differ systematically from each other. First, I compare the estimates from an OLS estimator and a fixed-effects estimator that controls for country-specific effects. Systematic estimate difference is likely to be the result of the latter controlling for country-specific effects while the former does not. The null of no systematic estimate difference is rejected at any reasonable significance level, p-value being 0.0002. Furthermore, when comparing an OLS estimator with a fixed-effects estimator that controls for time, the null is also rejected, p-value being 0.0000. Consequently, both country and time fixed effects will be controlled for in the main analysis.

4.3 Time series persistence

In time series models, one is typically concerned with possible non-stationarity in the variables and so-called unit roots. These data features lead to biased estimates if not taken into account (Wooldridge, 2009, 393). This section explains the unit root problem and suggest how one can deal with the issue. Consider an autoregressive process of order one (an AR(1)):

$$x_{it} = \rho x_{it-1} + u_{it}^{x} \tag{4.7}$$

x_{it} is the variable's value in period t while ρ is an autoregressive coefficient assumed to take a value from zero to one. $u_{it}^{x} \sim IID(0, \sigma_u^2)$ is a white noise residual. Equation (4.7) is said to contain unit root if $\rho = 1$. In this case the time series is typically refereed to as a *random walk process*. To illustrate the implications of a unit root, I

repeatedly substitute lagged levels of x into (4.7). The result is

$$\begin{aligned} x_{it} &= \rho\left(\rho x_{it-2} + u^x_{it-1}\right) + u^x_{it} = \rho^2 x_{it-2} + \rho u^x_{it-1} + u^x_{it} \\ &= \rho^2\left(\rho x_{it-3} + u^x_{it-2}\right) + \rho u^x_{it-1} + u^x_{it} = \rho^3 x_{it-3} + \rho^2 u^x_{it-2} + \rho u^x_{it-1} + u^x_{it} \\ &= \dots \\ &= \rho^\infty x_{i0} + \sum_{j=0}^{\infty} \rho^j u^x_{it-j} \\ &= \sum_{j=0}^{\infty} \rho^j u^x_{it-j}, \end{aligned} \tag{4.8}$$

where the last line follows from the assumption that $x_{i0} = 0$. Equation (4.8) states that the value on x in period t, x_{it}, is equal to a weighted sum of all past shocks, where the shock j periods ago is denoted u^x_{it-j}. The weight on u^x_{it-j} is ρ^j. This equation has a number of implications. First, it demonstrates that the effect of an increase in u^x_{it-j} by one unit implies an increase in x_{it} by ρ^j units. Second, that effect is higher when ρ is a higher number.[4] Thus, a higher value on ρ implies that shocks to u^x take longer time before they die out (implying persistence in the series). Third, if $\rho = 1$, then shocks never die out. To see this, note that $\rho = 1$ implies that equation (4.7) becomes

$$x_{it} = x_{it-1} + u^x_{it} \tag{4.9}$$

while equation (4.8) can be written

$$x_{it} = \sum_{j=0}^{\infty} u^x_{it-j}. \tag{4.10}$$

The last result follows from the observation that $\rho^j = 1$ for any j whenever $\rho = 1$. If $\rho = 1$ (unit root), then inclusion of x_{it} in regression models can result in biased estimates. To see why, note that estimation involves the calculation of moments such as the mean and the variance, e.g. $E(x_{it})$ and $Var(x_{it})$. The mean is found by taking the expectation on both sides of (4.10):[5]

$$E(x_{it}) = E\left(\sum_{j=0}^{\infty} u^x_{it-j}\right) = \sum_{j=0}^{\infty} E\left(u^x_{it-j}\right) = 0$$

[4] As an example, suppose $j = 2$ and compare the cases $\rho = 0.5$ and $\rho = 0.1$. The former gives $\rho^j = 0.5^2 = 0.25$ while the latter gives $\rho^j = 0.1^2 = 0.01$.

[5] See e.g. Wooldridge (2009, 724-728) for details on how to derive the mean and the variance.

The last equality follow from the assumption that the white noise residual u_{it}^x has mean zero. The variance is derived from (4.10) and our finding that $E(x_{it}) = 0$:[6]

$$\begin{aligned} Var(x_{it}) &= E\left[(x_{it} - E(x_{it}))^2\right] = E\left[(x_{it})^2\right] \\ &= E\left[\left(\sum_{j=0}^{\infty} u_{it-j}^x\right)^2\right] = E\left[\sum_{j=0}^{\infty}\left(u_{it-j}^x\right)^2\right] = \sum_{j=0}^{\infty}\sigma_u^2 \end{aligned}$$

Clearly this expression depends on time. In fact, it increases as a linear function of j, and goes to infinity as $j \to \infty$. Thus, if $\rho = 1$ in (4.7) then the variance of x_{it} is not well defined, and the constant variance assumption that panel data estimators are based upon is violated. Failing to take this into account will yield biased estimates, and this is the problem of including a unit root series in the regression model (Wooldridge, 2009, 393).

To test for a unit root in (4.7), one can do the following: Subtract x_{it-1} on both sides to get

$$\Delta x_{it} = x_{it} - x_{it-1} = (\rho - 1)\, x_{it-1} + u_{it}^x = \rho^x x_{it-1} + u_{it}^x,$$

where $\rho^x = \rho - 1$. Note that $\rho^x = 0$ if $\rho = 1$, and that $\rho^x < 0$ if $\rho < 1$. Thus, one can simply estimate this equation by OLS, and test the null hypothesis that $\rho^x = 0$ versus the alternative $\rho^x < 0$. Presence of unit roots in the time-series for income and democracy is tested for in section 6.2. There I find evidence of a unit root in both series. To take this into account, I follow standard procedure and use the first difference of the variables. Then equation (4.7) becomes $\Delta x_{it} = u_{it}^x$, implying that the unit root series has been transformed into a stationary white noise process. I do the same transformation on both income and democracy. Consequently, it is the effect of economic growth on changes in democracy that will be estimated in the empirical analysis.[7]

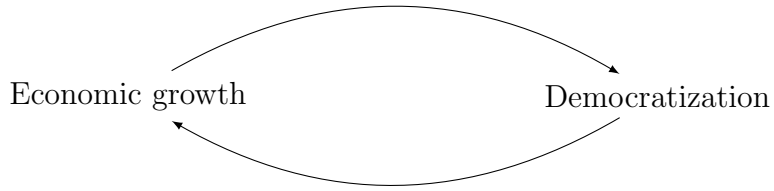
4.4 Reversed causality

Reversed causality (simultaneity) is a sub-category of the endogenity problem, which implies that one or more of the explanatory variables are correlated with the error term. There is simultaneity in the data if at least one of the explanatory variables x

[6] The second last equality uses that the variance of a sum is equal to the sum of the variances as long as the covariance between terms is zero.

[7] The first difference of income is the growth rate because the logarithm of income is being used.

has an impact on the dependent variable y, at the same time as the dependent variable y affects the explanatory variable x (Verbeek, 2008, 138). In particular, while it can be argued that economic growth affects democratization, in accordance with the working thesis presented here, it can also be claimed that democratization affects economic growth. Egypt following the Arab spring illustrates the reversed causality between economic growth and democratization. Following the revolution in January 2011, the country experienced a sharp reduction in GDP growth rate to 1 percent in 2011 from almost 5 percent in 2010 (Mishrif, 2012, 4).[8] Accordingly, previous research indicates that democratization reduces economic growth in the short term, while it stimulates growth in a longer perspective (Papaioannou and Siourounis, 2008). Democracies often have more secured property rights, which may lead to more investments and production, that further stimulates economic growth.[9] Simultaneity between economic growth and democratization can be illustrated in the following way:



If the data used in the analysis reflects such simultaneity, then the explanatory variable is partly explained by variation in democracy, which is a violation of assumption (A.4), i.e. that of exogenous variables (Wooldridge, 2009). If not controlled for, this simultaneity problem implies that a standard OLS-analysis would generate biased estimates that might lead to wrong policy conclusions. In section 7.3 I test for simultaneity between income growth and democracy and find that it cannot be ruled out.

Several previous studies have attempted to address the simultaneity problem by using lagged economic growth (or level) as explanatory variable (e.g. Muller (1995), Barro (1999)). By lagging the economic growth variable, it is determined that the growth occurred before the democratization. However, in order for this approach to fix the simultaneity problem, one must assume that economic actors cannot anticipate

[8]Several factors related to the revolution, can explain the reduced growth. Egypt experienced reduced tourism, FDI inflows and export revenues. As a result of inadequate response to these challenges Egypt has furthermore seen a sharp increase in inflation and unemployment (Mishrif, 2012, 5).

[9]Accordingly, the results from an extremely simple regression analysis (no control variables) using the dataset I have constructed for this thesis, indicate that democratization (positive change in democracy) has a negative effect on economic growth (coefficient: - 0.139, t-value: - 2.05), while democratic level seems to have a positive effect on growth (coefficient: 0.068, t-value: 2.94). These results however, are no longer significant when country and time fixed effects are controlled for.

democratization and adjust their behavior accordingly. This is a very strong assumption. Regime change is often the result of a gradual change in the society, which implies that economic actors can foresee the regime change and adjust their behavior accordingly. Hence, using a lagged economic growth variable does not provide a convincing solution to the simultaneity problem.

In this thesis I use an instrumental variables strategy to address simultaneity between economic growth and democratization. This approach controls for simultaneity *if* the instruments chosen are exogenous. The instrumental variables estimators applied in the econometric analysis are reviewed next.

4.4.1 Two Stage Least Squares

Instrumental variables (IV) methods can be used to solve endogeneity problems of one or more of the explanatory variables (Wooldridge, 2009, 506). Here, instrumental variables are used to obtain exogenous variation in the explanatory variable. The exogenous variation is then used to establish a causal effect on the dependent variable. Two fix things, we once again consider equation (4.1) without controls:

$$y_{it} = \beta_0 + \beta_1 x_{it-1} + u_{it}$$

As simultaneity between y_{it} and x_{it-1} implies $Cov(x_{it-1}, u_{it})$, i.e. violation of (A.4), the standard OLS estimator cannot be used. The two stage least squares (2SLS) estimator instead takes as point of departure an auxiliary regression model:

$$x_{it} = \phi_0 + \phi_1 z_{it} + r_{it} \tag{4.11}$$

Here, x_{it} is the explanatory variable that is vulnerable to simultaneity in equation (4.1), while z_{it} is a so-called instrument variable. ϕ_0 and ϕ_1 are parameters to be estimated, and $r_{it} \sim IID(0, \sigma_r^2)$ is a white noise residual. Equation (4.11) is typically referred to as the first stage model, for reasons that will become clear in a second. In principle the 2SLS procedure amounts to:

1. Estimate equation (4.11) in order to obtain predicted values for x_{it}, labeled $\hat{x}_{it}$.
2. Estimate $y_{it} = \beta_0 + \beta_1^{iv} \hat{x}_{it-1} + u_{it}$, where $\hat{x}_{it-1}$ are the predicted values from the first stage regression and β_1^{iv} denotes the (instrumented) parameter of interest.

These two stages have given rise to the name two stage least squares. One can show that the 2SLS estimator, denoted $\hat{\beta}_1^{iv}$, is given by (see Wooldridge (2009, 510) for

details):

$$\hat{\beta}_1^{iv} = \beta_1 + \frac{Cov(z_{it-1}, u_{it})}{Cov(z_{it-1}, x_{it-1})}. \tag{4.12}$$

When using the 2SLS estimator, global economic growth and climatic natural disasters will be used to instrument (local) economic growth. Equation (4.12) tells us that there are two requirements for an instrument to be valid:

1. The instrument z must be *relevant* in the sense that it is correlated with the explanatory variable economic growth, i.e. $Cov(z_{it}, x_{it}) \neq 0$.

2. The instrument z must be *exogenous* to democratization in the sense that it is uncorrelated with the error term u, i.e. $Cov(z_{it-1}, u_{it}) = 0$.

The first condition makes it possible to obtain variation in the explanatory variable, and requires that $\phi_1 \neq 0$ in (4.11). The second condition stresses that this variation needs to be exogenous. In particular, (4.12) illustrates that $Cov(z_{it-1}, u_{it}) = 0$ is a necessary condition for the 2SLS estimator to be unbiased. If the two conditions above hold, then the instrumented variation can be used to establish a *causal* effect of economic growth on democratization.

Given the restrictions outlined above, the 2SLS method makes it possible to estimate the causal effect from economic growth on democratization. Nevertheless, using an instrument-variable approach is not unproblematic. If the instruments used are weak, by either being correlated with the error term, or only being weakly correlated with the endogenous explanatory variable, then 2SLS estimates can be worse than OLS estimates (Wooldridge, 2009, 536). Furthermore, even when an IV approach is appropriate, 2SLS estimates always have higher variance than OLS estimates (Wooldridge, 2009, 523-524). The reason is that $\hat{x}$, by construction, has less variation than x. The variation in x is the total sum of squares, while the variation in $\hat{x}$ is only the explained sum of squares from the first stage regression. This makes it more difficult to find significant effects. The researcher therefore faces a tradeoff between efficient but possibly biased OLS estimates, and inefficient, but unbiased 2SLS estimates.

4.4.2 Arellano-Bond GMM

By instrumenting economic growth in the 2SLS estimator, simultaneity between economic growth and democratization is addressed. However, in the econometric analysis regime duration, corruption, oil dependency, civil conflict onset, income inequality and education level are included as controls to achieve more robust results. There might

also be simultaneity between democratization and some of these control variables (e.g. corruption, inequality, education). If simultaneity between *one* explanatory variable and democratization is not controlled for, all regression estimates can be biased and inconsistent (Verbeek, 2008, 140). Thus, for the estimated effect of economic growth on democratization to be reliable, possible simultaneity problems for all explanatory variables should be controlled for.

This could be achieved by instrumenting all the explanatory variables in a 2SLS model. However, it is often challenging to find valid instruments for all variables. Arellano and Bond (1991) have developed an alternative approach by constructing a GMM estimator. They show that instruments can be obtained in a dynamic panel data model if one utilizes independent (orthogonality) conditions that exist between lagged values of the explanatory variable and the error term (Baltagi, 2008, 149). The estimator was originally developed in a setting with lagged dependent variables as controls. Although I do not have a lagged dependent variable, I will still use a similar approach to identify a causal effect of economic growth. To see how, consider again the dynamic linear model from (4.1) without controls:

$$y_{it} = \beta x_{it-1} + u_{it}$$

y_{it} is democratization, x_{it-1} is lagged economic growth and needs to be instrumented, and the error term is $u_{it} = \alpha_i + \varepsilon_{it}$. As before, α_i is the country fixed effect while $\varepsilon_{it} \sim IID(0, \sigma_\varepsilon^2)$ is the white noise residual. By assuming a white noise residual we are by assumption ruling out the possibility of serial correlation in levels. Now an auxiliary assumption is made, namely that economic growth follows an AR(1) process[10]:

$$x_{it} = \rho x_{it-1} + \nu_{it}, \tag{4.13}$$

where $|\rho| \in (0, 1)$. It is also assumed that $\nu_{it} \sim IID(0, \sigma_\nu^2)$ (white noise). When inspecting equations (4.1) and (4.13), it is seen that the possible simultaneity between x_{it-1} and y_{it} in (4.1) can be accounted for by using x_{it-2} as an instrument. This instrument is valid as long as $\rho \neq 0$.

In my case the Arellano-Bond GMM estimator can be derived as follows: First, equation (4.1) is first differenced to eliminate country specific effects:

$$y_{it} - y_{it-1} = \beta(x_{it-1} - x_{it-2}) + (\varepsilon_{it} - \varepsilon_{it-1}) \tag{4.14}$$

[10] An AR(1) process implies that there is first order serial correlation, i.e. the value of the variable this year depends on the variables' value last year.

Now, constant country-specific effects are controlled for. In particular, the error term only consists of the white noise residual (in first differences). However, the simultaneity problem still remains unsolved. The first period in which an instrument variable can be used is $t = 4$, where

$$y_{i4} - y_{i3} = \beta(x_{i3} - x_{i2}) + (\varepsilon_{i4} - \varepsilon_{i3})$$

Here x_{i1} is a valid instrument because (4.13) tells us it is correlated with $(x_{i3} - x_{i2})$. At the same time, x_{i1} is not correlated with $(\varepsilon_{i4} - \varepsilon_{i3})$ (as long as ε_{it} is not serially correlated, which it by assumption is not). Anderson and Hsiao (1981) developed a GMM instrumental variables estimator similar to this, in which only *one* lagged version of the variable is used as an instrument. However, Arellano and Bover (1995) show through Monte Carlo evidence that the Andersen-Hsiao estimator can have large biases and large standard errors, especially when ρ is close to 1 (Verbeek, 2008, 379). What Arellano and Bond (1991) argue is that the list of instruments should be extended by exploiting additional moment conditions and let the number of instruments vary with t (Verbeek, 2008, 380). To see how this works, consider the next period, $t = 5$. Here we get

$$y_{i5} - y_{i4} = \beta(x_{i4} - x_{i3}) + (\varepsilon_{i5} - \varepsilon_{i4})$$

Now, the autoregressive structure of (4.13) implies that both x_{i2} and x_{i1} are valid instruments for $(x_{i4} - x_{i3})$. To see that x_{i1} is a valid instrument, one can solve (4.13) backwards from $t = 3$:

$$x_{i3} = \rho x_{i2} + \nu_{i3} = \rho\left(\rho x_{i1} + \nu_{i2}\right) + \nu_{i3} = \rho^2 x_{i1} + \rho\nu_{i2} + \nu_{i3}$$

Now we have an equation where x_{i3} is determined by both x_{i1} (the first term on the right hand side) and x_{i2} (the second term on the right hand side). Furthermore, the instruments are by assumption not correlated with $(\varepsilon_{i4} - \varepsilon_{i3})$. This procedure can be continued, adding an extra valid instrument with each forward period. Hence, in any generic period t, the set of valid instruments is $(x_{i1}, x_{i2}, \ldots, x_{it-2})$ (Baltagi, 2008, 149).

The strength of the Arellano-Bond GMM estimator for this thesis is first and foremost that it addresses possible simultaneity problems between all the explanatory variables and democratization. Furthermore constant country-specific effects are also controlled for. However, a common concern raised by Windmeijer (2005) and others, is that the method takes advantage of too many instruments, which can result in model

overfitting.[11] A model that includes more instruments than variables that are instrumented is always overidentified. The question is whether the instruments strengthen the estimation by adding relevant information. However, the Sargan and Hansen tests are tests for overidentification. They are reported in the regression tables. I follow the results from these tests when deciding upon how many instruments (lags) to include in the Arellano-Bond GMM estimations. In that manner I reduce the probability of overfitting the model.

In this chapter I have reviewed the estimators that will be applied in the main analysis. Moreover, I have pointed out why time-series persistence and simultaneity might be a problem, and reviewed possible solutions to these challenges. In the next chapter I review the data used in the main analysis.

[11] Overfitting occurs when a statistical model describes random error or noise instead of the underlying relationship. It often occurs when a model is excessively complex, by for instance including too many instruments. Such a model often has poor predictive power, as it can exaggerate minor fluctuations in the data (Wooldridge, 2009).

Chapter 5

Data

In order to study the effect of economic growth on democratization, I have constructed a dataset with observations on 168 countries from 1980-2007.[1] Total number of observations is 4406. Raw data are taken from several datasets and the original sources are cited when the variables are presented. The constructed dataset is unbalanced. This implies that there are missing values for certain country-year observations. Potential problems with an unbalanced panel are discussed in section 5.5.

In the rest of this chapter I describe the variables and the data. First, the democratization measurement is reviewed. Second, I outline the measure for economic growth. Third, the control variables are presented. Forth, I review descriptive statistics for all variables included in the main analysis. Finally, potential challenges related to missing data and measurement error are discussed.

5.1 Dependent variable: change in democracy

Yearly change in Polity2 is the dependent variable in the analysis. The Polity2 variable is taken from the Polity IV dataset (Marshall, Gurr and Jaggers, 2012). Polity2 is an index ranging from -10, denoting least democratic, to 10, denoting most democratic. By taking the first difference of the variable, yearly changes in Polity2 is measured.[2]

Polity2 is the difference between a country's score on the autocracy and democracy indicator, which are also included in the Polity IV dataset. These indicators are derived from codings on the following five dimensions. First, *competitiveness of executive recruitment*. It refers to whether the chief executive is democratically elected or rather

[1] 1980-2007 will be the time-series for the main analysis. Hoverer as a robustness test of the results the time-series will be extended.

[2] The variable is first differentiated because it cannot be ruled out that Polity2 contain unit root in level. The challenge of unit roots and why first differencing the variable fixes the problem is discussed in detail in section 4.3 and 6.2.

selected based on heritage or designation. Second, *openness of executive recruitment.* The process is coded as open if political active individuals have the opportunity (in principle), to get elected through a regularized process. Third, *constraints on the chief executive.* This concerns whether there are institutionalized constrains on decisions taken by the executive. Forth, *competitiveness of political participation.* It refers to whether citizens have the opportunity to express their preferences for alternative policies and leadership in the political arena. For a country to be defined as having political competition there must be a significant degree of civil interaction in the political process. Finally, *regulation of participation.* Participation is coded as regulated if there are binding rules for when, whether and how political preferences are expressed.[3]

The five dimensions that make up Polity2 are well suited for capturing the first two criteria I defined as decisive for democracy in section 1.3. I repeat the criteria here for convenience. i) There must be institutions and procedures through which citizens can express their preferences about political leaders and alternative policies. ii) There must be institutional constrains on the power of the executive. The third criteria however, namely that iii) citizens must be guaranteed civil and political rights, is not captured directly by the Polity2 variable. However, these factors are at least to some extent measured indirectly. The reason is that citizens must have civil and political rights for countries to obtain a high Polity2 score. For instance, without political rights such as the right to vote and run for political office, one cannot have competitive elections. Furthermore, without civil liberties such as freedom of expression and assembly, countries will get low scores on the competitiveness of political participation dimension. Nevertheless, as a robustness test of the regression results, the civil liberties and political rights index from Freedom House will be used as an alternative dependent variable.

While it is a weakness that Polity2 does not directly incorporate codings on civil liberties and political rights, Polity2 has some evident strengths. First of all, it has good coverage. Second, as observable formal institutions are the main focus, different biases related to the coders subjective judgment is reduced (Knutsen, 2011, 91). A further strength with the Polity2 measure is its transparency. The Polity IV project publish the indicator scores and have transparent weighing procedures (Munck and Verkuilen, 2002, 19-20). This makes it possible to critically asses the reliability of each component of the measure. It is a further strength that the five dimensions are weighted differently based on "importance". These strengths lead me to prefer the Polity2 measure of democracy over other available measurements.

[3]See Marshall, Gurr and Jaggers (2011, 14-16,21-28) for more information about the dimensions and how they are weighted.

5.2 Explanatory variable: economic growth

The main explanatory variable in the analysis is short term economic growth. That is the yearly growth rate in real GDP per capita (in terms of purchasing power parity). The variable is constructed based on the economic level variable from Penn World Table version 7.1 (Heston, Summers and Aten, 2012). Economic growth is used over economic level due to possible presence of unit roots in the time-series.[4] Economic growth is defined as:

$$\frac{(GDP/capita)_{it} - (GDP/capita)_{it-1}}{(GDP/capita)_{it-1}} \times 100.$$

This way of measuring economic growth is equivalent to taking the first difference of logged economic level, see Wooldridge (2009, 393). The variable has been multiplied by hundred to enable interpretation of the variable directly in percent points. I include a lagged version of the variable in the regression as it is reasonable to assume that it takes a while before economic growth affects democratization.[5] I use *real* rather than nominal GDP because it is measured in constant prices. This implies two things. First, that real GDP is adjusted for changes in purchasing power over time within a country, i.e. it has been adjusted for inflation.[6] Second, that real GDP is adjusted for changes in purchasing power across countries. Thus, purchasing power parity is adjusted for, by taking into consideration that 100 dollars are worth more in a poor country than it is in a rich one at a given time. The growth variable is furthermore measured in per capita because interest lies in the country's wealth rather than absolute GDP level.[7] A central objective in this thesis is to address simultaneity between income growth and democratization in a proper manner. Therefore, instruments are used to measure the effect of short term economic growth on democratization. The 2SLS instruments are reviewed next.

5.2.1 Instruments for the 2SLS estimator

Climatic natural disasters and global economic growth are used as instruments for (local) economic growth in the 2SLS estimator. I have already reviewed the requirements for an instrument to be valid, but repeat them here for convenience. First, the instru-

[4] See sections 4.3 and 6.2.

[5] All the control variables in the analysis are lagged for the same reason. The argument for using lagged variables will not be repeated.

[6] Inflation is adjusted for by deflating GDP against prices in 2005 (which is the reference year).

[7] Penn World Table reports Real GDP based on different techniques. I use economic data that is calculated with the Laspeyre 2 technique.

ment must be relevant in the sense that it is correlated with economic growth. The rule of thumb is that instruments with a t-value of 3 or higher are relevant (Staiger and Stock, 1997). Second, the instrument must be exogenous to democratization, in the sense that it is not correlated with the error term. In other words the instrument cannot affect democratization through other factors than the variables included in the regression model. While the first requirement can be tested for the second one cannot.

The *Climatic natural disasters* instrument stems from Bergholt and Lujala (2012).[8] Bergholt and Lujala (2012) find that climatic natural disasters are a valid instrument for economic growth for the whole world sample. The reason is that all types of countries experience climatic natural disasters. Furthermore, climatic disasters are expected to negatively affect economic growth, and therefore pick up variation in the country's economic growth. At the same time, it is very unlikely that climatic disasters directly affect democratization in a given country. The researchers only include storms and floods in their measure of climatic natural disasters, as these disasters typically have rapid onsets and disappear within a month. This strengthens the instrument's exogenity.[9] The researchers control for the fact that how a disaster affects national income is likely to depend on both the magnitude of the disaster and time elapsed since the event took place. Magnitude is accounted for by normalizing the size of an event by dividing the size of the affected population by total country population. Furthermore, a disaster event happening in January is likely to have a larger effect on countries annual economic growth than an event happening in December, which most likely will have a larger effect on the country's growth the following year. To address this problem, the researchers weigh the time elapsed since the event using the devaluation rate $(12 - \text{event month}_{jit})/12$. Where j is natural disaster in country i in year t.[10]

Figure 5.1 plots climatic natural disasters against economic growth. We see that most country-year observations do not have a climatic disaster incidence. Furthermore, observations with disaster incidences appear to have relatively low economic growth. Thus, the instrument seems to pick up variation in economic growth. Whether it picks up enough variation in economic growth however, will be revealed in the first stage of the 2SLS regression.

[8]Original disaster data stems from the Emergency Events Database (EM-DAT). For an event to be included in the EM-DAT database it must meet one of the following criteria: 10 or more casualties, 100 or more people affected, deceleration of a state emergency, or call for international assistance.

[9]EM-DAT includes information of the number of people killed and people affected by the event. Bergholt and Lujala (2012) use number of people affected as their main variable of interest.

[10]For more information about the instrument see Bergholt and Lujala (2012).

Figure 5.1: Climatic natural disasters and economic growth, 1980-2007

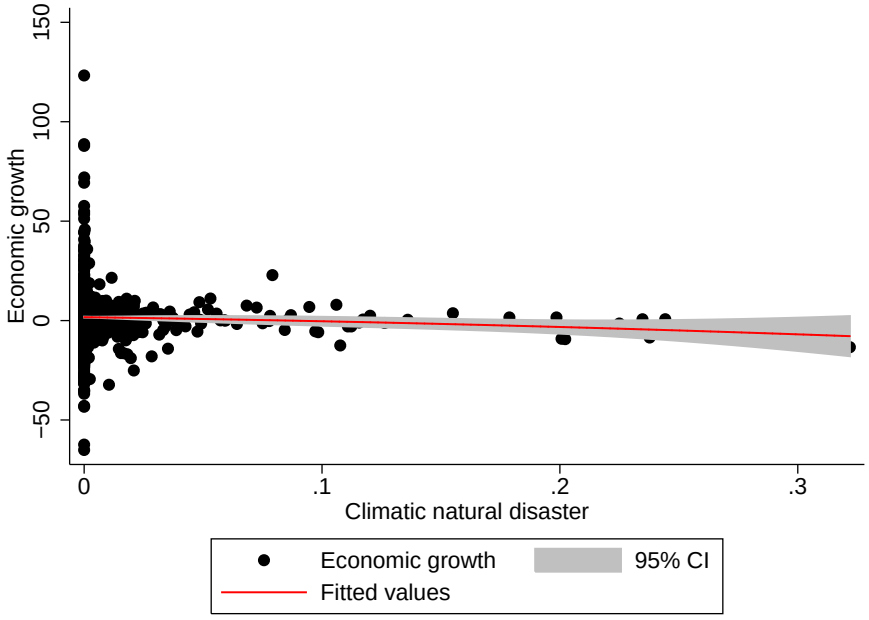
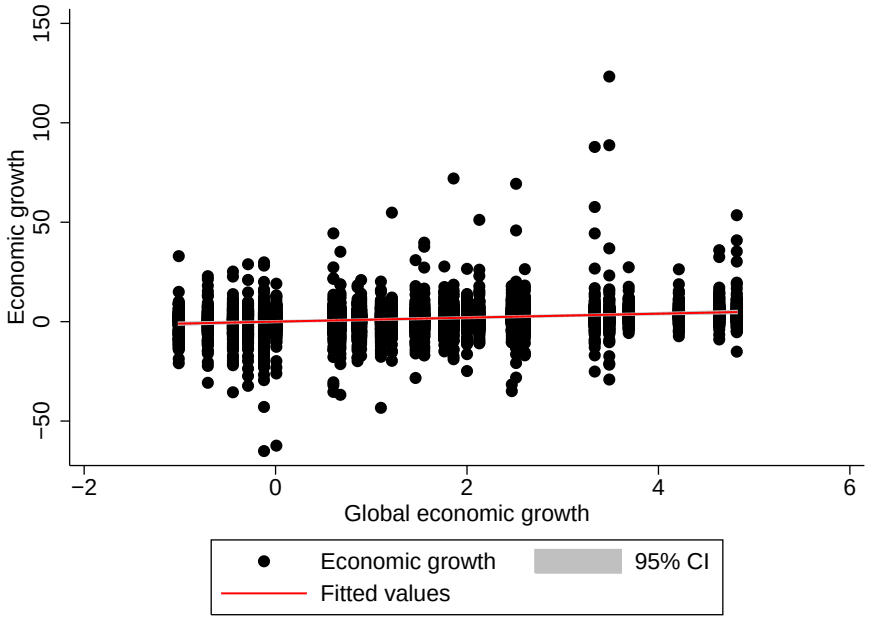


Figure 5.2: Global economic growth and local economic growth, 1980-2007



I constructed the *global economic growth* instrument by taking the average global growth rate each year, based on the economic growth variable reviewed in the previous section. Global economic growth is a plausible instrument for growth in gross domestic product because global growth is likely to affect countries' economic growth. Business

cycles across countries typically move together as they trade with each other, making the instrument relevant. Furthermore, unlike national economic growth, it is hard to see how global economic growth can have a direct impact on democratization in a given country. Rather, it seems likely that global economic growth affects democratization in a given country through its effect on the local economic growth. Figure 5.2 plots global economic growth against countries economic growth. From the figure we see that when global economic growth is low most countries have "normal" local growth. However, in country-years where global growth is high there are also some extreme local economic growth observations.

5.3 Control variables

Control variables are included in the regression analysis for two reasons. First, to explain variation in democracy. Second, to avoid omitted variable bias. I include a number of controls that are widely used in the democratization literature.

First, I control for *duration* of the current regime. The count variable stems from the Polity IV dataset (Marshall, Gurr and Jaggers, 2012). The variable either counts the number of years since the most recent regime change (defined as a three point change in polity score within the three last years), or the number of years since the end of a transition period (defined by the lack of stable political institutions) (Marshall, Gurr and Jaggers, 2011, 17).[11] Previous research indicates that regimes survival rate increases with time. Svolik (2009, 479) for instance, finds that the likelihood that a leader will be removed from office by a coup initially increases before it clearly decreases after some time.

Second, I control for political *corruption*. Political corruption is defined as abuse of public office for private gain (Shleifer and Vishny, 1993).[12] The annual political corruption index is taken from the International Country Risk Guide (ICRG, nd*b*). It ranges from 0 to 6, where 6 denotes a political system *without* political corruption. The corruption index is based on codings on financial corruption, extensive patronage, nepotism, job reservations, favor-for-favors, secret party funding and surprisingly close ties between politics and business (ICRG, nd*a*, 4-5).[13] Hegre and Fjelde (2011), study

[11] When calculating the durable variable, the first year with a new polity is coded as the baseline zero value. All subsequent years adds 1 to value of the variable (Marshall, Gurr and Jaggers, 2011, 17).

[12] For more nuanced definitions of the concept see Hegre and Fjelde (2011).

[13] ICRG corruption index is based on assessments by country experts. Thus, it is as all other corruption data, subjective. The ICRG corruption measure is applied in this thesis because it has the best coverage for cross-section time-series analysis (Hegre and Fjelde, 2011).

the effect of political corruption on the probability of regime transition and regime duration. They find that political corruption strengthens the durability of autocratic and semi-autocratic regimes (Hegre and Fjelde, 2011, 4). Political leaders in such regimes convert public resources into private privileges for some, and in that manner build informal support. As a result the leaders increase their probability of survival, and therefore also the regime's durability.

Third, I control for *oil* as portion of GDP. The variable is taken from the World Bank (2012). The variable is the difference between the value of crude oil production at world prices and total costs of production. Previous research has argued that income generated from natural resources such as oil, can create less pressure for democratization then income that stems from human and physical capital (Barro, 1999, 164).[14] The reason is that governments become less indebted to the population when national income stems from natural resources. Barro (1999, 165) accordingly finds that being an oil exporting country has a negative effect on democracy. Hegre, Knutsen and Rød (2012) on the other hand, do not find robust evidence for that abundance of natural resources reduces the probability of democratization.

Fourth, I control for *intrastate conflict*. The onset of intrastate state conflict variable originates from the UCDP/PRIO dataset (Themnér and Wallensteen, 2012). It is a dummy variable and takes the value 1 in all country-years with a new conflict or if more than two years have passed since the last observation of the conflict. In both cases there must have been at least 25 battle related deaths. Otherwise, the variable takes the value 0. Conflict can be damaging for democratization. Collier (1999, 169) points out that during conflict, civil liberties are often diminished while rule of law breaks down. Military forces get increased power at the expense of the police and law system. This can obviously be damaging for democratic institutions, at least in the short run. Conflict can furthermore, have a devastating effect on economic growth, through destruction of human life and capital (Collier, 1999, 169).

Fifth, I include investment share of GDP as a crude proxy for *economic inequality*. To the extent that capital owners hold a major share of GDP, and at the same time there are significant wealth differences between those who own capital and others, the investment share of GDP can proxy relative wealth differences within a country. The reason is that capital formation is achieved through investment.[15] The variable

[14] See also Ross (2001).

[15] A substantial part of previous statistical research focusing on economic inequality uses the gini-coefficient from Deininger and Squire (1996). A weakness with this measurement is poor data coverage. Time-series are short and many countries are not included. Capital share has also been used as a measure for economic inequality. Though also a crude measure, it can be argued that capital share is a stronger proxy for income equality than investment share because it also takes into consideration

is taken from the Penn World Table 7.1 dataset (Heston, Summers and Aten, 2012). Boix and Stokes (2003, 539) argue that democratization is not triggered by higher economic level in itself. Instead, democratization is caused by other changes that accompany development, particularly lower levels of income inequality. Their claim is that the elite has less (little) to lose by democratizing when income equality is high (Boix and Stokes, 2003, 539-540). The elite find a democratic tax structure less expensive for them as the country becomes richer and are therefore more willing to democratize. Boix and Stokes (2003, 540) find that higher levels of income equality have a positive effect on democracy. Hegre, Knutsen and Rød (2012, 20) however, do not find the income inequality (measured by the gini-index) to be systematically related to democratization.

Sixth, I control for *education*. The original variable stems from the World Bank (2012). The variable is the total number of students enrolled in public and private primary education institutions. The original variable does not account for the number of students relative to total country population. In order to do so, I have generated a new variable that is number of primary education pupils divided by total population. In general, more developed societies have a higher educated population. Lipset (1959) argued that higher levels of education would both increase the citizens' desire and capacity to claim democracy. Furthermore, higher educated citizens, was argued to be more suited to participate in the democratic proses. Accordingly, Hegre, Knutsen and Rød (2012, 19) find that education has a positive and robust effect on democratization. This is in accordance with Barro's (1999, 158) finding that the propensity for democracy raises with primary schooling.

5.4 Descriptive statistics

Table 5.1 summarizes descriptive statistics for all variables included in the analysis for 1980-2007. The dependent variable, change in Polity2, has a mean of 0.167. This implies that changes in Polity2 more often are towards democracy than autocracy. The most negative change in Polity2 a country has experienced from one year to the next is -15. This observation is Gambia 1994. The extreme negative democratization was caused by a military coup lead by Yahya Jammeh in 1994 (French, 1994). The coup overthrew the democratically elected president and banned political activity, taking the country from a democratic Polity2 score of 8 in 1993 to an autocratic score of -7 in 1994.

wages.

Table 5.1: Summary statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Polity2	1.455	7.274	-10	10	4129
Change in Polity2	0.167	1.722	-15	16	4097
Economic growth	1.698	7.411	-65.076	123.267	4326
Climatic disaster	0.002	0.014	0	0.322	4251
Global economic growth	1.681	1.616	-1.011	4.819	4406
Regime duration	23.042	29.172	0	198	4188
Corruption	3.092	1.399	0	6	3026
Oil rents of GDP	6.711	13.819	0	105.95	3316
Civil conflict onset	0.035	0.183	0	1	4403
Income inequality (log)	2.969	0.556	-0.368	4.418	4324
Primary education	0.118	0.047	0.023	0.29	3682

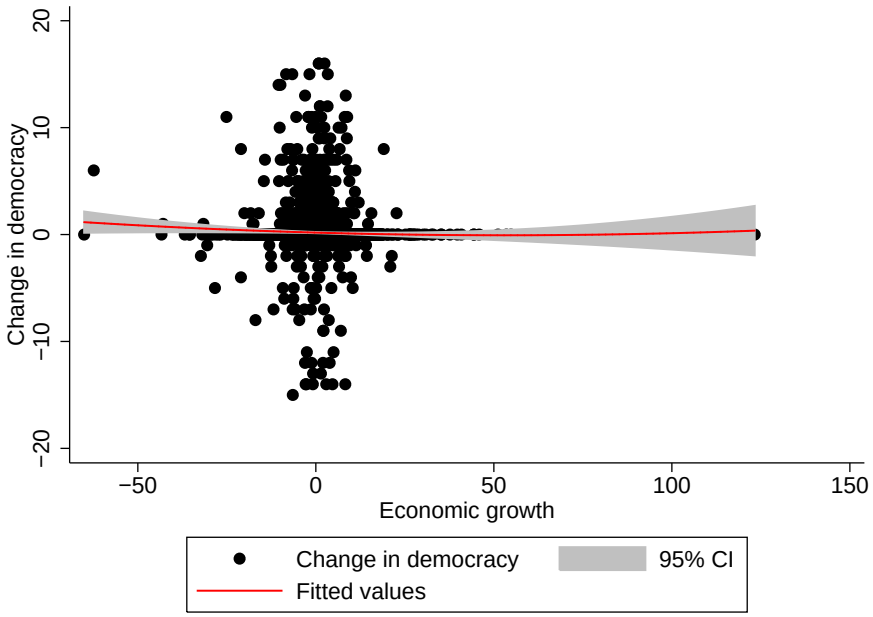
The most positive change in Polity2 a country has seen from one year to the next is 16. There are three country-year observations that take this value. First, Argentina's Polity2 score was increased from -8 in 1982, to 8 in 1983. Second, Uruguay went from a Polity2 score of -7 in 1984 to 9 in 1985. Finally, Panama's Polity2 score was increased from -8 in 1988, to 8 in 1989. These countries have several commonalities. First of all, they were all democratic countries in the 1960s. Then, military coups rendered the countries autocratic in the late 1960s and early 1970s. Finally, the increase in Polity2 of 16 occurred when the countries returned to democracy during the third wave of democratization (Remmer, 1992).[16]

The main explanatory variable, economic growth has a mean of 1.70. The standard deviation is 7.41. The lowest value a country has obtained on the economic growth variable is -65.08 percent points. This observation is Iraq 1991. The severely negative growth was the result of economic mismanagement and wars (Alnasrawi, 1994, 151). In particular, the negative growth occurred in the aftermath of the Gulf war (where Iraq invaded Kuwait), after which the international community launched a series of economic sanctions against the country. From Table 5.1 we see that the most positive economic growth a country has seen is 123.27. This country-year observation is Equatorial Guinea in 1997. The massive growth was the result of discovery of large oil reserves in 1996 (Blum, 2004).

[16]In Argentina and Uruguay the autocratic regimes were dissolved as a result of internal pressures. In Panama however, the democratization followed an invasion by the United States in 1980 (Remmer, 1992).

Notably, both the level of income and democracy display an upward sloping trend. This is illustrated in Figure 1.1, but also in the descriptive statistics because average changes in the variables are positive. That observation might signal non-stationarity in the (level) variables, and can result in spurious estimates if not accounted for. This is yet another motivation for first differencing income and democracy before estimation. The first difference of a variable that moves around a linear time trend will render it stationary, see (Wooldridge, 2009, 393) for details. Figure 5.3 plots economic growth against change in Polity2.

Figure 5.3: Economic growth and first differenced Polity2, 1980-2007



Clearly, the upward sloping trend in Figure 1.1 is now removed. Moreover, Figure 5.3 displays a slightly negative relationship between the two variables in first differences. Yet, as nothing else is controlled for, the figure does not inform us about causal relationships.

5.5 Missing data and measurement error

The dataset I have constructed is as mentioned initially in this chapter, unbalanced. This implies that there are missing values for certain country-year observations. The main reason for why the dataset is unbalanced is that data are taken from several sources. Raw datasets have included different countries and have different time-series. For instance, while the Polity IV dataset ranges from 1800 until 2011, the ICRG dataset

goes from 1984 to 2012. The dataset is also unbalanced because countries have entered the dataset at different times. For instance, several Eastern European countries for natural reasons entered the datasets in the beginning of the 1990s. To limit the challenge of an unbalanced dataset I have limited the time-series in the analysis to range from 1980 to 2007. Limiting the time-series in this manner is also likely to improve the data quality, as older data is likely to be less credible. It should be noted however, that limiting the time-series in this manner favors short term effects over long term effects. Even though missing values are never beneficial, it poses a limited problem if the missing values are random. Then, as a consequence of reduced sample size, the standard deviation increases which makes it harder to prove significant effects (Wooldridge, 2009, 322). If on the other hand, there are systemic missing values, i.e. the missing values are correlated with the dependent variable, the estimates can turn out biased. If for instance governments in authoritarian states are worse at reporting their economic data then more democratic states, then authoritarian states will be underrepresented in the sample. If there at the same time is a negative effect of economic growth on democratization, then the missing values are systemic, and the regression results will be biased upwards (towards zero). The effect of economic growth on democratization will in other words appear to be a weaker than what it actually is.

The use of lagged variables (both for explanatory variables and instruments) amplifies the number of missing values. The reason is that one loses the same number of observations of the variable that the number of years it has been lagged by. For instance, when economic growth is lagged by one year, the first economic growth observation in the time-series is dropped from the regression. If economic growth was lagged by two years, the two first economic growth observations would be excluded etc.[17]

Table B.1 in appendix B, provides an overview of percentage missing values for relevant variables. It illustrates that the coverage on the main variables in the analysis, economic growth and democratization is good. Out of 4406 country-year observations, Polity2 has 277 missing, which implies that 6.29 of the variable's observations are missing. The economic growth variable only has 2.59 percent missing observations. From Table B.1 we see that three control variables have a relatively high number of missing observations. First, the corruption variable has 31.32 percent missing observations. Second, oil rents of GDP, has 24.74 percent missing observations. Finally, primary education has 16.43 percent missing.[18] When control variables are included

[17]Furthermore, when using lagged values, countries that have holes in their time-series, will lose both this observation and the number of observations that the variable has been lagged by.

[18]It is not surprising that the data coverage on ICRG and World Bank data are more limited than

in the regressions, the number of observations included is reduced by almost one half. This implies that the different variables have missing values on different country-year observations. As a robustness test, I will exclude the control variables with the highest amount of missing observations, to see whether it alters the results.

Imputation is one method to address missing data. It implies replacing missing observations with probable values based on other available information (Honaker and King, 2010, 561). However, imputation is not unproblematic. One is generating observations where one in reality does not have data. Imputation is furthermore not a good method for addressing missing observations in my dataset because the missing data consists mainly of entire time series. For instance, it makes little sense to imputate values on variables for Eastern European countries before they existed. More generally, because the dataset consists of country-year observations, imputation can be challenging as different factors are likely to be decisive for economic growth in Norway and Zimbabwe. Thus, using identical variables to generate data on the economic growth variable may not make a lot of sense.

Measurement error denotes the difference between the observed variable and the variable that is included the regression equation (Wooldridge, 2009, 842). Measurement error leads to biased regression estimates if the error is correlated with the observed explanatory variable (Wooldridge, 2009, 319). Underreporting of economic growth is one possible source of measurement error. Foreign aid can for some (poor) countries be an incentive to do so. If it is the case that poor countries have lower levels of democracy, this implies that the measurement error is systemic. The reason is that it will appear like poor countries have a lower growth than what they actually have, at the same time as they have a lower probability of democratization. Then, the estimated effect of economic growth on democratization is biased downwards, i.e. it appears stronger (more negative) than the true value. Another possible source of measurement error is preconceived perceptions about democracy levels in certain countries. If for instance the Polity2 coders have a perception that African counties are less democratic than other countries all else being equal, it would lead to systemic measurement error, with downwards bias. Possible measurement errors and systemic missing observations must be taken into consideration when conclusions are drawn from the regression analysis.

Now that the theoretical framework, econometric methods and data has been presented, it is time to turn to the regression analysis. In chapter 6 I run an introductory analysis by replicating the most common finding in the literature, namely a positive

for instance Polity and Penn world table.

effect of income level on level of democracy. Then, I demonstrate that problems of time-series persistence and simultaneity cannot be ruled out. These econometric concerns are addressed in the main analysis in chapter 7. It is furthermore tested whether the results from the benchmark regressions are robust. Finally, in chapter 8 I inquire whether the effect of income growth on democratization matters more for some countries than others.

Chapter 6

Introductory analysis

In this chapter I do three things. First, I use ordinary least squares (OLS) methods to replicate the most prominent finding in previous research, namely a positive effect of income level on level of democracy. Second, I perform panel data tests for unit roots in both of these variables. Third, I perform two different tests for simultaneity. The central message in this chapter is that the data seems to contain both unit roots and simultaneity between the key variables. Ignoring these issues is likely to result in seriously biased estimates.

6.1 OLS estimation of level effects

Previous attempts to identify a causal effect of income on democracy might suffer from problems with i) unit root in the series and ii) simultaneity between variables. To demonstrate how these issues matter, I start the econometric part of the thesis by ignoring both issues. That is, in this section I replicate the common finding in previous research of a positive effect of income on democracy. This is achieved by using OLS methods (which assume no simultaneity) to estimate the effect of *the level* of income (GDP per capita) on *the level* of democracy (Polity2). In particular I parameterize equations (4.4) and (4.6), extended with a battery of controls. Results are reported in Table 6.1. Models 1 and 2 replicate standard pooled OLS regressions (equation (4.4)). In model 3 time dummy variables are also included. Models 4 and 5 control for country fixed effects (equation (4.6)). In model 6 both time and country fixed effects are controlled for.

The results in models 1 through 5 reproduce the well-documented positive relationship between log income and democracy found in e.g. Barro (1999), Epstein et al. (2006) and Moral-Benito and Bartolucci (2011). A one percent increase in income

level is predicted to go together with a substantial increase in democracy level, even though the size of the income coefficient varies between the models.[1]

Table 6.1: OLS 1980-2007

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	OLS	OLS	OLS	OLS FE	OLS FE	OLS FE
Economic level (t-1) (log)	2.35***	2.02***	1.76***	1.50*	6.66***	0.78
	(6.99)	(3.02)	(2.75)	(1.68)	(5.01)	(0.64)
Regime duration (t-1)		-0.012	-0.018		-0.19***	-0.19***
		(-1.10)	(-1.64)		(-5.45)	(-5.16)
Corruption (t-1)		0.83***	1.43***		-0.049	0.31**
		(2.71)	(4.01)		(-0.29)	(2.13)
Oil rents of GDP (t-1)		-0.27***	-0.27***		-0.027	-0.054**
		(-5.47)	(-5.97)		(-1.08)	(-2.04)
Civil conflict onset (t-1)		1.93	2.32**		-0.36	-0.13
		(1.55)	(2.00)		(-0.95)	(-0.46)
Income inequality (t-1)		0.49	0.22		-1.00	-0.64
		(0.38)	(0.17)		(-1.41)	(-0.85)
Primary education (t-1)		-1.03	7.70		-8.16	10.1
		(-0.12)	(0.96)		(-0.67)	(1.05)
R-squared	0.183	0.410	0.467	0.008	0.264	0.469
Observations	4019	2034	2034	4019	2034	2034
Number of countries	160	115	115	160	115	115
Country FE	No	No	No	Yes	Yes	Yes
Year FE	No	No	Yes	No	No	Yes

Notes: Dependent variable is Polity2. Robust t-statistics clustered on countries in parentheses. Significance levels are denoted by *** p<0.01, ** p<0.05, * p<0.1.

This finding is also in accordance with Figure 1.1 in the introduction chapter that illustrates the positive correlation between income and democracy. While inclusion of country fixed effects in model 5 increases the point estimate, the positive coefficient disappears all together when controlling for time fixed effects in model 6. Notably, Acemoglu et al. (2008) use time fixed effects throughout when they argue that the income effect is non-existent. While these authors point to fixed country effects as

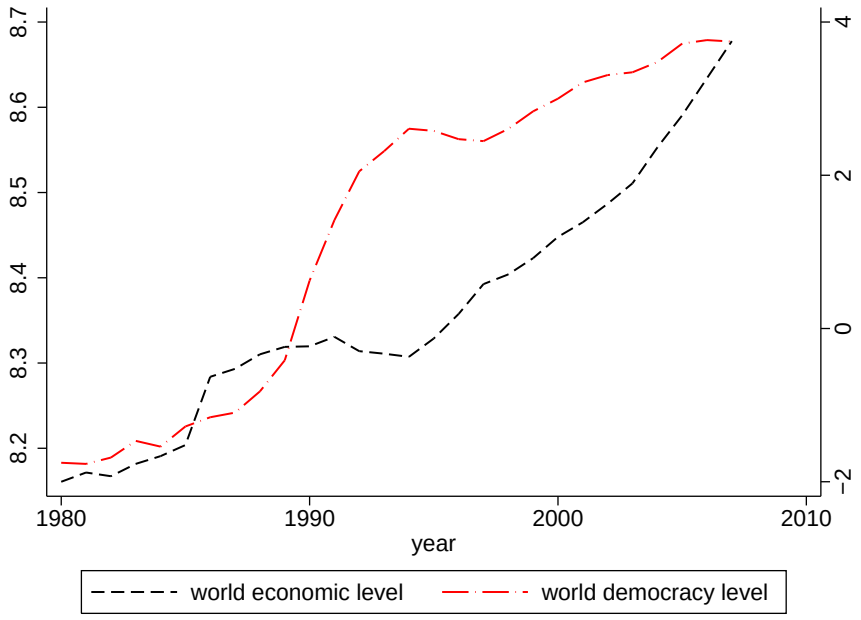
[1]As the purpose of this section is to replicate the common finding of a positive effect of income on democracy, control variables and other features with the regressions will not be discussed.

crucial, the results presented here might indicate that fixed year effects are more important. Note that the year dummies effectively control for the upward sloping trend in Figure 1.1. Still, if income and democracy level contain unit roots and/or there is simultaneity between the variables, all results in Table 6.1 will remain biased. In the following sections I test for these econometric concerns.

6.2 Unit root tests

The concern for unit roots in income and democracy levels is raised by the observation that both series seem to be highly persistent. The series furthermore seem to be growing with time. Combined, these observations raise the question of whether the series are non-stationary with upward trends. Figure 6.1 plots the World economic income level (measured as the cross-sectional mean of observed log real GDP per capita, along the left axis) and World democracy level (measured as the cross-sectional mean of observed polity levels, along the right axis) over time.[2]

Figure 6.1: World income and democracy levels over time, 1980-2007



From Figure 6.1 we clearly see that both series have upward trends. However, whether the processes contain unit roots depends on whether they come back to the upward

[2]Since this figure just plots the unweighted average of cross-country observations, it severely understates the importance of countries with large populations. For instance, a small (but rich and democratic) country such as Norway is given the same weight as a large (but poor and less democratic) country like Pakistan. Nevertheless, the figure is at least an attempt to illustrate some overall time series properties of the two key variables. It should also be noted that controlling for country fixed effects in the regressions should take (most of) this bias into account.

linear trend following shocks in income and democracy levels. It is not clear from the figure that they do. Thus, tests are carried out to determine whether it can be ruled out that the variables' time series contain unit roots. The upward trends in both series are accounted for in the tests. In particular, it is tested for whether $\rho = 1$ in a version of equation (4.7) in which a linear time trend is included:

$$x_{it} = \rho x_{it-1} + \nu t + u_{it}^{x}$$

Here, t denotes the linear trend while ν is its growth rate. Note that this equation is panel specific. Thus, the unit root test amounts to check whether $\rho = 1$ in each of the panels, and then construct a summary statistic that combines the results from all the panel specific tests. The null hypothesis is that $\rho = 1 \; \forall \; i$.

Table 6.2: Unit root test for Polity2 and GDP/capita

	Statistic	P-value
Polity2		
Inverse chi-squared (318) P	335.720	0.237
Inverse normal Z	0.174	0.569
Inverse logit t (599) L*	-1.6982	0.045
Modified inv. chi-squared Pm	0.703	0.241
Real GDP per capita (log)		
Inverse chi-squared (332) P	390.053	0.015
Inverse normal Z	2.632	0.996
Inverse logit t (819) L*	2.370	0.991
Modified inv. chi-squared Pm	2.253	0.012

Notes: This is a Fisher-type unit-root test for Polity2 and logged Real GDP per capita respectively. It is based on augmented Dickey-Fuller tests. H_0 is that all panels contain unit roots. Number of panels is 161 for Polity2 and 167 for logged income level. AR parameters are panel-specific. Panel means and time trends are included.

Table 6.2 provides the results from the unit root tests. Results from four different estimators are reported.[3] These differ in the way they construct the p-values, see Choi

[3]Some of this information is taken from the Stata user manual. I refer to the section explaining the command `xtunitroot` for further details regarding panel unit root tests.

(2001) for details. The P statistic can be used when the number of panels is finite. The Pm statistic on the other hand is a modified version that is suitable when the number of panels tend to infinity. The Z statistic and the L* statistic can be used in either cases. The most interesting numbers are in the last column, as these are the p-values. A p-value lower than 0.05 implies rejection of the null hypothesis at a 5 percentage level. Evidently, three out of four unit root tests do not reject the hypothesis that there are unit roots in all panels for Polity2. For logged real GDP per capita, two tests reject H_0. Note that H_0 is a very strong claim, as it states that there is a unit root in *all* panels. It is enough that some panels contain unit roots to make regression estimates biased. Thus, it cannot be reject that neither Polity2 nor logged real GDP per capita are random walk processes. Furthermore, Choi (2001) finds that the Z statistic offers the best combination of size and power in a simulation exercise.[4]

To make myself perfectly clear, I am not claiming that Polity2 and logged real GDP per capita necessarily contain unit roots. I am however, emphasizing that the possibility of unit roots cannot be ruled out. Thus, in order to avoid unreliable regression estimates, the possible presence of unit roots is controlled for. Fortunately, taking the first difference of the variables, fixes the problem of persistent time-series (Wooldridge, 2009, 393). The reason is that after first differencing a variable that contain unit root, all that is left is the white noise component of the variable. This component is per definition independent of x_{it-1}. Accordingly, the variable is now stationary rather than persistent.

In sum, possible presence of a unit root implies that both democracy and income level need to be first differenced in the econometric model. Thus, I will study the effect of economic growth on changes in democracy. However, as long as the econometric model is correctly specified and the unit root results are valid, the empirical estimates should reflect the level effects in the theory model. To see why, consider once again the linear equation $y_{it} = \beta x_{it-1}$. Under the assumption that this equation is true in all periods, including $t-1$, then also $y_{it-1} = \beta x_{it-2}$ must hold. Subtracting one from the other yields the difference equation $\Delta y_{it} = \beta \Delta x_{it-1}$, where β is exactly the same coefficient as before. Thus, if the level equation is true, one can always estimate the difference equation and interpret the result in level terms.

[4]An alternative to the Dickey-Fuller specification in the Fischer unit root tests is the Phillips-Perron option. Using that specification does not alter the results (not shown).

6.3 Two tests of simultaneity

Granger causality and Hausman specification tests are carried out to get an indication of whether there is simultaneity between economic growth and change in democracy. Granger causality is a statistical concept of causality based on prediction. In particular, tests are carried out to determine whether one time series is useful for forecasting another. A time series x is said to Granger cause y if it can be shown through a series of t-tests and F-tests on lagged values of x, that those values provide statistically significant information about future values of y. Granger (1969) argues that simultaneity between two variables can be determined by testing for such predictability in both directions.[5] However, one should note that Granger causality does not imply causality in the standard sense. Thus, it is an indication, not a proof, of true causality.[6]

For my case the Granger causality test amounts to i) testing whether economic growth Granger causes changes in democracy, and ii) testing whether changes in democracy Granger causes economic growth. Granger causality in both directions is consistent with a true causal simultaneity (although not a necessary condition). Concretely, in the first part of the test I estimate the following regression, where y_{it} and x_{it} now stand for democratization and economic growth, respectively:[7]

$$y_{it} = \beta_1 y_{it-1} + \ldots + \beta_p y_{it-p} + \gamma_1 x_{it-1} + \ldots + \gamma_q x_{it-q} + u_{it}$$

Then, a F-test is carried out to determine whether economic growth provides significant information about democracy. Here, H_0 of no Granger causality implies $\gamma_1 = \ldots = \gamma_q = 0$. H_1 on the other hand, is that x Granger causes y.

Second, it is tested whether y Granger causes x. Now the following regression is estimated:

$$x_{it} = \alpha_1 x_{it-1} + \ldots + \alpha_p x_{it-p} + \sigma_1 y_{it-1} + \ldots + \sigma_q y_{it-q} + u_{it}$$

Now, H_0 of no Granger causality amounts to $\sigma_1 = \ldots = \sigma_q = 0$. H_1 is that y Granger causes x. I set the number of lags (q) to 3 in both regressions.

A Hausman specification test can also give an indication of whether there is simul-

[5] Granger also shows that the simultaneity mechanism can be considered the sum of the two causal mechanisms (Granger, 1969, 438).

[6] After all, Granger causality is only a statistical statement about correlations across time. One can think of other variables that cause variation in both x and y, i.e. the classical concern of omitted variables. See Wooldridge (2009, 650) for a critical discussion of the concept Granger causality.

[7] Note the slight abuse of notation compared to earlier expressions, where y_{it} and x_{it} denoted variables in levels.

taneity in the data. It tests' whether estimates from one estimator are significantly different from another (Hausman, 1978). H_0 is that estimates do not differ systematically from each other. I compare the estimates from an OLS fixed effects estimator with the estimates from an Arellano Bond GMM estimator. As the latter addresses simultaneity problems in the data while the former does not, differences in estimates indicates that there might be simultaneity in the data. Table 6.3 reports the results from the Granger causality and the Hausman specification tests.

Table 6.3: Statistical tests of simultaneity

Null hypothesis	F-stat	P-value	Observations
X does not Granger cause Y	6.34	0.096	3937
Y does not Granger cause X	6.48	0.091	3936
Hausman, Difference not systemic	13.55	0.059	2033/1805

Notes: Granger causality tests based on an Arellano-Bond GMM estimator with 3 lags. X denotes economic growth, Y denotes changes in democracy. The reported F-stat is a Wald-type test of the joint significance of all estimated coefficients on the lags. Hausman specification test compares estimates from an OLS estimator with an Arellano Bond GMM estimator. Both estimators includes control variables and control for country fixed effects. Robust standard errors are clustered on countries.

The results from the Granger Causality tests reject H_0 at the ten percentage level in both cases. Thus, the data are weakly supportive of Granger causality in both directions, and thereby provide some indication of simultaneity between economic growth and democratization. I also carried out the Granger causality test based on an OLS estimator without robust standard errors. Then, H_0 of no Granger causality is rejected even at a one percentage level (result not shown). Accordingly, Fayad et al. (2012) find that H_0 of no Granger causality is rejected at a one percent level when they test for simultaneity between income and democracy levels. The results from the Hausman specification test in Table 6.3 furthermore shows that it is rejected at a six percent level that the estimates from the OLS estimator and the Arellano-Bond GMM estimator does not differ systematically from each other. This is a further indication that there might be simultaneity in the data. Overall, the statistical tests seem supportive of simultaneity between economic growth and democratic development. Thus, instrumental variables methods are applied throughout the main analysis to avoid simultaneity bias in the estimates.

The central message from this chapter is that the data seems to contain both unit

roots and simultaneity between key variables. Thus, in order to avoid unreliable estimates in the main analysis, these issues must be addressed. Unit roots are controlled for by first differencing income and democracy. Simultaneity is addressed by using instrumental variable strategies. The following chapter presents the main analysis.

Chapter 7

Main analysis

This chapter reports the results from the main econometric analysis. It consists of three parts. First I present the benchmark estimation results when using the following estimators: i) pooled and fixed effects ordinary least squares (OLS and OLS FE), ii) two stage least squares (2SLS), and iii) Arellano-Bond GMM (AB-GMM). The choice of these estimators is motivated in the methodology chapter. Second I offer a broader discussion of main findings. Third, I present a comprehensive battery of robustness tests. The key message in this chapter is that once unit roots and simultaneity issues are accounted for, income seems to have a negative causal effect on democracy. This finding dramatically contrasts the conventional wisdom in previous literature and in policy debates.

7.1 Benchmark results

7.1.1 Ordinary least squares

Table 7.1 summarizes the results from pooled OLS and OLS FE regressions. The econometric specification is similar to the models estimated in Table 6.1. That is, I reestimate the equations (4.4) (pooled OLS) and (4.6) (OLS FE) (supplemented with control variables), except that income and democracy are now used in first differences.[1] Thus, the objective of Table 7.1 is to demonstrate how results change when the presence of unit root is taken into account.[2]

[1] A comment is in place: time dummies are not included in neither of the regression models in Table 7.1. This renders the results comparable with the 2SLS estimates in Table 7.2. Time dummies cannot be included in the 2SLS estimation because global economic growth is used as an instrument.

[2] Importantly, simultaneity between economic growth and democratization remains uncontrolled for.

Table 7.1: OLS 1980-2007

VARIABLES	(1) OLS	(2) OLS	(3) OLS FE	(4) OLS FE
Economic growth (t-1)	-0.0032	-0.020***	-0.0013	-0.017**
	(-0.84)	(-3.02)	(-0.32)	(-2.16)
Regime duration (t-1)		-0.0012*		0.018***
		(-1.73)		(3.16)
Corruption (t-1)		-0.079***		-0.075
		(-2.86)		(-1.34)
Oil rents of GDP (t-1)		-0.0084***		-0.0097**
		(-4.40)		(-2.24)
Civil conflict onset (t-1)		-0.048		0.014
		(-0.26)		(0.065)
Income inequality (t-1) (log)		-0.11		-0.42**
		(-1.01)		(-2.08)
Primary education (t-1)		-0.84		2.65
		(-1.15)		(1.00)
R-squared	0.000	0.016	0.000	0.018
Observations	4031	2033	4031	2033
Number of countries	159	115	159	115
Country FE	No	No	Yes	Yes
Year FE	No	No	No	No

Notes: Dependent variable is first differenced Polity2. Robust t-statistics clustered on countries in parentheses. Significance levels are denoted by *** p<0.01, ** p<0.05, * p<0.1.

Consider first the results in models 1 and 2. The parameter of interest is negative, but far from significant in model 1. In model 2 however, the negative coefficient is significant even at a one percentage level. These results stand in sharp contrast to their counterparts when using variables in levels, i.e. models 1 and 2 in Table 6.1. However, the controls comes with a significant reduction in the number of observations. I do not know whether the significant coefficient in model 2 is due to variation captured by the controls, or to the exclusion of observations. Just as in Table 6.1, failing to control for fixed effects might lead to biased estimates. Models 3 and 4 address this concern. Again, the point estimates turn negative once unit roots are controlled for by means of first differencing. In particular they challenge the results reported in models 4 and 5

in Table 6.1. Also, inclusion of the standard set of controls (model (4)) leads to highly significant estimates. Overall, these results represent a first step towards the central message in this thesis, namely that the link between income and democracy might be more involved than commonly held views suggest.

How can one interpret the coefficients reported here? Take the point estimate in model 4, which arguably is the most reliable (due to control for both time varying regressors and fixed effects). It can be interpreted as follows: when a country's economic growth rate increases by one percentage point (say, from 1 to 2 percent), then that country is predicted to experience a decline in democratic development equal to 0.017.

Arguably, the results presented here differ quite dramatically from the level equation counterparts in Table 6.1. While the economic *level* coefficient had a positive sign in all models in Table 6.1, the economic *growth* coefficient has a negative sign in all the models in Table 7.1, even though it is only significant when control variables are included. Moreover, the results in Table 6.1 and Table 7.1 combined suggest that first differencing the key variables is far more important for the results than controlling for fixed effects.[3,4] However, at this stage an important remark is in place: even though the estimate of income growth on democratic development is statistically significant in Table 7.1, it is of modest importance quantitatively. For example, an increase in the per capita growth rate equal to one standard deviation (in other words a "typical" increase in economic growth), is predicted to reduce democratic development the next year by $0.017 \times 7.4 = 0.126$. This effect is arguably not very large economically.

For the sake of completeness, I next comment on the estimates of significant control variables. In model 2 regime duration is predicted to create a drop in democracy, but the coefficient is only significant at a ten percentage level. That result is overturned in model 4, where the coefficient is positive even at a one percentage level: when a regime type has aged by one year, the country is predicted to see a positive change in polity of 0.018. Intuitively, this is consistent with the observation that countries have become more democratic over time. The negative point estimate in model 2 might be due to unobserved country specific factors, which bias the coefficient downwards when they are not accounted for. In Table 6.1 the estimated effect of corruption on democracy

[3]The absolute size of the coefficients however, are similar in the growth and level models. This is because the coefficients in Table 6.1 must be divided by 100 to be comparable with those in Table 7.1 (the growth coefficient has been multiplied by 100 to interpret estimates directly in percentage points). For instance, after dividing the estimate in model 2 in Table 6.1 by 100, we get 0.0202. The comparable estimate in model 2 in Table 7.1 is -0.020.

[4]The results reported here are also robust to controlling for random effects. The random effects version of model 4 gives a point estimate equal to -0.020 with t-value equal to -3.02 (results not shown).

was positive and highly significant in most models, a rather counterintuitive result. Here instead, corruption is negatively correlated with democratic development, even though the link is insignificant when controlling for fixed effects. Thus, while Hegre and Fjelde (2011) find that corruption strengthen the durability of autocratic- and semi-autocratic regimes, it here seems that corruption, if anything, pushes countries further towards autocracy. Both models 2 and 4 suggest that oil dependent countries (in terms of oil income as percentage of GDP) have less positive democratic development. This is in accordance with previous research (see e.g. Ross (2001)). One explanation might be that income generated from natural resources such as oil, make the government less indebted to the population than income generated from human and physical capital. Income inequality has a negative sign in both models, but is only significant when country specific factors are controlled for. A negative effect of income inequality is in accordance with Boix's (2003) argument that less inequality increases the probability of democracy.

The results reported in this section demonstrate that popular claims about a positive effect of income on democracy might be premature. Indeed, once the variables are first differenced to control for unit roots, the estimates turn negative. However, these results come with at least two restrictions. First, the negative coefficients in Table 7.1 seem rather small in absolute value from an economic point of view. That is, the policy implications of those coefficients might be limited. Second, and most importantly, the analysis so far has abstracted from any simultaneity problem. This observation renders the current estimates prone to biases caused by reversed causality, especially in light of the introductory simultaneity tests in chapter 6. Thus, we should move on to more advanced econometric techniques that take simultaneity problems into account. This is exactly what I do in the next two sections.

7.1.2 Two stage least squares

Motivated by the methodological discussion in section 4.4.1, I next report the estimates when equation (4.6) (extended with control variables) is parameterized by means of 2SLS. Once again, Polity2 in first differences is the dependent variable. Country fixed effects are controlled for in all models. I do not control for time fixed effects because global economic growth is used as an instrument. Table 7.2 report the main results. As an introductory exercise, I first estimate the effect of economic growth on democratic development without the control variables in models 1 and 2. Model 1 represents the first stage, i.e. effects of the instruments on economic growth. All the test statistics strongly suggest that climatic disasters and global income growth provide relevant

information about the movements in the economic growth rate.

Table 7.2: 2SLS 1980-2007

	(1)	(2)	(3)	(4)
VARIABLES	2SLS(1st)	2SLS(2nd)	2SLS(1st)	2SLS(2nd)
Economic growth (t-1)	–	-0.029*	–	-0.198***
		(-1.74)		(-3.88)
Climatic disaster (t-1)	-18.643***	–	-15.193	–
	(-3.76)		(-1.52)	
Global economic growth (t-1)	0.968***	–	0.623***	–
	(8.57)		(5.58)	
Regime duration (t-1)			-0.003	0.021***
			(-0.19)	(3.04)
Corruption (t-1)			-0.209	-0.180**
			(-1.24)	(-2.52)
Oil rents of GDP (t-1)			0.003	-0.007
			(0.06)	(-0.73)
Civil conflict onset (t-1)			0.114	0.005
			(0.14)	(0.02)
Income inequality (t-1) (log)			2.405***	0.087
			(3.01)	(0.27)
Primary education (t-1)			10.765	2.850
			(1.17)	(0.98)
Kleibergen-Paap F-statistic	41.552***	41.552***	16.505***	16.505***
Hansen test (J-statistic)	0.168	0.168	0.289	0.289
Observations	3,930	3,930	2,019	2,019
Number of countries	155	155	113	113
Country FE	Yes	Yes	Yes	Yes
Year FE	No	No	No	No

Notes: Dependent variable in the first stage regressions is economic growth. Dependent variable in the second stage regressions is first differenced Polity2. Robust t-statistics clustered on countries in parentheses. Significance levels are denoted by *** p<0.01, ** p<0.05, * p<0.1. Kleibergen-Paap Waald F-statistic test for weak identification. H_0: Equation is weakly identified. Hansen test for overidentification. H_0: Too many instruments are not included.

For starters, both instruments have highly significant point estimates with t-values equal to -3.76 and 8.57, respectively. The rule of thumb here is that the t-value should 3 or higher in absolute value. The Kleibergen-Paap F-statistic demonstrates that the instruments are jointly significant at virtually any significance level. Here, the rule is that the F-value should be 10 or higher (Staiger and Stock, 1997). Furthermore,

the Hansen test of the overidentification restrictions cannot reject the null hypothesis that both instruments are valid.

Turning to the estimated coefficients in the first stage, the coefficient of -18.643 is comparable with the estimated effect of climatic disasters on economic growth in Bergholt and Lujala (2012). Intuitively, climatic disasters are destructive for human life and capital, and therefore hamper the potential of economic growth in most economies. Global income growth on the other hand is predicted to induce economic growth within countries: an increase in global income growth by one percentage point stimulates country-level economic growth rate by 0.968 percentage points. This is in line with the vast business cycles literature which documents that global business cycles are important drivers of macroeconomic conditions in most economies.

Given the relevance of the instruments, model 2 represents the second stage when controls are not included. The estimated causal effect of economic growth on democratic development is -0.029, although this result is only significant at a ten percentage level. The point estimate is still far more negative (and more significant) than its counterparts in Table 7.1. The models without controls in that table, models 1 and 3, produced point estimates very close to zero (-0.0032 and -0.0013 respectively). However, compared to models 2 and 4 in Table 7.1, the 2SLS estimates without control variables are only about 50-70 percent more negative.

How do the results change when additional regressors are included? Models 3 and 4 in Table 7.2 present more robust results as these include controls for corruption, oil dependency, civil conflict onset, income inequality, and education levels. Interestingly, the first stage results reported in model 3 reveal that climatic disasters provide much less information now than without controls. Indeed, the t-statistic has declined in absolute value to 1.52, way below the rule of thumb that it should be 3 or higher. This could imply that climatic disasters should be dropped as instrument when control variables are included in the regression. However, the Hansen (J statistic) test does not indicate that neither of the instruments should be left out. Furthermore, the Kleibergen-Paap statistic indicates that the instruments jointly remain strong determinants for economic growth (this latter result is probably driven by the global economic growth instrument). Consequently, both instruments are used.[5]

Turning to the second stage, model 4 points to a negative and highly significant causal effect of economic growth on democratic development. The point estimate is large in absolute value, suggesting that an increase in the income growth rate by one percentage point reduces democratic development by 0.198. The policy relevance of

[5]I also estimated model 4 with only global income growth as instrument, but the coefficients were similar (results not shown).

this estimate is better understood if we calculate the effect of a one standard deviation increase in the explanatory variable (i.e. a "typical" increase given the volatility in economic growth). Model 4 implies that such an increase will create a drop in Polity2 equal to $-0.198 \times 7.4 = -1.46$. This effect is undoubtedly substantial, and it is furthermore statistically significant at a one percent level.[6] Regarding the control variables, only two remain significant. Regime duration still seems to stimulate democratization while the opposite is true for corruption.[7] Overall, these results demonstrate the relevance of controlling for reversed causality, and provide significant support to the idea that income might affect democracy negatively.

An important question that arises from the analysis so far, is *why* the 2SLS estimates are so negative compared to the estimates using OLS and OLS FE. A first step towards answering this question might come about from reviewing the theory behind the OLS estimator. Consider once again the simple regression equation (4.1), which is restated below for convenience (control variables are abstracted from):

$$y_{it} = \beta_0 + \beta_1 x_{it-1} + u_{it}$$

Remember the definitions of β_1, the population parameter of interest, and the OLS estimator $\hat{\beta}_1$. These were given in (4.2) and (4.3) respectively:

$$\beta_1 = \frac{Cov(x_{it-1}, y_{it})}{Var(x_{it-1})}, \qquad \hat{\beta}_1 = \beta_1 + \frac{Cov(x_{it-1}, u_{it})}{Var(x_{it-1})}$$

If $Cov(x_{it-1}, u_{it}) = 0$, then the OLS estimator $\hat{\beta}_1$ represents an accurate approximation to the true population parameter β_1. The interesting question however is what happens to the OLS estimate when $Cov(x_{it-1}, u_{it}) \neq 0$.[8] Note that any variable not included in (4.1), which affects y_{it}, must be part of the residual u_{it}. Acemoglu et al. (2008) provide valuable insight using this observation. In particular they point out that almost all theories in political science, sociology and economics suggest that we have $Cov(x_{it-1}, u_{it}) \geq 0$. In other words, it is hard to think of any factors that are left out of (4.1) (or more importantly its counterpart with controls), that will affect income growth in one direction and democratization in the other. Take for instance the rule of law. It seems likely that more rule of law will have a positive effect on both economic

[6] The results are similar when country fixed effects are not controlled for. When control variables are not included the growth estimate is -0.032 and t-value is -1.98. When control variables are included the growth coefficient is -0.149 and the t-value is -4.07 (results not shown).

[7] 2SLS estimation typically comes at the cost of larger standard deviations compared to OLS, see Wooldridge (2009) for details.

[8] We learned in chapter 4 that this is a violation of (A.4), one of the assumptions needed for consistent estimation of β_1.

growth and democratization. It is hard to see however, how rule of law can have opposite effects on these variables. Thus, if rule of law leads to omitted variable bias when left out of (4.1), then it should lead to $Cov(x_{it-1}, u_{it}) \geq 0$. A similar argument should hold for other omitted variables. Comparing the expressions for β_1 and $\hat{\beta}_1$, we see that this implies

$$\hat{\beta}_1 = \beta_1 + \frac{Cov(x_{it-1}, u_{it})}{Var(x_{it-1})} > \beta_1,$$

i.e. that the OLS estimate is biased upwards. Therefore, estimates from the OLS regressions (including those who control for fixed effects) can be viewed as representing upper bounds of the true causal effect of economic growth on democratization. This seems to be a reasonable explanation for why the 2SLS estimates, which arguably can control for most of the bias, are more negative than the pooled OLS estimates. Notably, also Acemoglu et al. (2008, 817) report considerably more negative estimates when they instrument the economic level (compared to the standard OLS results).

7.1.3 Arellano-Bond GMM

A limitation of the 2SLS estimator is that it requires at least one external instrument per endogenous explanatory variable. If one suspects that several explanatory variables suffer from simultaneity problems or other sources of bias, establishing enough external instruments quickly becomes a daunting task. Furthermore, one can never fully rule out that at least one of the external instruments I have used are endogenous to democratization. These observations motivate the use of an alternative estimator to 2SLS, namely the one proposed by Arellano and Bond (1991). A more detailed discussion of this estimator is offered in the methodology chapter.

Table 7.3 provides the results from Arellano-Bond GMM estimations. Now, all control variables are instrumented by their own lags. Thus, given that the specification is appropriate I control for simultaneity in the entire set of explanatory variables. Change in Polity2 is still the dependent variable. Country fixed effects are removed in the first step of the estimation procedure through first differentiating. Due to the large number of instruments involved in Arellano-Bond GMM estimation, I set up and report a battery of misspecification tests for each model. The Sargan and Hansen tests share as null hypothesis that the overidentification restrictions are valid (i.e. that the instruments are not correlated with the residual term). The Sargan test can handle an arbitrary high number of instruments, but is not robust to heteroskedasticity. The Hansen test on the other hand is robust, but can be weakened by too many

instruments. Thus, I report both. Regarding persistence in the residuals, I test for first and second order autocorrelation. Arellano-Bond estimation of a correctly specified model should, result in first order autocorrelation (in the residuals), but not in second order autocorrelation.

Table 7.3: AB-GMM 1980-2007

VARIABLES	(1) AB	(2) AB	(3) AB	(4) AB
Economic growth (t-1)	-0.026*	-0.055**	-0.044	-0.055**
	(-1.76)	(-2.25)	(-1.30)	(-2.08)
Regime duration (t-1)		0.018		0.073*
		(0.58)		(1.84)
Corruption (t-1)		-0.16		-0.27
		(-1.11)		(-0.83)
Oil rents of GDP (t-1)		-0.015		0.0076
		(-0.59)		(0.24)
Civil conflict onset (t-1)		-0.64		-0.40
		(-0.93)		(-0.61)
Income inequality (t-1) (log)		-0.79		-0.49
		(-0.93)		(-0.56)
Primary eduction (t-1)		26.7*		8.51
		(1.67)		(0.52)
Sargan test	0.749	0.248	0.943	0.346
Hansen test	0.420	0.649	0.778	0.513
AR(1) test	0.000	0.000	0.000	0.000
AR(2) test	0.780	0.830	0.048	0.507
Observations	4,000	1,805	4,000	1,805
Number of countries	159	113	159	113
Country FE	Yes	Yes	Yes	Yes
Year FE	No	No	Yes	Yes

Notes: Dependent variable is first differenced Polity2. Robust t-statistics clustered on countries in parentheses. Significance levels are denoted by *** p<0.01, ** p<0.05, * p<0.1. Sargan and Hansen test for overidentification. H_0: Too many instruments are not included. AR(1) tests for first order serial correlation. AR(2) tests for second order serial correlation. H_0 in both AR tests is that there is not serial correlation.

Model 1 is a preliminary version without any control variables or control for fixed year

effects (international trends). Model 2 adds the standard set of controls to model 1. Model 3 instead extends model 1 with time controls. The full specification is given in model 4, where both the set of standard controls and the time dummies are included. Models 1 and 3 utilize the entire time series of lagged observations as instruments, as suggested by Arellano and Bond (1991). Models 2 and 4 on the other hand limit the number of instruments per variable to 15. This is because the degrees of freedom fall when more instrumented control variables are included. 16 instruments or more break down the inference results, as indicated by the Sargan and Hansen tests (results not reported). Models 2 and 4 can therefore be viewed as hybrid specifications of those proposed by Anderson and Hsiao (1981) and Arellano and Bond (1991) respectively.[9]

Starting with the results in model 1, we see that the estimated causal effect of economic growth on democratic development is negative and significant at the ten percentage level. The point estimate is -0.026, remarkably similar to the corresponding 2SLS estimate (model 3 in Table 7.2), and considerably more negative than the OLS estimates in Table 7.1. Neither the Sargan test nor the Hansen test can reject the null hypothesis that the overidentification restrictions are valid (p-values of 0.749 and 0.420 respectively). Further confidence is gained from the autoregressive tests. The null hypothesis of no first order autocorrelation is rejected at the one percentage level (p-value equal to 0.000), while no indication of second order autocorrelation is found (p-value equal to 0.78). All these specification tests strengthen the estimation result.[10]

Model 2 provides a more negative point estimate of -0.055, significant at the five percentage level. However, in contrast to its counterpart in Table 7.2 (model 4 in that table), none of the control variables are significant. Models 3 reveals that controlling only for time fixed effects in this GMM setting makes the parameter of interest insignificant at the ten percentage level. Still, the point estimate remains relatively stable compared to model 2. Model 4 repeats the story in model 2. That is, the estimated effect of economic growth on democratic development is -0.055 and significant at the five percentage level. The overall impression of the specification tests is that neither the use of instruments nor the dynamic specification seem invalid. The only exception is the AR(2) test in model 3, which suggests that the inference suffer from some higher order autocorrelation.

For sake of completeness I briefly explain the control variables once more. Regime duration still has a positive sign, but is only significant when time fixed effects are

[9] See the methodology chapter for details.

[10] Although I do not comment much on the specification tests from now on, one should note that they are relatively stable across estimations.

controlled for, and only at a ten percentage level. Primary education is significant for the first time at the ten percentage level in model 2. The positive sign is in accordance with Lipset's (1959) modernization hypothesis. However, the result can hardly be said to be robust in my regressions.

The main conclusion from the Arellano-Bond estimations is that economic growth seems to have a negative causal effect on democratization, although the models without control variables seem to struggle somewhat in reproducing this finding. This latter finding is fairly common across estimation techniques. The growth coefficients that are statistically significant however, are still important quantitatively. Take the coefficient in model 4 for instance, which arguably is the most reliable point estimate in Table 7.3. It implies that an increase in the growth rate by one standard deviation leads to $0.055 \times 7.4 = 0.407$ less democratization as measured by the Polity scale. For comparison, the difference in Polity2 score between India and the UK in 2011 was 1.

Interestingly, the estimate in model 4 lies somewhere in between those found using pooled OLS and OLS FE on the one hand, and the prediction generated by 2SLS on the other. The first observation is consistent with the argument made earlier that OLS estimates are biased upwards. The second can be due to a number of things. However, there are important reasons why one should not disregard the Arellano-Bond GMM estimates (compared with 2SLS results). First of all, while any simultaneity between the control variables and democratization is assumed away during 2SLS estimations, these assumptions are relaxed here. Second, the battery of specification tests from Arellano-Bond is largely supportive of both the number of instruments and dynamics. Third, the Arellano-Bond results are robust to the flexible specification of international time trends that is provided by time dummies. Fourth, while the 2SLS estimates vary from -0.03 to -0.20, the Arellano-Bond estimates remain much more stable across specifications. Overall, it might seem reasonable that the true population parameter lies somewhere in between 2SLS results and Arellano-Bond results. Note however, that to the extent that some of the positive covariance between income and democracy ($Cov(x_{it-1}, u_{it})$) remains, both 2SLS and Arellano-Bond still yields upward biased estimates.

7.2 Some thoughts on the main results

Let us take a step back and review what the empirical exercises have produced so far. Most importantly, the results illustrate the importance of taking unit roots and simultaneity between income and democracy into account. Controlling for unit roots

in an OLS setting is sufficient to alter the widely believed positive effect of income on democracy to a negative effect. That in itself is of great importance, as it indicates that economic stimulus to create democracy might fail. At least the negative coefficient warns us against naively running level regressions without considering unit root issues. However, even though the negative coefficient is statistically significant, it is arguably of less economic significance due to upward bias caused by the OLS estimator. Once simultaneity and omitted variable problems are taken seriously, the estimated effect of income on democracy increases substantially in terms of quantitative significance. Yet, even instrumental variables estimators deliver relative diverse point estimates. It is possible that the 2SLS and the Arellano-Bond results represents upper and lower bounds for the true population parameter. In any case, the negative effect of income on democracy suggests that one should think carefully about how income is transferred to poor countries. In particular, one should design wealth transfers in such a way that they do minimal harm to democratic development. At least the benefits of these transfers should be traded of against the costs in terms of higher risks of diverting resources, corruption, etc.

In discussing the benchmark results further, it is instructive to compare model predictions with some real life observations. As the regression estimates represent average effects across the sample, they are not well suited for predicting specific country cases. Thus, I find it more constructive to review some cases that illustrate the predicted effect in a broader sense. Keeping in mind that average economic growth from 1980 to 2007 was 1.69, Venezuela and Russia have that in common that they have had years of high economic growth followed by democratic recession. Regardless of whether it was purely the result of large oil reserves, the Venezuelan economy eventually grew rapidly under the former President Hugo Chavez. From the economic recession in 2003 to 2007, the economy grew by 76 percent (Weisbrot and Sandoval, 2007, 4). At the same time Chavez expanded his own powers. Consequently, in 2006 Venezuela's Polity2 score was reduced from 6 to 5. This trend furthermore continued. While the economic growth advanced, Venezuela's Polity2 level was reduced to -3 by 2009. For the Russian economy, 1998 marked the lowest point of the post-communist downturn. Due to a debt default, the government was forced to devalue the Rouble. However, after this crisis the Russian economy has grown rapidly. Between 1998 and 2007 the average annual growth rate was 7 percent due to raising output and oil prices (Hanson, 2008, 2). At the same time the Kremlin has tightened political constrains. Consequently, Russia saw a decline in its Polity2 level from 6 in 2006, to 4 in 2007.

Since I estimate linear models, the results from the benchmark regressions also

imply that negative economic growth will have a positive effect on democratization. This is consistent with what the economic crises literature claim to be the case for authoritarian regimes (see e.g. Gasiorowski (1995) and Geddes (1999)). In particular, Haggard and Kaufman (1995, 32-33) find that twenty-one out of twenty-seven democratic transitions between 1970 and 1990, occurred in regimes that had experienced declining economic growth.[11] A specific country case where democratic transition occurred in times of economic difficulties is the Philippines. For several years the autocratic President Marcos, received the necessary support from the private-sector elite in the country. This support however, diminished from the beginning of the 1980s due to the pressure from the global economic recession (Teorell, 2010, 74). Triggered by the assassination of Marcos' main political rival Aquino in 1983, public mobilization against the regime developed. As the economic crisis aggravated (negative growth rates of -6.97 in 1984, and -9.99 in 1985), Marcos' legitimacy was further decreased. In an attempt to calm down the masses, Marcos called for snap Presidential elections[12] in 1986. Independent watchdog organizations concluded that an opponent won the election. Yet, the state controlled electoral commission declared Marcos as winner, and thereby helped intensify the protests against the regime. Within days these protests forced Marcos from office, and the opposition candidate came to power (Haggard and Kaufman, 1995, 65). This took the Philippines from a Polity2 score of -6 in 1985, to 1 in 1986. Furthermore, in 1987 the Philippines obtained a new constitution which reinforced constrains on the power of the executive. Consequently, the Polity2 level was increased to a democratic score of 8 in 1987. Even though it is clear that several aspects contributed to the democratization of the Philippines, the economic crisis is widely accepted as an important explanatory factor.

It is now time to see how robust the main results are. Thus, the rest of this chapter is devoted to a number of robustness tests.

7.3 Robustness tests

The results from the benchmark regressions are supportive of the working hypothesis. Economic growth does seem to have a negative effect on democratization. However, to increase confidence in the picture drawn in the main analysis it must be vindicated that it is not driven by a particular model specification or features of the data. Thus, in this section I carry out a series of robustness tests. First, I look at other measurements

[11] Here, democratic transition is defined as the first year of a competitively elected government (Haggard and Kaufman, 1995, 32).

[12] Snap election is an election called earlier than expected.

of democracy. Second, I use growth in rainfall as an alternative 2SLS instrument on a sub-Saharan sub-sample. Third, I test whether influential observations are driving the results. Forth, I account for censoring in the data by estimating a Tobit model. Fifth, I exclude the control variables with the highest percentage missing values. Finally, the time-series is prolonged to range from 1950 to 2011. All in all, the results from the main analysis hold up well.

Table B.2 in appendix B summarizes descriptive statistics for new variables added in the robustness models. In this section I only interpret the economic growth coefficient. Furthermore, misspecification tests are included in the tables but not commented upon unless they deviate from expected values.

7.3.1 Alternative democratization measurements

In this section I test the robustness of the benchmark results against different measurements of democracy. Two alternative measures are used in Table 7.4 as dependent variables: i) the first differenced Scalar Index of Policies (from now on SIP2), and ii) the first differenced civil liberties and political rights index (from now on FHI).

SIP2 ranges from 0, denoting least democratic, to 1, denoting most democratic.[13] The index is derived from codings along the same dimensions as Polity2. Those are the nature of the recruitment of executives, the extent to which the executive is constrained by other institutions, and popular participation (Gates, Hegre, Jones and Strand, 2006, 897-898). Data on the two first dimensions are taken from the Polity IV dataset. Data on the third dimension, political participation, are taken from Vanhanen's (2000) polyarchy index. It has been argued that codings on participation in the Polity IV dataset are too subjective.[14] Vanhanen's participation measure is the percentage of population that voted in the latest election (Gates et al., 2006, 897).[15]

A weakness with both Polity2 and SIP2 is that neither of them take civil liberties and political rights directly into account. In section 1.3, I defined these factors as decisive for democracy. In contrast, the measurement from Freedom House includes codings on these factors. I have combined the civil liberties and political rights indexes (labeled FHI).[16] Civil liberties are defined as the liberties that allow for freedoms of

[13] I take the variable from Strand, Hegre, Gates and Dahl (2012).

[14] For further details see Gates et al. (2006, 897).

[15] To make the measure account for whether the election is decisive for selection of the executive, competition must also be taken into account. This is done by multiplying participation with $\frac{competition}{30\%}$ *if* the election winner obtained more that 70 percent of the election votes. Competition is here the percentage of the valid vote won by all parties except the plurality winner or winning coalition. For more information see Gates et al. (2006)

[16] The variables are taken from the Quality of Government dataset (Teorell, Samanni, Holmberg

expression and belief, associational and organizational rights, rule of law, and personal interference from the state (Teorell, Samanni, Holmberg and Rothstein, 2011*a*, 42-43). Political rights enable people to participate freely in the political process, including the right to vote freely for distinct alternatives in legitimate elections, compete for public office, join political parties and organizations, and elect representatives who have decisive impact on public policies and are accountable to the electorate (Teorell et al., 2011*a*, 43). FHI is the unweighed average of the civil liberties and political rights indicators. The original index ranges from 1 to 7, where 7 denotes the least free society. However, I have inverted the index to render it comparable with Polity2 and SIP2. That is, here 1 denotes the least free society while 7 denotes the most free society.[17]

I repeat the benchmark findings in Table 7.4 for convenience. Results from three different regressions are reported for each of the dependent variables. The first set of estimates (models 1, 4 and 7) are from the second stage of a 2SLS regression that includes control variables and control for country fixed effects. The second set of estimates (models 2, 5 and 8) are the results from the corresponding Arellano-Bond GMM estimator. The third estimate set (models 3, 6 and 9) add controls for time fixed effects to the Arellano-Bond regressions.

The economic growth coefficient is negative in all models in Table 7.4. However, it is only statistically significant in some models where alternative dependent variables have been applied. Consider first the models where change in SIP2 is the dependent variable (models 4-6). Here, the negative growth coefficient is only statistically significant in model 4, the 2SLS regression. The size of this coefficient however, is similar in size to the corresponding coefficient in the benchmark model, when measured in terms of a standard deviation change.[18]

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and Rothstein, 2011*b*).

[17] As the time-series in my analysis is 1980-2007 and FHI captures all three criteria I defined as decisive for democracy, one might ask why I do not use FHI as my main measure of democracy. There are three reasons for this. First, FHI includes codings on indicators which I have not defined as decisive for democracy e.g. property rights and academic freedoms. This can weaken the validity of the estimates. Second, scores on FHI indicators are largely based on the subjective opinions of the coder. The subjectivity increases the risk of bias and unsystematic measurement error. Third, there is the issue of transparency. Freedom House do not release data on indicators or coding rules (Munck and Verkuilen, 2002). This prevents tests of reliability.

[18] In order to make the growth coefficient in model 4 comparable with the corresponding coefficient in model 1, the growth coefficient must be multiplied with its own standard deviation, and then divided by the standard deviation of the dependent variable. From Table B.2 we see that the standard deviation of SIP2 (in first differences) is 0.090. Furthermore, we remember from earlier interpretations that the standard deviation for economic growth is 7.411. Consequently, the effect of economic growth on democratization is estimated in model 4 to be $\frac{-0.008 \times 7.411}{0.090} = -0.659$. The corresponding effect when change in Polity2 is the dependent variable is $\frac{-0.198 \times 7.411}{1.722} = -0.852$. Thus, even though the

Table 7.4: 2SLS and AB-GMM 1980-2007, alternative dependent variables: first differenced SIP2 and FHI

VARIABLES	(1) 2SLS Polity2	(2) AB Polity2	(3) AB Polity2	(4) 2SLS Sip2	(5) AB Sip2	(6) AB Sip2	(7) 2SLS FHI	(8) AB FHI	(9) AB FHI
Economic growth (t-1)	-0.198***	-0.055**	-0.055**	-0.008***	-0.0019	-0.0025	-0.003	-0.0094*	-0.0071
	(-3.88)	(-2.25)	(-2.08)	(-2.70)	(-1.50)	(-1.59)	(-0.23)	(-1.66)	(-0.82)
Regime duration (t-1)	0.021***	0.018	0.073*	0.001*	0.00068	0.0021	0.002	-0.0099	-0.022
	(3.04)	(0.58)	(1.84)	(1.94)	(0.46)	(0.69)	(1.22)	(-1.12)	(-1.45)
Corruption (t-1)	-0.180**	-0.16	-0.27	-0.004	0.0014	-0.0095	-0.022*	-0.048	-0.30**
	(-2.52)	(-1.11)	(-0.83)	(-1.14)	(0.13)	(-0.61)	(-1.66)	(-0.71)	(-2.21)
Oil rents of GDP (t-1)	-0.007	-0.015	0.0076	0.0001	-0.00076	0.000022	-0.002	0.0019	0.019*
	(-0.73)	(-0.59)	(0.24)	(0.22)	(-0.49)	(0.015)	(-0.61)	(0.26)	(1.65)
Civil conflict onset (t-1)	0.005	-0.64	-0.40	-0.007	-0.088**	-0.090*	0.064	-0.26	0.059
	(0.02)	(-0.93)	(-0.61)	(-0.82)	(-2.19)	(-1.82)	(0.58)	(-1.08)	(0.20)
Income inequality (t-1) (log)	0.087	-0.79	-0.49	0.009	0.0041	-0.0067	-0.063	-0.046	-0.16
	(0.27)	(-0.93)	(-0.56)	(0.57)	(0.083)	(-0.14)	(-1.01)	(-0.21)	(-0.50)
Primary education (t-1)	2.849	26.7*	8.51	0.124	1.48	0.97	0.046	1.38	-2.09
	(0.98)	(1.67)	(0.52)	(0.98)	(1.54)	(0.96)	(0.08)	(0.34)	(-0.46)
2SLS 1st stage t-value IV 1	-1.52	–	–	-1.67	–	–	-1.65	–	–
2SLS 1st stage t-value IV 2	5.58	–	–	5.43	–	–	5.82	–	–
Kleibergen-Paap test	16.505	–	–	15.920	–	–	17.909	–	–
Sargan test	–	0.248	0.346	–	0.855	0.951	–	0.073	0.447
Hansen test	0.289	0.649	0.513	0.893	0.264	0.845	0.119	0.147	0.744
AR(1) test	–	0.000	0.000	–	0.002	0.003	–	0.000	0.000
AR(2) test	–	0.830	0.507	–	0.128	0.218	–	0.697	0.799
Observations	2019	1805	1805	1966	1741	1741	2030	1801	1801
Number of countries	113	113	113	111	111	111	113	112	112
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	No	Yes	No	No	Yes	No	No	Yes

Notes: Robust t-statistics clustered on countries in parentheses. Significance levels are denoted by *** p<0.01, ** p<0.05, * p<0.1. IV 1 denotes climatic natural disaster instrument. IV2 denotes global economic growth instrument. Kleibergen-Paap Waald F-statistic test for weak identification. H_0: Equation is weakly identified. Sargan and Hansen test for overidentification. H_0: Too many instruments are not included. AR(1) tests for first order serial correlation. AR(2) tests for second order serial correlation. H_0 in both AR tests is that there is not serial correlation.

The 2SLS coefficient is furthermore significant at a one percent level, which corresponds to the significant level when change in Polity2 is the dependent variable (model 1). One should also note that the economic growth coefficients in models 5 and 6 are close to being significant at a ten percent level (13 percent and 11 percent respectively). This increases my confidence in the view that there might be a negative short term effect of economic growth on democratization.

When change in FHI is the dependent variable (models 7-9), the negative economic growth coefficient is only statistically significant in the Arellano-Bond GMM regression that does not control for time (model 8). This growth coefficient is furthermore less negative than the corresponding estimate in the benchmark analysis (model 2).[19] The growth estimate is moreover only significant at a ten percent level, while the corresponding significance level when Polity2 is the dependent variable is 5 percent. Thus, the general impression from Table 7.4 is that economic growth has less of an effect on civil liberties and political rights than it has on the institutional features of democracy.

All in all, the benchmark results are not robust against using changes in SIP2 and FHI as dependent variables. However, the economic growth coefficient has a negative sign in all models. It is furthermore statistically significant in some models and close to significant in others. Thus, the general impression still remains; if anything, there is a negative short term effect of economic growth on democratization.

Several studies in the literature use a dichotomous measure of democracy. Even though I have argued that a continuous definition and conceptualization of democracy is preferable, I use the democracy variable from Przeworski et al. (2000) as a further robustness test of the results. The variable takes the value 1 for democratic countries and 0 otherwise.[20] A regime is considered to be democratic if the executive is directly or indirectly elected by popular vote, multiple parties are allowed, there is de facto existence of multiple parties outside regime front, there are multiple parties within the legislature, and there has been non consultation of incumbent advantage. Observations with regime transition within the year are coded according to the regime at the end of the year (Teorell et al., 2011*a*, 32). In Table 7.5, logistic regressions are estimated. Consequently, as simultaneity problems in the data are not addressed, the regression

estimate is less negative when change in SIP2 is the dependent variable, the coefficients are similar.

[19] From Table B.2 we see that the standard deviation for change in FHI is 0.456. Thus, the effect of economic growth on democratization when FHI is the dependent variable (model 8) is predicted to be $\frac{-0.009 \times 7.411}{0.456} = -0.153$. The corresponding effect when change in Polity2 is the dependent variable (model 2) is $\frac{-0.055 \times 7.411}{1.722} = -0.237$.

[20] The variable has been updated by Cheibub, Gandhi and Vreeland (2010) (see also Alvarez et al. (1996) and Alvarez, Cheibub, Limongi and Przeworski (1999)). I take the variable from the Quality of Government dataset (Teorell et al., 2011*b*).

estimates are not reliable by themselves.[21]

Table 7.5: ML Logit 1980-2007, change in ACLP as dependent variable

	(1)	(2)	(3)	(4)
VARIABLES	ML Logit	ML Logit	ML Logit FE	ML Logit FE
Economic growth (t-1)	-0.050***	-0.061**	-0.11***	-0.16***
	(-3.87)	(-2.28)	(-3.01)	(-2.99)
Regime duration (t-1)		-0.036*	0.037*	0.028
		(-1.87)	(1.84)	(1.21)
Corruption (t-1)		-0.32**	-0.47	-0.69*
		(-2.19)	(-1.62)	(-1.67)
Oil rents of GDP (t-1)		-0.019	-0.032	-0.088
		(-0.71)	(-0.60)	(-1.19)
Civil conflict onset (t-1)		0.49	0.76	0.017
		(0.59)	(0.82)	(0.014)
Income inequality (t-1) (log)		0.18	-0.098	1.28*
		(0.45)	(-0.19)	(1.69)
Primary education (t-1)		-0.60	-2.01	-18.6
		(-0.19)	(-0.16)	(-1.12)
Observations	4269	2031	490	490
Number of countries	165	114	25	25
Country FE	No	No	Yes	Yes
Year FE	No	No	No	Yes

Notes: Robust t-statistics clustered on countries in parentheses. Significance levels are denoted by *** p<0.01, ** p<0.05, * p<0.1.

The economic growth coefficient has a negative sign in all models. The coefficient is furthermore statistically significant at a one percent level in models 1, 3 and 4, and at a five percent level in model 2. The results in Table 7.5 increase my confidence in the benchmark conclusion for two reasons. First, the estimates illustrate that the negative and statistically significant effect of income growth on democratization holds when a dichotomous measure of democracy is applied. Second, the results in Table 7.5 clarify that the benchmark results are not driven by the use of a linear model.

7.3.2 Alternative instrument: growth in rainfall

When using instruments, one can never fully rule out that these are not endogenous to the dependent variable (democratization). This fact motivates the use of an alternative instrument as a robustness check. Accordingly, I follow Miguel, Satyanath

[21] 2SLS and Arellano-Bond GMM commands for dichotomous variables exist. However, they are not as well developed as the linear commands, and are not available in current versions of Stata.

and Serengeti (2004) and use the annual growth rate in rainfall as an instrument for economic growth in a sub-Saharan African sub-sample.[22] Miguel, Satyanath and Serengeti (2004) argue that weather shocks are plausible instruments for economic growth in economies that largely rely on rain-fed agriculture. Weather shocks will directly affect the economic growth in these countries, and therefore pick up variation in their economic growth. At the same time it is very unlikely that growth in rainfall will have a direct effect on democratization in a given country. Growth in rainfall is not a good instrument for the whole sample because weather shocks are unlikely to affect economic growth in more industrialized economies.

The rainfall instrument is used in combination with the global economic growth instrument. In Table 7.6, models 1 and 3 repeat the second stage 2SLS results from models where economic growth is instrumented through global economic growth and climatic natural disasters. In models 2 and 4 on the other hand, growth in rainfall and global economic growth is used as instruments on a sub-Saharan sub-sample. Control variables are only included in models 3 and 4. Country fixed effects however, are controlled for in all models.

From Table 7.6 we see that the economic growth coefficient is negative and statistically significant in all models. The growth estimate moreover becomes more negative in models that include the alternative instrument (models 2 and 4). It is impossible to say however, whether the increased coefficient size (in absolute value) stems from a stronger growth effect on democratization in sub-Saharan Africa, or whether it is a by-product of the instrument. While the economic growth coefficient becomes more significant in model 2 compared to model 1, the growth estimate is more significant in model 3 than it is in model 4. However, model 4 only includes 204 observations. This makes it difficult to find significant effects.[23]

Accordingly, the benchmark results hold up well against including growth in rainfall as instrument. This robustness test also suggests that the negative effect of income growth on democratization holds for sub-Saharan African countries.

[22]Rainfall growth data are taken from Miguel, Satyanath and Serengeti (2004). I use the rainfall variable that originally stems from U.N. Food and Agricultural Organization Climatic (FAO). There are rainfall estimates for each point at which latitude and longitude degree lines cross, at 2.5 degree-intervals. For more information about rainfall data see Miguel, Satyanath and Serengeti (2004).

[23]The rainfall instrument obtains a low t-value in the first stage of the 2SLS regression. This is particularly true for the regression that does not include control variables (t-value being -0.77). A low t-value in combination with Kleibergen-Paap statistics below 10, implies that the rainfall instrument only is weakly correlated to economic growth, also in sub-Saharan African countries. However, the results from the Hansen test do not imply that neither of the included instruments should be excluded from the regression.

Table 7.6: 2SLS 1980-2007, alternative instrument: growth in rainfall

VARIABLES	(1) 2SLS(2nd) original	(2) 2SLS(2nd) robtest	(3) 2SLS(2nd) orginal	(4) 2SLS(2nd) robtest
Economic growth (t-1)	-0.030*	-0.25**	-0.20***	-0.23*
	(-1.74)	(-2.31)	(-3.88)	(-1.84)
Regime duration (t-1)			0.021***	0.031*
			(3.04)	(1.66)
Corruption (t-1)			-0.18**	-0.055
			(-2.52)	(-0.20)
Oil rents of GDP			-0.0071	-0.0057
			(-0.73)	(-0.19)
Civil conflict onset (t-1)			0.0046	-0.13
			(0.020)	(-0.28)
Income inequality (t-1) (log)			0.087	-0.38
			(0.27)	(-0.61)
Primary education (t-1)			2.85	20.0
			(0.98)	(1.04)
2SLS 1st stage t-value IV1	-3.76	–	-1.52	–
2SLS 1st stage t-value IV2	8.57	3.93	5.58	3.39
2SLS 1st stage t-value IV3	–	-0.77	–	-1.87
Kleibergen-Paap test	41.552	7.776	16.505	9.004
Hansen test	0.168	0.798	0.289	0.402
Observations	3930	711	2019	204
Number of countries	155	43	113	20
Country FE	Yes	Yes	Yes	Yes
Year FE	No	No	No	No

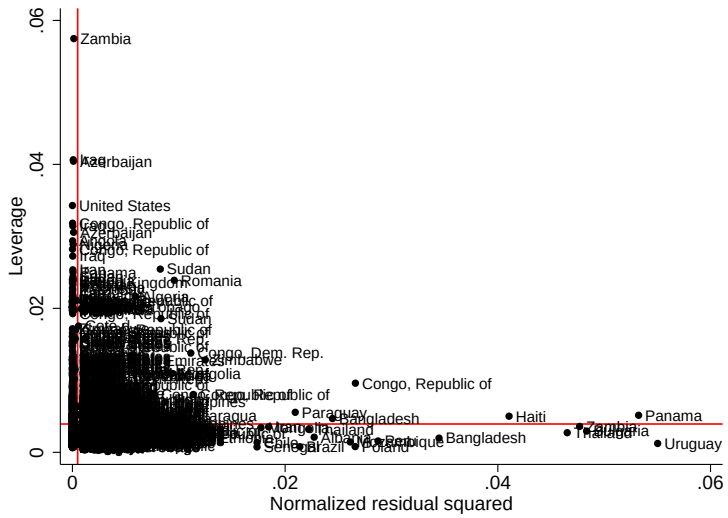
Notes: Dependent variable is first differenced Polity2. Robust t-statistics clustered on countries in parentheses. Significance levels are denoted by *** p<0.01, ** p<0.05, * p<0.1. IV 1 denotes climatic natural disaster instrument. IV2 denotes global economic growth instrument. IV3 denotes growth in rainfall instrument. Kleibergen-Paap Waald F-statistic test for weak identification. H_0: Equation is weakly identified. Hansen test for overidentification. H_0: Too many instruments are not included.

7.3.3 Influential observations

Another concern is the possibility that certain influential observations are driving the results in the main analysis. Observations with high leverage have abnormal values on predictor variables. This renders them as candidates of being especially influential

for results. An observation is defined as influential *if* excluding it from the analysis changes the key estimates substantially (Wooldridge, 2009, 325). Observations with large residuals are often labeled outliers. They are observations that the model predicts poorly, and can therefore affect the regression results. Figure 7.1 plots the squared residuals from the OLS model (model 2 in Table 7.1) against their leverage.[24]

Figure 7.1: Normalized residual and Leverage, 1980-2007



The observations that should raise concern in Figure 7.1 are those with both high leverage *and* high residuals (squared). The reason is that these are "abnormal" observations with high influence. However, the plot does not seem to reveal any observations where both abnormal leverage and residuals are observed *simultaneously*. Zambia stands out in the plot with one observation with high leverage. However, the red line indicates that the model predicts this case well (it has a low residual squared). Panama on the other hand, has a high residual. Nevertheless, as this observation has low leverage, it is not an influential observation and therefore not a cause of concern.

As a more thorough test for influence I compute two more statistics, cook's D and dfbeta. Cook's D combines information about an observation's leverage and residual. It summarizes how much each observation influences the fitted model. The higher the cook's D, the higher the influence (Hamilton, 2009, 210). Dfbeta follows the same strategy but is coefficient specific. Thus, it measures how much each observation affects the given predictor coefficient (Hamilton, 2009, 210). I follow the convention in earlier literature, and define observations with values of cook's D and dfbeta higher than one

[24]It is not possible to compute cook's D and dfbeta statistics based on the 2SLS and Arellano-Bond GMM estimators. Thus, the statistics reported here are based on the ordinary least squares results.

as potential influential observations (Christophersen, 2009, 163). Results are reported in Figure B.2 and Figure B.1 in appendix B. These figures reveal that neither of the observations have cook's D or dfbeta values higher than the threshold. This indicates that the message in the main analysis is not driven by influential observations.

Table 7.7: 2SLS and AB-GMM 1980-2007, excluding possible influential observations

	(1)	(2)	(3)
VARIABLES	2SLS(2nd) 5/95 growth	AB 5/95 growth	AB 5/95 growth
Economic growth (t-1)	-0.20***	-0.055**	-0.055**
	(-3.88)	(-2.25)	(-2.08)
Regime duration (t-1)	0.021***	0.018	0.073*
	(3.04)	(0.58)	(1.84)
Corruption (t-1)	-0.18**	-0.16	-0.27
	(-2.52)	(-1.11)	(-0.83)
Oil rents of GDP (t-1)	-0.0071	-0.015	0.0076
	(-0.73)	(-0.59)	(0.24)
Civil conflict onset (t-1)	0.0046	-0.64	-0.40
	(0.020)	(-0.93)	(-0.61)
Income inequality (t-1) (log)	0.087	-0.79	-0.49
	(0.27)	(-0.93)	(-0.56)
Primary education (t-1)	2.85	26.7*	8.51
	(0.98)	(1.67)	(0.52)
2SLS 1st stage t-value IV1	-1.52	–	–
2SLS 1st stage t-value IV2	5.58	–	–
Kleibergen-Paap test	16.505	–	–
Sargan test	–	0.248	0.346
Hansen test	0.289	0.649	0.513
AR(1) test	–	0.000	0.000
AR(2) test	–	0.830	0.507
Observations	2019	1805	1805
Number of countries	113	113	113
Country FE	Yes	Yes	Yes
Year FE	No	No	Yes

Notes: Dependent variable is first differenced Polity2. Robust t-statistics clustered on countries in parentheses. Significance levels are denoted by *** p<0.01, ** p<0.05, * p<0.1. IV 1 denotes climatic natural disaster instrument. IV2 denotes global economic growth instrument. Kleibergen-Paap Waald F-statistic test for weak identification. H_0: Equation is weakly identified. Sargan and Hansen test for overidentification. H_0: Too many instruments are not included. AR(1) tests for first order serial correlation. AR(2) tests for second order serial correlation. H_0 in both AR tests is that there is not serial correlation.

Since I am not able to compute cook's D and dfbeta statistics based on the benchmark models (those using instruments), I also carry out a more informal test of influential observations. In Table 7.7 I re-estimate the benchmark models without observations with "extreme" values on economic growth. In particular, all observations outside the five and ninety five percentiles of the (unconditional) economic growth distribution are excluded.[25] Model 1 shows the results from a 2SLS estimation in which both standard control variables and country specific effects controlled for. Model 2 reports the results from the corresponding Arellano-Bond GMM regression. Model 3 is the same as model 2, except that also time fixed effects are controlled for.

From Table 7.7 we see that observations with particularly high and low economic growth are not influential, as excluding them from the analysis does not alter the results. Both the economic growth coefficients and the corresponding significant levels remain unchanged compared to Table 7.2 and Table 7.3. Thus, the overall conclusion seems to be that influential observations do not drive the results found in the benchmark analysis.

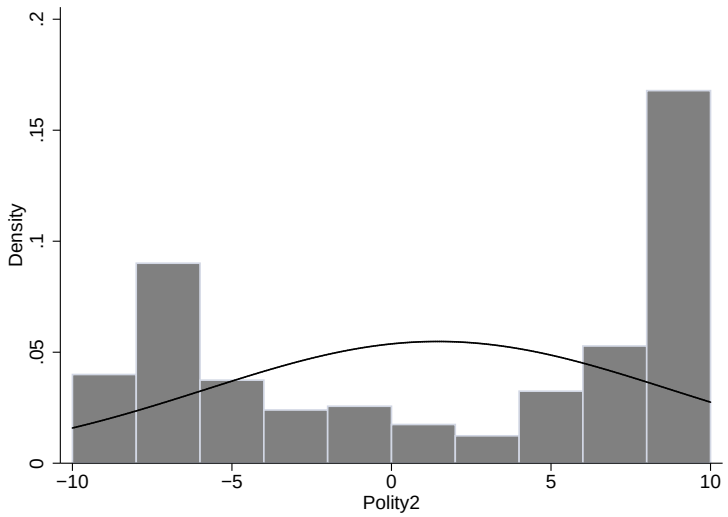
7.3.4 Taking censoring into account

Data are referred to as censored when their theoretical counterparts can take values outside the bounds given in operational definitions. Censoring is especially relevant if the researcher does not know how far the true value is located outside the bounds (Long, 1997, 187). For Polity2, censoring implies that many countries in the sample should be coded as -10 and 10. Suppose for instance that there exist a "true" distribution of Polity2 that goes all the way to 20. Then observations will stack up at 10, given that this is the upper bound. A democratic regime coded as 10 can for instance be more democratic than another regime that is given the same Polity2 score. As they are both coded as 10, it is impossible to say by how much.[26] Figure 7.2 illustrates that the Polity2 distribution piles up in both ends. There are particularly many countries at the democratic end, indicating right-censoring. This is hardly surprising as most OECD countries have been stable democracies (with Polity2 level of 10) for a long time.

[25] The five and ninety five percentiles in economic growth have -62.37 and 88.72 percentage points as cut-off points, respectively.

[26] For instance, in 2011 both Cape Verde and France had a Polity2 score of 10. In absolute terms however, it seems likely that France is more democratic than Cape Verde.

Figure 7.2: Polity2 distribution, 1980-2007



Censoring can be a problem because it violates the linear model assumption of normally distributed error terms. From Figure 7.2 we see that Polity2 clearly does not have an unconditional normal distribution. However, the assumption is that the conditional error term (i.e. the error term where explanatory variables have been controlled for) is normally distributed. Still, as Polity2 is not normally distributed it is unlikely that the conditional error term will be. In particular, as factors that are left out of the equation may explain why Polity2 is censored, they will prevent the conditional error term from being normally distributed.

Two factors make it unlikely that censoring is driving the results in the main analysis. First, as I control for country specific effects, countries that do not have any variation in Polity2 are left out of the regression. By looking at the data it is evident that most fully democratic countries (Polity2 equal to 10) have been stable since the beginning of the sample (1980). This is also the case for several of the most autocratic countries (e.g. Saudi Arabia and Qatar). Second, as I study change in Polity2 rather than Polity2 level, censoring of the data is likely to pose less problems. However, to make sure that my results are robust to censoring, I estimate a Tobit model. It is based on a Maximum Likelihood (ML) estimation and takes censoring into account (see e.g. Wooldridge (2009, 587-595), Long (1997) and Greene (2003) for textbook derivations of the Tobit model).

Both right and left-censoring is controlled for in Table 7.8. It is however, not possible to control for country-specific effects when using the Tobit estimator.[27] In

[27]Just as in the Logit model, the non-linearity of the Tobit model implies that inclusion of country dummies will not remove bias caused by omitted, time-independent variables. See Wooldridge (2009) for further details.

model 1 economic growth is the only included explanatory variable. Model 2 also includes the standard set of control variables. Model 3 furthermore controls for time.

Table 7.8: ML Tobit 1980-2007, controlling for censoring

	(1)	(2)	(3)
VARIABLES	ML Tobit	ML Tobit	ML Tobit
Economic growth (t-1)	-0.0034	-0.018***	-0.014**
	(-0.93)	(-2.58)	(-1.98)
Regime duration (t-1)		-0.0013*	-0.00032
		(-1.74)	(-0.42)
Corruption (t-1)		-0.075**	-0.13***
		(-2.43)	(-3.70)
Oil rents of GDP (t-1)		-0.0077***	-0.0072**
		(-2.64)	(-2.53)
Civil conflict onset (t-1)		-0.040	-0.15
		(-0.22)	(-0.83)
Income inequality (t-1) (log)		-0.073	-0.024
		(-0.80)	(-0.27)
Primary education (t-1)		-0.85	-1.97**
		(-1.15)	(-2.56)
Observations	4031	2033	2033
Left-censored observations	15	4	4
Right-censored observations	30	12	12
Country FE	No	No	No
Year FE	No	No	Yes

Notes: Dependent variable is first differenced Polity2. Robust t-statistics clustered on countries in parentheses. Significance levels are denoted by *** p<0.01, ** p<0.05, * p<0.1.

A proper interpretation of the Tobit coefficients involves some technical details which I do not cover here (see e.g. Long (1997, 206-210) and Wooldridge (2009, 589-594)). The reader should note that these coefficients are not directly comparable with those in the main analysis. Still, one can always infer the sign of estimated effects. From Table 7.8 we see that the economic growth coefficient has a negative sign in all models. The estimate is not statistically significant in model 1, which does not include additional controls. This, however, is also the case in several of the benchmark regressions. Hence, the results from the main analysis do not seem to be driven by censoring in Polity2.[28]

[28]As right-censoring looks most alarming in Figure 7.2, I also estimated the same Tobit models

7.3.5 Excluding controls with high percentage missing

A potential problem with the benchmark regression results is that inclusion of control variables substantially reduces the number of observations. To check how this observation affects the results, I now exclude the control variables with the highest amount of missing.

Table 7.9: 2SLS and AB-GMM 1980-2007, excluding regressors with high missing

	(1)	(2)	(3)	(4)	(5)	(6)
	2SLS(2nd)	2SLS(2nd)	2SLS(2nd)	AB	AB	AB
VARIABLES	all variables	excluding 2	excluding 3	all variables	excluding 2	excluding 3
Economic growth (t-1)	-0.20***	-0.050**	-0.036*	-0.055**	-0.050*	-0.030
	(-3.88)	(-2.08)	(-1.79)	(-2.08)	(-1.77)	(-0.93)
Regime duration(t-1)	0.021***	0.0053	0.0063**	0.073*	-0.044	-0.065
	(3.04)	(1.37)	(2.14)	(1.84)	(-0.55)	(-0.82)
Civil conflict onset (t-1)	0.0046	0.11	0.32	-0.40	0.45	1.08
	(0.020)	(0.53)	(1.32)	(-0.61)	(0.40)	(0.75)
Income inequality (t-1) (log)	0.087	-0.083	-0.011	-0.49	1.67	-0.99
	(0.27)	(-0.65)	(-0.091)	(-0.56)	(1.43)	(-0.60)
Primary education (t-1)	2.85	-0.26		8.51	-3.74	
	(0.98)	(-0.15)		(0.52)	(-0.19)	
Corruption (t-1)	-0.18**			-0.27		
	(-2.52)			(-0.83)		
Oil rents of GDP (t-1)	-0.0071			0.0076		
	(-0.73)			(0.24)		
2SLS 1st stage t-value IV1	-1.52	-3.30	-3.37	–	–	–
2SLS 1st stage t-value IV2	5.58	10.05	8.49	–	–	–
Kleibergen-Paap test	16.505	52.965	43.556	–	–	–
Sargan test	–	–	–	0.346	0.808	0.999
Hansen test	0.289	0.174	0.163	0.513	0.731	0.978
AR(1) test	–	–	–	0.000	0.000	0.000
AR(2) test	–	–	–	0.507	0.472	0.941
Observations	2019	3343	3893	1805	3157	3957
Number of countries	113	152	155	113	153	159
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	No	No	Yes	Yes	Yes

Notes: Dependent variable is first differenced Polity2. Robust t-statistics clustered on countries in parentheses. Significance levels are denoted by *** p<0.01, ** p<0.05, * p<0.1. IV 1 denotes climatic natural disaster instrument. IV2 denotes global economic growth instrument. Kleibergen-Paap Waald F-statistic test for weak identification. H_0: Equation is weakly identified. Sargan and Hansen test for overidentification. H_0: Too many instruments are not included. AR(1) tests for first order serial correlation. AR(2) tests for second order serial correlation. H_0 in both AR tests is that there is not serial correlation.

From Table B.1 we see that three variables have particularly high fraction of missing observations. Those are corruption (31.32 percent missing), oil rents share of GDP

while only controlling for right-censoring. This did not alter the results (not shown).

(24.74 percent), and primary education (16.43 percent).

In Table 7.9 models 1 through 3 report the second stage results from 2SLS regressions with control for country specific effects. In model 1 all control variables are included. In model 2, corruption and oil rents of GDP are excluded. Finally, in model 3 primary education is also excluded. Models 4 through 6 report the results from corresponding Arellano-Bond GMM regressions which control for both country and time fixed effects.

Consider first the 2SLS results. The parameter of interest has a negative sign in all models. However, once controls are thrown out, the coefficient drops in absolute value and becomes substantially less significant. The significance level drops from one percent in model 1 to ten percent in model 3. Note that the 2SLS estimate is in line with those found using Arellano-Bond once the corruption and oil variables are excluded.

The Arellano-Bond estimates reveal a stable point estimate, although it drops to about -0.03 in model 6. However, the significant level is reduced from a five percent level in model 4 to clearly insignificant in model 6.[29] The insight from this exercise seems to be that i) the negative point estimate found in the benchmark regressions is relatively robust to exclusion of controls with high percentage missing, ii) the 2SLS results become much more consistent with the Arellano-Bond results once corruption and oil (the two regressors with the highest percentage missing) are thrown out, and iii) inclusion of controls still increases the significance of the parameter of interest.

7.3.6 Extended time-series: 1950-2011

Data availability and concerns about quality of the data (and percentage of missing observations) have motivated the use of data from 1980-2007 in the main analysis. In this section however, the time-series is prolonged to range from 1950 to 2011.[30] As control variables have different time-series they are not included in this exercise. Furthermore, as the climatic natural disaster instrument starts in 1980, economic growth is only instrumented through global economic growth in the 2SLS estimator.[31]

Models 1 and 2 report the result from the second stage 2SLS estimator which controls for country specific effects for 1980-2007 and 1950-2011 respectively. Models

[29]The negative growth coefficient also becomes insignificant when all three variables are excluded in Arellano Bond regressions that do not control for time (results not shown).

[30]Extending the time-series has serious implications for percentage of missing observations. In particular, from 1980-2007 the economic growth variable has 1.82 percent missing observations. When the time-series is extended to range from 1950 to 2011, the corresponding percentage is 17.93.

[31]This is the reason why the economic growth estimate in model 1 differs some from the regression coefficient in model 3 in Table 7.2.

3 and 4 show the result from the corresponding Arellano-Bond GMM regressions. In models 5 and 6 time fixed effects are furthermore controlled for.

Table 7.10: 2SLS and AB-GMM 1980-2007 and 1950-2011

	(1)	(2)	(3)	(4)	(5)	(6)
	2SLS(2nd)	2SLS(2nd)	AB	AB	AB	AB
VARIABLES	1980-2007	1950-2011	1980-2007	1950-2011	1980-2007	1950-2011
Economic growth (t-1)	-0.038**	-0.065***	-0.026*	-0.030**	-0.044	-0.039
	(-2.16)	(-4.20)	(-1.76)	(-2.01)	(-1.30)	(-1.55)
2SLS 1st stage t-value IV2	8.65	11.68	–	–	–	–
Kleibergen-Paap test	74.815	136.336	–	–	–	–
Sargan test	–	–	0.749	0.618	0.943	0.902
Hansen test	–	–	0.420	0.540	0.778	0.735
AR(1) test	–	–	0.000	0.000	0.000	0.000
AR(2) test	–	–	0.780	0.430	0.048	0.080
Observations	4031	6636	4000	6470	4000	6470
Number of countries	159	159	159	159	159	159
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	No	No	No	Yes	Yes

Notes: Dependent variable is first differenced Polity2. Robust t-statistics clustered on countries in parentheses. Significance levels are denoted by *** p<0.01, ** p<0.05, * p<0.1. IV2 denotes global economic growth instrument. Kleibergen-Paap Waald F-statistic test for weak identification. H_0: Equation is weakly identified. Sargan and Hansen test for overidentification. H_0: Too many instruments are not included. AR(1) tests for first order serial correlation. AR(2) tests for second order serial correlation. H_0 in both AR tests is that there is not serial correlation.

For models 1-4 we see that extending the time-series yields similar, although somewhat more negative estimates of the economic growth effect on democratization. Furthermore, when the time-series is prolonged to range from 1950-2011, the growth coefficient in the 2SLS estimator is significant at a one percent level rather than a five percent level. Similarly, the Arellano Bond GMM estimate is significant at a five percentage level rather than a ten percentage level. The increase in significance is not surprising as the models with longer time-series have more observations. The economic growth coefficient is not statistical significant in neither time-series when time fixed effects are controlled for.

In sum, the negative effect of income growth on democratization does not seem to be driven by choice of time-series. Rather, when prolonging the time-series to range from 1950 to 2011, the effect of economic growth on democratization appears to be even more negative.

The overall conclusion from this section is that the results from the benchmark analysis are relatively robust. In particular, they do not seem to be driven by the democratization measurement, external instruments, influential observations, censoring of the data, nor the time-series. This section has also illustrated that the benchmark results are not driven by the use of a linear model. In the next chapter I investigate whether the negative effect of income growth on democratization matters more for some countries than others.

Chapter 8

What kind of countries explain the results?

The analysis in the previous chapter established that the estimated effect of income on democracy turns negative when i) key variables are first differenced, and ii) income is instrumented (either by other variables, or by its own lags). In this chapter I take a step further and investigate what might cause these results. In particular I ask three questions: First, does the effect of income growth on democratization depend on regime type? For instance, are the results limited to a subset of autocratic, rich states such as the OPEC countries? Second, does the direction of democratic change matter for the effect of economic growth? For instance, is the negative effect limited to countries on a path towards autocracy? Finally, using an error correction model, I ask whether the effect of income on democracy changes with time. In particular I ask whether there might be a long term effect that differs from the negative short run effect. In this chapter I only comment upon the economic growth coefficient, unless other estimates deviate substantially from expected values.

8.1 A regime dependent effect

Some contributions in the literature argue that economic growth has a stabilizing impact on the regime type that is already in place.[1] In particular, it is claimed that economic growth increases the ruler's popularity, regardless of regime type, and thus his odds of survival. This argument has for instance been used to explain why China, who has seen a substantial economic growth the last decades, remains autocratic (Bueno de

[1]See e.g. Kennedy (2010), Epstein et al. (2006), Przeworski and Limongi (1997), Londregan and Poole (1990) Bueno de Mesquita and Downs (2005).

Mesquita and Downs, 2005). Even though there clearly is a difference between regime stability and democratization, it is interesting to examine whether the effect of income growth on democratization differs between regime types. Following the argument above, one might expect that the effect of income on democracy is positive in democracies and negative in autocracies. Furthermore, Fayad, Bates and Hoeffler (2012) find a negative effect of income on democracy in inconsistent regimes, i.e. those that are neither democratic nor autocratic. Consequently, in this section I test the effect of income growth on democratization in three sub-samples: democracies, inconsistent regimes, and autocracies.

To determine whether there is a regime dependent effect of growth, models 1-3 in Table 8.1 show the (second stage) 2SLS results for democratic ($Polity2 \geq 8$), inconsistent ($-8 < Polity2 < 8$), and autocratic regimes ($Polity2 \leq -8$) respectively. Models 4-6 report the results from corresponding Arellano-Bond GMM regressions. Time dummy variables could not be included in the Arellano-Bond regressions because the number of observations is severely reduced. However, both the standard set of control variables and country fixed effects are controlled for in all models.

The results in Table 8.1 are *not* in line with the story told above (i.e. that economic growth reinforces the ruling elite, and thereby has a positive effect in democracies and negative effect in autocracies). First of all, only the coefficients in models 1 and 2 are statistically significant.[2] Furthermore, if anything, the growth coefficient in model 1 should be positive to be supportive of the described regime dependent effect. As neither of the coefficients in the autocratic models are close to being significant, the results in Table 8.1 could on the contrary imply that there is a regime dependent effect, but that it works the other way around. Economic growth seems to have a negative effect on democratization in democracies and inconsistent regimes, but not a significant effect in autocratic countries. However, the negative effect is never significant in the Arellano-Bond regressions.

The results in Table 8.1 should be interpreted with caution as the number of observations is now severely reduced. This is especially the case for the models that only include autocratic countries (observations being 165 and 145). The fewer the observations, the harder it is to find significant effects. That being said, the results in Table 8.1 are robust against different thresholds for the regime types. I used $\{< -6, < -7, < -8, < -9\}$ as additional thresholds for autocracy, and $\{> 6, > 7, > 8, > 9\}$ as thresholds for democracy. In all cases inconsistent regimes were defined as the regimes in between these two regime types. The results remained similar (not shown).

[2]The estimates that are significant however, are very similar to the comparable estimate in model 4 in Table 7.2 (coefficients being -0.23 and -0.26 compared to -0.198).

Table 8.1: 2SLS and AB-GMM 1980-2007, a regime dependent effect

VARIABLES	(1) 2SLS(dem)	(2) 2SLS(bet)	(3) 2SLS(aut)	(4) AB(dem)	(5) AB(bet)	(6) AB(aut)
Economic growth (t-1)	-0.23*	-0.26**	-0.0040	-0.065	-0.0055	0.0017
	(-1.82)	(-2.41)	(-0.46)	(-1.52)	(-0.19)	(1.11)
Regime duration (t-1)	0.024	0.062***	0.0045	-0.013	0.040	-0.00096
	(1.04)	(3.71)	(1.40)	(-1.12)	(1.15)	(-0.30)
Corruption (t-1)	-0.033	-0.41**	0.019	-0.46*	0.18	0.014
	(-0.43)	(-2.10)	(0.79)	(-1.66)	(0.60)	(0.76)
Oil rents of GDP (t-1)	-0.032	-0.023	0.0010	-0.026	-0.018	0.0034
	(-1.01)	(-0.88)	(0.65)	(-1.03)	(-0.51)	(1.48)
Civil conflict onset (t-1)	-0.078	-0.044	0.060	-1.08	1.09	-0.043
	(-0.36)	(-0.099)	(0.53)	(-1.32)	(1.11)	(-0.63)
Income inequality (t-1) (log)	0.41	0.035	-0.022	-0.72	-0.57	-0.0045
	(0.34)	(0.081)	(-0.91)	(-0.69)	(-0.53)	(-0.15)
Primary education (t-1)	8.53	5.38	0.23	29.4*	7.96	-2.97
	(1.13)	(0.96)	(0.34)	(1.81)	(0.27)	(-1.39)
2SLS 1st stage t-value IV1	-1.21	-0.81	-10.81	–	–	–
2SLS 1st stage t-value IV2	4.81	3.05	0.67	–	–	–
Kleibergen-Paap F-statistic	12.031	5.647	61.703	–	–	–
Sargan test	–	–	–	0.243	0.803	0.989
Hansen test	0.343	0.463	0.316	0.993	0.971	1.000
AR(1) test	–	–	–	0.013	0.000	0.127
AR(2) test	–	–	–	0.678	0.630	0.430
Observations	1001	851	165	914	746	145
Number of countries	65	72	21	66	73	21
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	No	No	No	No	No

Notes: Dependent variable first differenced Polity2. Robust t-statistics clustered on countries in parentheses. Significance levels are denoted by *** p<0.01, ** p<0.05, * p<0.1. IV 1 denotes climatic natural disaster instrument. IV2 denotes global economic growth instrument. Kleibergen-Paap Waald F-statistic test for weak identification. H_0: Equation is weakly identified. Sargan and Hansen test for overidentification. H_0: Too many instruments are not included. AR(1) tests for first order serial correlation. AR(2) tests for second order serial correlation. H_0 in both AR tests is that there is not serial correlation.

From models 1-3 we see that the 2SLS instruments differ in strength across the three regime types. While climatic natural disasters is a weak instrument in democratic and inconsistent regimes (which also has been the tendency in former regressions) this instrument is strongly negatively correlated to economic growth in autocratic countries (t-value being -10.81). Furthermore, while the global economic growth instrument has

a t-value higher than 3 in both democratic and inconsistent regimes, this instrument is only weakly correlated with economic growth in autocracies. However, the Hansen tests imply that neither of the instruments should be excluded. The AR(1) test in model 6 is furthermore higher than 0.05. This implies that one *cannot* reject the null of no first order serial correlation in first differences, and points to potential model misspecification. However, the AR(2) test does not suggest second order serial correlation either. Evaluated separately, this implies that no misspecification is detected.

8.2 The direction of democratization

As change in Polity2 is the dependent variable in this thesis, it is informative to analyze whether the *direction* of that change matters. All linear models studied so far have implicitly used the assumption that an increase in economic growth has the same quantitative effect as a decrease, i.e. that increases and decreases in the growth rate differ only in terms of the sign of the effect. For instance, the point estimate in model 4 in Table 7.3 implies that an increase in the growth rate by one standard deviation leads to $0.055 \times 7.4 = 0.407$ lower democratization. An equivalent statement derived from this model is that a *decrease* in the growth rate by one standard deviation *increases* democratization by 0.407. Here I ask whether such symmetry in the effects is evident in the data. Alternatively, one could think that the estimated negative effect depends on the regime a country is currently moving towards (e.g. that the effect is only negative when countries have a negative change in democratization). I perform a two-step exercise to analyze this question. First I check whether countries on a democratic path experience a slow-down caused by higher income growth. This is done by estimating the benchmark models on a sub-sample consisting only of those observations with positive values of Polity2 (as measured in first differences). Second, I test whether countries heading for autocracy get this process accelerated when income growth rises. Then I look only at the sub-sample with negative democratization values.

Models 1 and 2 in Table 8.2 provide the second stage results from 2SLS regressions using sub-samples "democratic path" and "autocratic path", respectively. Models 3 and 4 report the results from the corresponding Arellano-Bond GMM regressions. Control variables and country fixed effects are included in all models. Time is controlled for in the Arellano-Bond GMM models.

From Table 8.2 we see that economic growth has a negative coefficient in all models. The coefficient however, is only statistically significant in models 1 and 3.[3] Accord-

[3]The size of these coefficients are similar to those estimated in the main analysis (-0.161 compared

ingly, it seems that the negative effect of economic growth on democratization is more relevant for countries on their way to democracy than those heading for autocracy.

Table 8.2: 2SLS and AB-GMM 1980-2007, direction of democratic change

	(1)	(2)	(3)	(4)
VARIABLES	2SLS(dem)	2SLS(aut)	AB(dem)	AB(aut)
Economic growth (t-1)	-0.161***	-0.037	-0.039*	-0.024
	(-4.37)	(-1.12)	(-1.85)	(-0.63)
Regime duration (t-1)	0.021***	8.05e-08	0.0055	0.32***
	(3.31)	(0.00)	(0.11)	(3.49)
Corruption (t-1)	-0.138**	-0.033	0.15	-1.21***
	(-2.50)	(-0.90)	(0.41)	(-2.67)
Oil rents of GDP (t-1)	-0.005	-0.002	0.026	0.0098
	(-0.53)	(-0.87)	(0.81)	(0.30)
Civil conflict onset (t-1)	0.025	0.014	0.21	-1.13
	(0.14)	(0.10)	(0.20)	(-1.59)
Income inequality (t-1) (log)	-0.016	0.088	0.70	-2.25*
	(-0.06)	(1.09)	(0.73)	(-1.92)
Primary education (t-1)	2.760	0.271424	25.0	-12.7
	(1.18)	(0.20)	(1.04)	(-0.61)
2SLS 1st stage t-value IV1	-1.28	-1.75	–	–
2SLS 1st stage t-value IV2	5.59	5.60	–	–
Kleibergen-Paap test	16.070***	16.811***	–	–
Sargan test	–	–	0.224	0.001
Hansen test	0.163	0.297	0.406	0.518
AR(1) test	–	–	0.000	0.009
AR(2) test	–	–	0.761	0.062
Observations	1981	1878	1769	1681
Number of countries	113	113	113	113
Country FE	Yes	Yes	Yes	Yes
Year FE	No	No	Yes	Yes

Notes: Dependent variable first differenced Polity2. Robust t-statistics clustered on countries in parentheses. Significance levels are denoted by *** p<0.01, ** p<0.05, * p<0.1. IV 1 denotes climatic natural disaster instrument. IV2 denotes global economic growth instrument. Kleibergen-Paap Waald F-statistic test for weak identification. H_0: Equation is weakly identified. Sargan and Hansen test for overidentification. H_0: Too many instruments are not included. AR(1) tests for first order serial correlation. AR(2) tests for second order serial correlation. H_0 in both AR tests is that there is not serial correlation.

In other words, the results in the main analysis do not seem to be driven by some

to −0.198, and −0.037 compared to −0.055).

autocratic countries that have seen high growth followed by more autocracy. So why is the negative effect stronger in countries heading for democracy? It is important to keep in mind that the time series used here spans a period often referred to as the third wave of democratization (Huntington, 1991). In this period many countries, including former Soviet nations (at least formally), became democracies. However, several of these countries have subsequently returned to authoritarianism, Russia being one example. Russia is also one of the countries in this time period that experienced relatively high economic growth. These features might have an impact on the results. Also, longer lasting democracies like Venezuela and Colombia experienced movements away from democracy within the same period (Teorell, 2010, 2). At least the results in Table 8.2 do not support the notion that the effect of income on democratization changes sign when comparing democratic and autocratic paths.

8.3 Long term effects

The focus in this thesis is on the short term effects of income growth on democratization, and the main analysis suggests that this effect is negative and significant. In this section I investigate the long term effect. It is possible that economic development has a negative short term effect on democratization, while at the same time slowly alter the society in such a way that democratization becomes more likely over time. The changes to society that Lipset (1959) and others discuss are after all not swift changes. Rather they are time consuming, persistent processes. Take for instance education. It will take many years to increase the general education level in a society. Accordingly, Bueno de Mesquita and Downs (2005, 79) argue that economic development can threaten the survival of repressive governments in the long run because it raises the probability that effective political competitors will emerge. There are at least three reasons for this. First, in accordance with the theoretical arguments presented in this thesis, income development raises the stakes of the political game by increasing the economic returns available to the winner. Second, in a longer perspective, economic development also creates more individuals with sufficient human and monetary resources to get involved in politics (ibid). Third, following development, it is also more likely that the population will stand up and demand democratic rights. In this section I construct a regression model that makes it possible to determine long term effects.

8.3.1 An error correction model

Motivated by the results from the unit root tests (section 6.2), I here establish an error correction model (ECM) to estimate long term effects. Error correction models are widely used when both dependent and explanatory variables contain unit roots, which is what we have found here. See e.g. Verbeek (2008, 332) for further details. To derive the error correction model, two terms are added to (4.1). First, ρy_{it-1}, which gives dynamics in y. Second, $\alpha_1 x_{it}$, which makes it possible to determine long-term effects.[4] By adding these two terms to (4.1), I now have the following equation (again I abstract from control variables to ease the interpretation):

$$y_{it} = \beta_0 + \rho y_{it-1} + \alpha_1 x_{it} + \beta_1 x_{it-1} + u_{it} \tag{8.1}$$

Then y_{it-1} is subtracted from both sides to get

$$\begin{aligned} y_{it} - y_{it-1} &= \beta_0 + \rho y_{it-1} + \alpha_1 x_{it} + \beta_1 x_{it-1} - y_{it-1} + u_{it} \\ \Delta y_{it} &= \beta_0 + (\rho - 1) y_{it-1} + \alpha_1 x_{it} + \beta_1 x_{it-1} + u_{it}. \end{aligned}$$

Remember, $\Delta y_{it} = y_{it} - y_{it-1}$ is defined as the first difference of Polity2. Next, $\alpha_1(x_{it-1} - x_{it-1})$ is added at the right side (this difference is zero by construction). Then, a couple of manipulations yield

$$\begin{aligned} \Delta y_{it} &= \beta_0 + (\rho - 1) y_{it-1} + \alpha_1 x_{it} + \beta_1 x_{it-1} + \alpha_1 (x_{it-1} - x_{it-1}) + u_{it} \\ &= \beta_0 + (\rho - 1) y_{it-1} + \alpha_1 (x_{it} - x_{it-1}) + (\alpha_1 + \beta_1) x_{it-1} + u_{it} \\ &= \beta_0 + (\rho - 1) y_{it-1} + \alpha_1 \Delta x_{it} + (\alpha_1 + \beta_1) x_{it-1} + u_{it}. \end{aligned}$$

Now we have the error correction model that I estimate in this thesis (control variables excluded):

$$\Delta y_{it} = \gamma_0 + \gamma_1 y_{it-1} + \gamma_2 \Delta x_{it} + \gamma_3 x_{it-1} + u_{it} \tag{8.2}$$

where

$$\gamma_0 = \beta_0, \qquad \gamma_1 = \rho - 1, \qquad \gamma_2 = \alpha_1, \text{and} \qquad \gamma_3 = \alpha_1 + \beta_1.$$

The short term effect of economic growth on democratization is simply the economic growth coefficient, i.e. $\gamma_2 = \alpha_1$. Thus, this effect is the same in the error correction

[4] By including this term, I allow the short and long term effect of growth to have different signs.

model as it is in all the other models. The long term effect however, can be derived using the idea that the long run must represent some kind of a steady state (or balanced growth path). That is, in the long run there should be no changes in x or y (once trends are taken out). Consequently, in the long run we have $\Delta y = 0$ and $\Delta x = 0$. This fact makes it possible to solve equation (8.2) for y in the steady state version of this equation. Inserting $\Delta y = 0$ and $\Delta x = 0$ into (8.2) I now get

$$\begin{aligned}
\Delta y_{it} &= \gamma_0 + \gamma_1 y_{it-1} + \gamma_2 \Delta x_{it} + \gamma_3 x_{it-1} + u_{it} \\
0 &= \gamma_0 + \gamma_1 y + 0 + \gamma_3 x + 0 \\
0 &= \gamma_0 + \gamma_1 y + \gamma_3 x \\
\gamma_1 y &= -\gamma_0 - \gamma_3 x \\
y &= -\frac{\gamma_0}{\gamma_1} - \frac{\gamma_3}{\gamma_1} x. \qquad (8.3)
\end{aligned}$$

The long term parameter of interest is therefore

$$-\frac{\gamma_3}{\gamma_1} = -\frac{\alpha_1 + \beta_1}{\rho - 1} = \frac{\alpha_1 + \beta_1}{1 - \rho}.$$

One should note that it is the long term *level* effects that are estimated in the error correction model. In particular, it is the long term effect of a permanent shift in the income level on level of democracy that is being parameterized.

8.3.2 Long term effects – Results

Table 8.3 reports the results when estimating equation (8.2) using the 2SLS and Arellano-Bond GMM estimators. Model 1 reports the second stage results from a 2SLS regression that includes the standard set of controls as well as control for country fixed effects. Model 2 reports the results from the corresponding Arellano-Bond GMM regression. In model 3 time fixed effects are furthermore controlled for.

Consider first the economic growth coefficient which represents the contemporaneous effect of economic growth. It has a negative sign in all models, but is never statistically significant. One reason for the lack of significance might be that economic growth is no longer lagged. It is likely that it takes a while from the economic growth occurs until it affects democratization. Furthermore, the error correction model is constructed to study long term effects and therefore adds more structure to the model. This can make it more difficult to find significant short term effects. One should note that the income level last year does not have any significant effect on Polity2 changes either.

Table 8.3: Error correction model 1980-2007, long term effects

VARIABLES	(1) 2SLS(2nd)	(2) AB	(3) AB
Polity2 (t-1)	-0.188***	-0.22***	-0.53***
	(-6.15)	(-3.33)	(-4.12)
Economic growth	-0.066	-0.037	-0.019
	(-1.53)	(-1.05)	(-0.71)
Economic level (t-1)	0.240	2.15	1.82
	(0.76)	(1.52)	(1.38)
Regime duration (t-1)	-0.013*	-0.042	-0.15**
	(-1.68)	(-1.07)	(-2.23)
Corruption (t-1)	-0.164***	-0.15	0.032
	(-2.81)	(-1.01)	(0.087)
Oil rents of GDP (t-1)	-0.016**	-0.026	-0.00097
	(-2.51)	(-1.41)	(-0.033)
Civil conflict onset (t-1)	-0.129	-1.28**	-1.49
	(-0.62)	(-2.18)	(-1.36)
Income inequality (t-1) (log)	-0.326	-1.28**	-0.70
	(-1.35)	(-2.05)	(-0.93)
Primary education (t-1)	-2.230	15.0	19.0
	(-0.88)	(0.75)	(0.88)
2SLS 1st stage t-value IV1	-2.14	–	–
2SLS 1st stage t-value IV2	7.13	–	–
Kleibergen-Paap F-statistic	32.85	–	–
Sargan test	–	0.351	0.774
Hansen test	0.244	0.620	0.791
AR(1) test	–	0.000	0.000
AR(2) test	–	0.463	0.652
Observations	2019	1804	1804
Number of countries	113	113	113
Country FE	Yes	Yes	Yes
Year FE	No	No	Yes

Notes: Dependent variable first differenced Polity2. Robust t-statistics clustered on countries in parentheses. Significance levels are denoted by *** p<0.01, ** p<0.05, * p<0.1. IV 1 denotes climatic natural disaster instrument. IV2 denotes global economic growth instrument. Kleibergen-Paap Waald F-statistic test for weak identification. H_0: Equation is weakly identified. Sargan and Hansen test for overidentification. H_0: Too many instruments are not included. AR(1) tests for first order serial correlation. AR(2) tests for second order serial correlation. H_0 in both AR tests is that there is not serial correlation.

Increased Polity2 level last year is predicted to have a negative effect on change in Polity2 this year. This is true in all models. The coefficients are moreover statistically significant at a one percent level. The negative effect is not surprising given that the dependent variable is first differenced Polity2. Thus, lagged Polity2 level is at both sides of the regression. However, on the left hand side it has a negative sign in front of it. Thus, when the Polity2 level one year increases, the first differenced variable next year decreases by construction.[5]

As explained above, the long term effect of a shift in income on democracy is calculated by dividing the lagged economic level coefficient by the lagged democracy coefficient. Accordingly, the long term effect in model 1 is given by $\frac{0.240}{0.188} = 1.276$. A nonlinear test however, reveals that even though positive, this long term effect is not statistically significant (combined z-value of 0.80). In model 2 the long term effect of income on democracy is still predicted to be positive. In particular, it is estimated to be $\frac{2.15}{0.22} = 9.77$. Furthermore, in contrast to model 2, the positive long term effect is now statistically significant at a ten percent level (z-value of 1.66). However, to be comparable with the economic growth coefficient the estimate must be divided by 100.[6] Accordingly, in the long term, a one percentage increase in income (per capita) is predicted to increase Polity2 by 0.0977. The variables are here interpreted in levels. Even though statistically significant at a ten percent level, this effect can hardly be said to be quantitatively important. In model 3 the long term effect of increased income on democracy (both in levels) is given by $\frac{1.82}{0.53} = 3.43$. The non-linear test however reveals that this effect is not statistically significant (z-value of 1.19).

In sum, I do not find strong evidence for any long term effect of income on democracy. If anything however, the results suggest that the effect of income at level of democracy turns positive with time. A positive long term effect of income on democracy could offer a possible explanation for why most wealthy countries in the world are democracies, even though I find the short term effect of income growth on democratization to be negative. Differences between short and long term effects can possibly also provide a solution to the long lasting debate in the literature regarding Lipset's (1959) modernization theory. The disagreement could be the result of researchers talking about (and testing for) different dynamics.

I close this chapter with a brief summary of the regression results presented here. First, they indicate that the negative income growth effect on democratization matters

[5]This is obviously true when evaluating the effect of lagged levels in isolation. However, the statement does not imply anything about the correlation between current and lagged levels of Polity2.

[6]The reason is that the economic growth variable has been multiplied by 100 in the previous part of the study, to make direct interpretation in percentage points possible.

more for democratic and inconsistent regimes than it does for autocratic regimes. However, it should be noted that the results for autocratic regimes may not be reliable as the number of observations is severely reduced. Second I find that the negative income growth effect seems to matter more for countries already on a democratic development path than it does for countries that are heading for autocracy. Finally, I do not find any strong evidence for a long term effect of income on democracy. If anything however, this effect appears to be positive.

Chapter 9

Conclusion

There is wide agreement among academics and policy makers that democracy is a universal value—it brings with it a more secure, stable and prosperous society. Of crucial importance is therefore to identify what creates democracy. In this thesis I study the causal, short term effect of economic growth on changes in democracy. Data tells us that wealthy countries are democratic, and common wisdom suggests these countries are democratic because of their wealth. This observation seems to have founded a premise in the policy debate, namely that higher income must stimulate democracy. Even researchers who do not take this for granted, typically analyze a one-sided hypothesis—the null of no effect is tested against the alternative—higher income causes more democracy.

In this thesis I take one step back. As a point of departure, I make the observation that higher national income means there is more to grab for those who control policy. Also, once in power a wealthy government has more resources to either buy or fight off opponents. Thus, higher national income might provide both incentives and opportunities for some players to push for autocracy. At least in the short run. This renders the one-sided hypothesis improper. Yes, there are good reasons why income should stimulate democracy. But there are also arguments pointing in the opposite direction.

I construct a simple, theoretical model to formalize these ideas. A key result is that when income reaches a certain level, then the equilibrium regime shifts from democracy to autocracy. The purpose of the model is not to make claims about reality. Rather, it is to demonstrate that a positive effect of income on democracy is not so obvious from a conceptual point of view. Thus, we should consult data to form opinions about causality.

Two econometric concerns make the empirical analysis extra challenging. First,

both the level of income and democracy display high persistence. This suggests that these series might contain a unit root. Failing to take unit roots into account normally results in biased estimates. Second, there is the problem of simultaneity. While income might cause democracy, it can also be the case that democracy determines income. For instance, if democratic development stimulates income growth, then the naive OLS estimate will be biased upwards. While most previous work overlooks these econometric issues, I perform a battery of specification tests to get an idea about their relevance. In particular, to look for unit roots I conduct a Dickey-Fuller test on each of the panels, and then combine information from all these tests into a single statistic. The procedure is known as a Fisher type (unit root) test for panel data. The results reveal that presence of unit root is likely in both income and democracy, when these variables are measured in levels. I perform two tests for simultaneity. The first is a Granger causality test, where each variable is regressed on its own lags as well as the lags of the other variable. I find that income Granger causes democracy, but also that democracy Granger causes income. This is consistent with causality running in both directions, and hence a simultaneity problem. The results are significant at a ten percentage level. The second test is a Hausman test, where OLS estimates are compared with those from a model with instruments. Coefficients in the second model are consistent under the null, implying that the OLS results are biased if these are different. I find that they are, a further indication that there might be a simultaneity problem.

With these results in mind, I proceed to ask the question: What can the data tell us about causality when we take i) non-stationarity and ii) simultaneity into account? To the best of my knowledge, this thesis presents the first attempt to address that question in a systematic way. The unit root problem is solved by taking the first difference of income and democracy, and use the resulting values in the empirical analysis. Consequently, it is the effect of economic *growth* on *changes* in democracy that is estimated. However, if the level equation is correctly specified, then the estimate from the first differenced equation will effectively reflect level effects. Intuitively, higher economic growth *must* give higher income levels. This is true by the very definition of these concepts. The same goes for the link between level and change in democracy. I take the simultaneity problem into account by using an instrument variables approach. That is, I estimate the short term causal effect of economic growth on democratization by means of the two stage least squares estimator and the Arellano-Bond GMM estimator. Climatic disasters and global economic growth are used as instruments in the first case. Lags of economic growth are used in the second.

The empirical analysis, which is based on a cross-country panel dataset from 1980 to 2007, provides a number of interesting results. First, I show that the data are capable of reproducing an estimated positive effect of income on democracy when unit roots are not taken into account. Second, I demonstrate that this effect turns negative once the variables of interest are first differenced. However, the estimated coefficients are small in magnitude, and therefore of limited significance from a policy perspective. Third, I find considerably larger estimates in absolute value when economic growth is instrumented by climatic disasters and global income growth. That is, the point estimates drop by a magnitude of ten, from about -0.02 to -0.2. The latter implies that an increase in the growth rate by one standard deviation (7.4) reduces democratization by about 1.46 the next year. This is arguably a very large decline, especially given that a standard deviation change in the growth rate is quite common, while at the same time the Polity2 variable is bounded between -10 and 10. Fourth, a more modest negative effect is found using Arellano-Bond GMM. Then the point estimate is -0.05, implying that the standard deviation increase in the growth rate reduces democratization with about 0.4 the next year. In total, I show that the estimated effect goes from positive to negative and statistically significant when the data are first differenced, and to negative and quantitatively significant when growth is instrumented. These results question the popular notion that income stimulates democracy. While contrasting most existing income–democracy studies (Fayad, Bates and Hoeffler (2012) being the exception), my work is more in line with findings from the resource-curse literature. There one emphasizes how higher income can pose a challenge rather than a blessing to countries.

A large battery of tests is set up to check how robust the main results are. In particular I look at alternative democratization measures, estimate Logit models, use an alternative instrument, discard potential influential observations, estimate censored models, exclude control variables with many missing observations, and extend the time series to 1950. Interestingly, the results in the main analysis prove relatively robust to these tests. The point estimates always remain negative, although not significantly different from zero in some of the specifications.

I then ask what kind of observations are driving the results. The answer provided is threefold. First, if anything, it seems that the negative effect is most important in countries that are relatively democratic. Second, it seems that the effect is stronger for countries on a transition path towards democracy. It is less strong in countries on a path towards autocracy. Third, I find that the long term effect, if anything, is positive. However, the long term effect is not significant at the ten percentage level.

Furthermore, all these three results are hampered by trouble with few observations.

Even though I look into short term effects, a negative effect of income growth on democratization should have substantial policy implications. Governments in developed countries, international financial agencies and aid organizations, seem to base their policies on an underlying assumption that increased income has positive implications for democracy, peace and development more generally. If the effect of income on democracy instead is negative, then providing financial aid and encouraging economic development in poor autocratic countries can come at the expense of hampering democratization. The solution to this problem should not be to stop money transfers to poor countries all together. Rather, one should ask *how* money transfers can be designed to prevent negative effects on democratic processes. It is possibly better to commit to long term aid programs rather than one time transfers, given that the negative effect of income on democracy seems to hold only in the short term. Furthermore, as the theoretical framework suggests that the negative effect is the result of rent-seeking behavior by the elite, payments should be conditioned upon being used for the common good, rather than diverted for personal benefits for the few.

There are a number of factors that might lead the reader to question the results from this thesis. In particular, one might ask whether results from a growth analysis can really be used to say something about level effects. Furthermore, the assumption of linearity might not hold. The instruments used in the benchmark regression can be endogenous. There might be other data that are better suited for studying the research question at hand, and other variables that should be controlled for. Moreover, the results could have turned out differently if I had data ranging back to the 1800s etc. Nevertheless, the results from this thesis suggest that one should not take for granted the convention that income stimulates democracy. Furthermore, one should go back and critically assess the existing literature at the field and ask whether studies have been too constrained by a one-sided hypothesis and preconceived assumptions about the link between income and democracy.

Chapter 10

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Appendix A

Review of formulas for geometric series

A generic player i's period utility in period t, denoted $u_{i,t}$, is determined by his own action and the other player's action (in that period).However, in infinitely repeated games, the players want to maximize their *lifetime utility* denoted U. The lifetime utility for player i is assumed to be an infinite discounted sum of period utilities, where $\delta \in (0,1)$ is a time discount factor:

$$U = u_{i,0} + \delta u_{i,1} + \delta^2 u_{i,2} + \ldots = \sum_{t=0}^{\infty} \delta^t u_{i,t}. \tag{A.1}$$

The discount factor effectively downplays the present value of future realizations, and captures the notion that players care more about the current than the future. Throughout the thesis I will make extensive use of the formula for a geometric sum. The formula is repeated below for convenience. Consider a geometric sum with k number of terms and quotient $\delta \neq 1$. The sum can then be written (see Sydsæter, Seierstad and Strøm (1990) for details)

$$\delta^0 + \delta^1 + \delta^2 + \delta^3 + \ldots + \delta^{k-1} = \frac{1-\delta^k}{1-\delta}. \tag{A.2}$$

Notice that if $|\delta| < 1$, then taking the limit as $k \to \infty$ gives

$$\lim_{k\to\infty} \frac{1-\delta^k}{1-\delta} = \frac{1}{1-\delta}. \tag{A.3}$$

Appendix B

Analysis

Table B.1: Percentage missing values 1980-2007

Variables	Missing	Total	Percent missing
Polity2	277	4406	6.29
Economic growth	114	4406	2.59
Climatic disaster	155	4406	3.52
Global economic growth	114	4406	2.59
Regime duration	218	4406	4.95
Corruption	1380	4406	31.32
Oil rents of GDP	1090	4406	24.74
Civil conflict onset	3	4406	0.07
Income inequality	82	4406	1.86
Primary education	724	4406	16.43

Table B.2: Summary statistics for robustness models

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**N**
Sip2	0.535	0.376	0	0.98	3982
Change Sip2	0.006	0.090	-0.887	0.958	3908
FHI	3.836	2.026	1	7	4221
Change in FHI	-0.031	0.456	-3.5	4.5	4252
ALCP	0.476	0.499	0	1	4363
Change ALCP	0.009	0.141	-1	1	4335
Growth in rainfall	0.008	0.105	-0.468	0.768	780

Figure B.1: Cook's D, 1980-2007

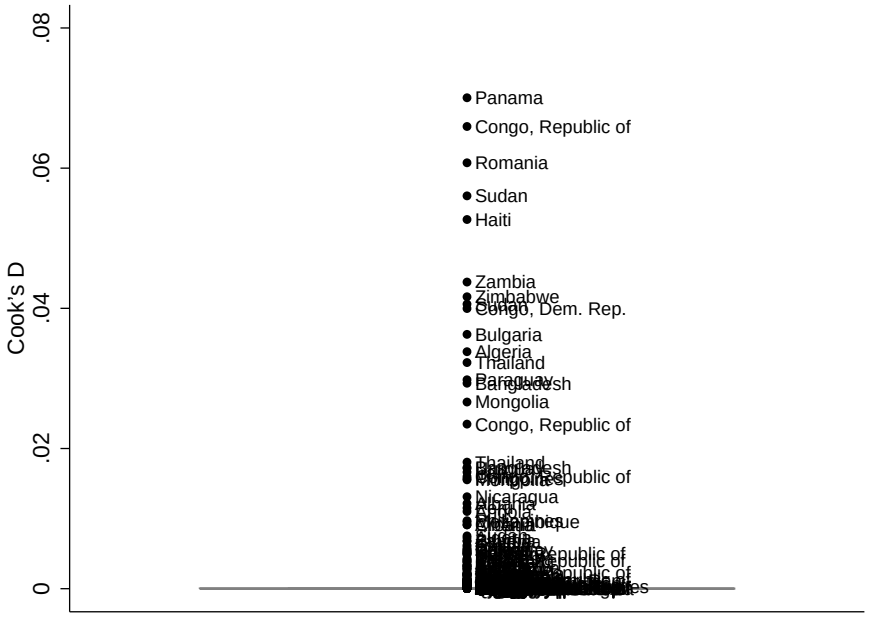

Figure B.2: Dfbeta, 1980-2007

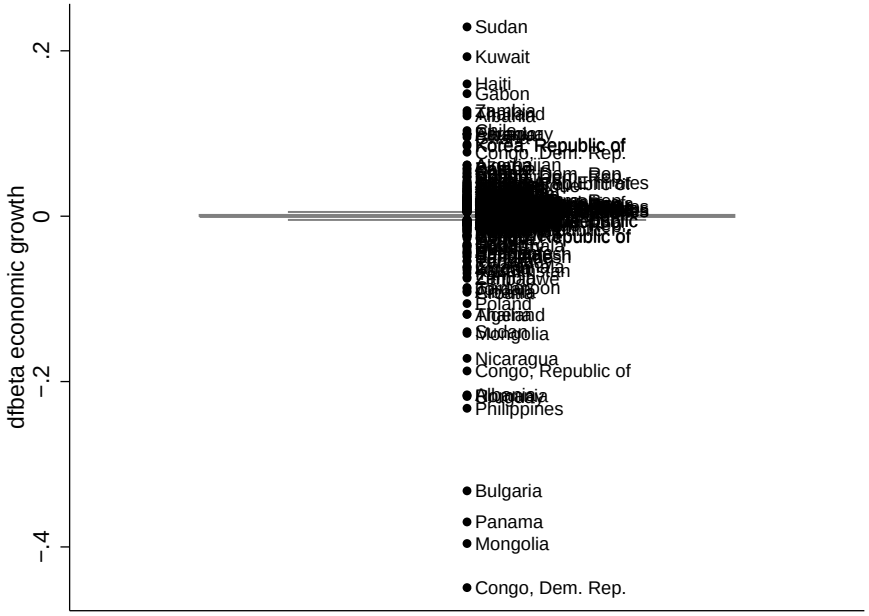

Table B.3: Cross-correlation table

Variables	Polity2	Economic growth	Duration	Corruption	Oil	Conflict onset	Income inequality	Primary education
	1.000							
Economic growth	-0.0718	1.0000						
Duration	-0.0553	0.0393	1.0000					
Corruption	-0.0640	-0.0234	0.4773	1.0000				
Oil	-0.0491	0.0089	-0.0765	-0.2670	1.0000			
Conflict onset	-0.0048	-0.0031	-0.0123	-0.0644	0.0563	1.0000		
Income inequality	-0.0599	0.1706	0.1116	0.1833	-0.0101	0.0116	1.0000	
Primary education	0.0071	-0.1516	-0.2711	-0.3482	0.2027	0.0355	-0.0882	1.0000

Appendix C

Stata do-file

Table C.1: Frequently used regression commands in STATA

Model	Command
OLS	`reg dep_var var_list if year > 1979 & year < 2008, robust cluster(country)`
OLS-FE	`xtreg dep_var var_list if year > 1979 & year < 2008, fe robust cluster(country)`
2SLS	`xtivreg2 dep_var (end_var = iv) if year > 1979 & year < 2008, fe robust cluster(country) first`
AB-GMM	`xtabond2 dep_var end_var var_list if year > 1979 & year < 2008, twostep noleveleq gmmstyle(end_var var_list, lag(2 .) c) nomata robust`
AB-GMM	`xtabond2 dep_var end_var var_list t36-t58 if year > 1979 & year < 2008, twostep noleveleq gmmstyle(end_var var_list, lag(2 .) c) nomata robust`

All Stata codes are on the accompanying CD. The do-file can also be provided upon request, contact me at ingriras@student.sv.uio.no.