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How do Political and Economic Institutions Affect Each Other?

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Abstract

This paper provides evidence for the mutually reinforcing relation of political and economic institutions. To overcome problems of endogeneity I utilize lag instruments within a GMM framework for dynamic panel data. Employing recently developed tests, I show that limiting the number of lag instruments and collapsing the instrument matrix eliminates many and weak instrument biases. My major findings are that (i) improving economic institutions has a large positive effect on future political institutions, and (ii) political institutions have a positive but quantitatively smaller effect on current economic institutions. In addition, (iii) political instability positively affects future political institutions. In line with predictions from the institutional literature, the timing of effects is such that political institutions depend on lags of explanatory variables, while economic institutions are contemporaneously determined. Moreover, results are driven by countries with initially low political institutions implying that in these countries, much is to be gained from institutional reform.

JEL Classification: P16, O10

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1 Introduction

The role of institutions as a fundamental driver of economic development has raised the question of how they are themselves determined (see e.g., Acemoglu et al. (2005a), or Besley and Ghatak (2009)). The research on determinants of institutions has noted that various types of institutions - particularly political and economic institutions - interact and evolve jointly over time (see also Besley and Persson (2011b)). However, so far empirical studies fail to identify the mutually reinforcing and dynamic relation of well measured, distinct political and economic institutions. As a consequence potential reformers find that crucial questions remain unanswered. For example, how does reform targeted at one specific institution affect other institutions, and could it initiate overall institutional development, ultimately leading to economic prosperity? In the phrasing of Acemoglu and Johnson (2005), it is therefore necessary to "unbundle" institutions if we are to explain exactly how and why institutions develop in clusters.

This paper empirically studies the joint dynamic development of (i) political institutions and (ii) economic institutions, with a focus on their mutually reinforcing relation, and its dependence on political instability. Political institutions are measured by *Constraints on the Executive* and economic institutions by *Property Rights Protection*. Both measures capture *de facto* institutions - institutions as they are effectively practiced.[1] The two measures correspond to two institutional functions theorized to be jointly important for economic development (see e.g., Acemoglu et al. (2005a) and Besley and Persson (2011b) for a general reference). One function is to place constraints on the despotic behavior of governments, the other is to provide a good environment for economic activity such as well established property rights protection. The theoretical literature suggests that institutions interact in the following way. Political institutions affect property rights, through the decision making power they provide (e.g., over the legal framework), which in turn influence political institutions via their distributional consequences. The empirical challenge then is to estimate the dynamic development of institutions and deal with the endogeneity that originates from their mutual influence.

The econometric approach presented here utilizes a dynamic panel model that employs the well known Arellano and Bond (1991) and Blundell and Bond (1998) GMM estimators. The setup allows me both to estimate short term (5 years) effects, and then compute long run outcomes. Furthermore, the estimators are designed to

[1] See section 2 for details on institutional measures. Note that *de facto* measurement of institutions is not to be confused with the concept of "de facto" power in Acemoglu et al. (2005a), which refers to a type of power (opposed to "de jure" power).

deal with the endogeneity of regressors by using lagged values as (internal) instruments. In practice, the Arellano-Bond and Blundell-Bond estimators are associated with problems related to many and weak instruments.[2] To overcome these problems, I reduce the instrument count by (i) limiting the lags used as instruments and (ii) by collapsing the instrument matrix (see Roodman (2009b) for details). Furthermore, I employ recently developed tests of instrument validity and strength to provide evidence that these practices together, substantially decrease the bias stemming from too many (and weak) instruments, and achieve identification.[3]

The major result of this paper is the empirical support for the mutually reinforcing relation of political and economic institutions. The effect of economic institutions on political institutions is large, whereas political institutions have a quantitatively small effect on economic institutions. To illustrate, a one standard deviation improvement in the quality of property rights leads to a tightening of constraints on the executive that corresponds to the difference between e.g., the Ukraine and the United Kingdom in 2010. This novel finding indicates that institutional reform towards better economic institutions, in the sense of stronger and more equal property rights protection, can be used by potential reformers as an instrument to initiate development of better political institutions as well.

There are two important qualifications to the main finding. First, the result that political and economic institutions reinforce each other is driven by countries that initially have less developed political institutions. That means that more can be gained from reform in countries with less constrained executives, as proposed by the theoretical literature (see section 2.1). Second, with respect to the timing of effects, I find that current economic institutions affect future political institutions, while the effect of political towards economic institutions is contemporaneous. The evidence thus supports the timing proposed by Acemoglu et al. (2005a).

An additional result concerns the role of political instability for institutional change. Political instability here refers to a government's perceived threat of losing office as a consequence of conflict, or similar mechanisms that are not constitutionally regulated. This threat of losing office is theorized to incentivize governments to share political power in order to avoid its total loss. In this way political instability acts as a fundamental driver of institutional change (McGuire and Olson (1996), Acemoglu and Robinson (2006), Besley and Persson (2011b)). I propose to measure

[2]The problems of the GMM estimators are well studied in the empirical growth literature. See e.g., Bond et al. (2001), Hauk and Wacziarg (2009), Bun and Windmeijer (2010), and Bazzi and Clemens (2013). My application for a different context is informed and inspired by these studies.

[3]The application follows Bun and Windmeijer (2010), and Bazzi and Clemens (2013) who utilize these tests for GMM estimates in the context of economic growth.

political instability by a country's past experience of internal conflict.[4] Under the assumption that conflict experience makes violent opposition today more likely this will shape a governments perception of the threat to its office (see section 2.1 for details on the measure of political instability). My results indicate that the effect of political instability on economic institutions is negative and very small. In contrast, political instability has a positive, and quantitatively more important effect on political institutions. In terms of the previous example, four additional years with internal conflict experience over the last fifteen years, would, on average, lead to a strengthening of executive constraints that corresponds to half of the difference between Ukraine and the United Kingdom in 2010.

Furthermore, I find that an interaction of political instability and economic institutions negatively affects political institutions. This means that the positive effect of political instability on political institutions is smaller in the presence of better economic institutions. The same holds for the positive effect of economic on political institutions. A possible explanation is that property rights can, to some extent, protect citizens' economic interests against despotic governments. Political institutions, that otherwise place checks and balances on governments, will then be less important, as their low level is partly compensated by economic institutions, which explains that the positive effect of political instability is smaller. However, the positive conditional effects of political instability and economic institutions dominate the negative interaction, so that both variables have an overall[5] increasing effect on political institutions.

The related empirical literature on political institutions mainly investigates the effects and determinants of democracy - not constraints on the executive. The reason is the observed correlation of democracy with economic institutions and development, which inspired theories suggesting that democracy limits despotic behavior of governments, and induces them to provide better economic policies and institutions (McGuire and Olson (1996), Acemoglu (2006)). However, recent contributions argue that constraints on the executive are a better and more direct measure of such limits, and can explain economic institutions and outcomes (Acemoglu et al. (2001), Acemoglu and Johnson (2005)) not only of democracy, but also the good performance of some autocracies (see in particular Besley and Kudamatsu (2007)).

That said, the strong and positive effect of economic institutions on constraints

[4] Internal conflict over government with more than 1000 battle deaths per year from the UCDP/PRIO Armed Conflict Dataset.

[5] "Overall" here refers to unconditional marginal effects evaluated at the median (or mean), taking into account the constitutive (conditional effect) and the interaction term. All discussions in terms of standard deviations are based on these unconditional marginal effects.

on the executive is in line with predictions from the theoretical literature that connect economic institutions and their distributional effects to constraints on the executive (Besley and Persson (2011b)), and political institutions in general (Acemoglu et al. (2005a), Acemoglu and Robinson (2006)). My second finding, that political institutions positively affect property rights in turn lends support to theories which propose that limiting political power will indeed lead to less despotic behavior and better provision of economic institutions which profit a large share of the population (see Acemoglu (2006), Besley and Kudamatsu (2007), and Besley and Persson (2011b)). It is an important substantiation to the empirical work of Persson (2005) who fails to find an effect of political institutions (specifically of reforms into parliamentary democracy) on a measure of property rights protection.[6]

The work in this paper is also related to the literature on how political instability and conflict are connected to political and economic institutions. On the empirical side, the result that political instability increases executive constraints is in line with Besley et al. (2013) who find that political uncertainty has similar effect using the random death of political leaders as exogenous shock to political stability.[7] Regarding the role of political instability, my findings lend support to the theory that reform of political institutions is a result of revolutionary pressure (Acemoglu and Robinson (2006)), or investment in institutions under the threat of losing political office (Besley and Persson (2011b)).

The next section explains in more detail how my empirical model is connected to and motivated by the theoretical literature of institutional development. The section also shows how these theories influence my choice of institutional measures. Section 3 then presents the empirical model, and introduces the GMM estimators as well as tests for instrument validity. Section 4 discusses the main results, and section 5 their robustness. Finally, section 6 concludes.

2 Institutions in Theory and Empirical Counterparts

This section explains how the choice of specific institutions, their measures, and the formulation of the empirical model below is guided by theories of institutional determinants and interactions.

[6]Note that Persson (2005) finds a moderate effect of reforms into parliamentary democracy on a combined measure of property rights and trade openness. As the effect on property rights alone is not significant this has very different implications compared to the findings of this paper.

[7]Very broadly the paper is also connected to Aidt and Jensen (2011), and Przeworski (2009) who find positive effects of the threat of revolution on the extension of the franchise.

Political and Economic Institutions. The theoretical literature suggests two aspects of institutions that are essential for a functioning developed country to emerge. First, political institutions have to be such that they constrain the ruler's power in order to avoid despotism and political uncertainty (see e.g., McGuire and Olson (1996), Acemoglu (2006), Besley and Kudamatsu (2007)). Second, the state has to be strong enough to provide valuable public goods and establish productivity enhancing economic institutions such as legal protection (see e.g., McGuire and Olson (1996), Besley and Persson (2010)). Acemoglu (2005) and Besley and Persson (2011b) theorize that having institutions that fulfill both functions is an important condition for long term economic prosperity and stability.

Going from theory to empirics, a first challenge is to find measures of institutions that are close to the theoretical concepts. To capture the limiting function of political institutions, I follow the literature (Acemoglu et al. (2001), Acemoglu and Johnson (2005), and Besley et al. (2013) among others) in the argument that this is best done by the Polity IV index of *Constraints on the Executive*. The index measures to what extent the executive branch of a government is subject to institutionalized de facto control by groups or individuals outside the government when making decisions. One example of high executive constraints is a parliamentary democracy where most legislation is made by the parliament, or the parliament has the power to assign the executive branch of government. But high executive constraints could also apply to a monarch or autocrat who depends on the acknowledgment of a group of lords, oligarchs, or any other identity (see the data appendix and Marshall et al. (2013) for a detailed description). This demonstrates that the concept of constraints on the executive is distinct from democracy and more closely represents the limiting function of institutions.[8]

The other institutional dimension in the focus of this paper is the economic dimension. One particularly important economic institution is the degree of property rights protection. The productivity enhancing role of strong and equal property rights protection has been well established (see Besley and Ghatak (2009) for an overview), and the empirical literature has proposed a variety of indices of their quality. However, the most commonly used measures are only available for a cross section or a short panel (e.g., the Political Risk Services' measures). Since the dynamics of institutional development are the focus of this study I make use of the data set with the longest time dimension. This is the index *Property Rights Protection* provided by the Canadian think tank Fraser Institute and its international

[8]Besley and Kudamatsu (2007) lend further support to the importance of executive constraints. They find that autocracies have better economic performance when a group controls the leader.

collaboration Economic Freedom of the World (see Gwartney et al. (2012)) which is available for the period 1975-2012 in (at least) five-year intervals. The index is the combined score from several categories that directly cover the legal framework but also other outcome oriented, or *de facto*, aspects such as effective law enforcement (see the data appendix for more detail). The index is comparable to property rights measures previously used in the literature, for example the Political Risk Services' measure of "Law and Order" (used by e.g., Hall and Jones (1999), Acemoglu et al. (2001), and Besley and Persson (2009b)), with which it has a correlation of 0.87.

Political Instability and Institutions. Property rights and constraints on the executive are the two institutions that are the focus of my study. Specifically, I pose the question of how one type of institution affects the other. These effects cannot be studied without taking into consideration other factors that shape and determine institutions. One such factor is political instability.

Theoretical models emphasize the role of political instability, the risk faced by an incumbent government of losing its political power, in explaining how political and economic institutions develop and affect each other (e.g., McGuire and Olson (1996), Acemoglu (2005), Acemoglu and Robinson (2006), Acemoglu and Robinson (2008), Besley and Persson (2011b)). For example, risk of losing power is at the core of the seminal work of Acemoglu and Robinson (2000) that explains democratization as a consequence of revolutionary threats to the incumbent's political power. Besley et al. (2013) tell a related story where the incumbent group initiates reform towards more limits on future governments, in order to prevent transfers away from itself, when it is likely to lose political office.[9]

Empirical studies have made different attempts to assess the effects of political instability on political institutions. The literature employs for example the random deaths of political leaders (e.g., Jones and Olken (2009), Besley et al. (2013)), or, more common in political sciences, riots and strikes (e.g., Przeworski (2009)), and revolutionary activity in neighboring countries (Aidt and Jensen (2011)). For this paper's dynamic panel model, the event studies of deaths of leaders are less well suited and the proxies for revolutionary activity are not available for a large part of my sample period.

Instead, I propose to measure political instability in terms of conflict, and focus

[9]Note that the mechanisms driving institutional reform, as a reaction to risk of losing political power, suggested in Acemoglu and Robinson (2000) (and Acemoglu and Robinson (2006)), and Besley et al. (2013) (and Besley and Persson (2011b)) are similar. Most importantly both give answers to the question why a government would limit or share its own power. Therefore the theory of democratization applies to political institutions in general and provides valuable insight for constraints on the executive, which, as argued above, is a more relevant dimension of political institutions.

on this particular channel. The main idea is that if a country experienced internal conflict in the past, this is an indication of political instability. Political instability here is defined as the perceived threat that a government will lose office by any means other than regulated succession.[10] Thus the assumption behind the conflict based measure is that the government of a country that experienced conflict in the past will expect a larger threat to its power and a higher probability of losing.[11] It is this threat that drives the mechanism leading to institutional change in the theories of Acemoglu and Robinson (2000) and Besley et al. (2013). In the light of past experience of conflict, governments expect a higher threat that will induce them to be cautious about weakening institutions, or even strengthen them in the hope of avoiding present conflict that may result in their loss of office.

The proxy for *Political Instability* I construct is based on a conflict indicator from the UCDP/PRIO Armed Conflict Dataset (Gleditsch et al. (2002), Harbom and Wallensteen (2012)). This indicator is a dummy variable equal to one for each country year of civil war, defined as internal conflict with at least 1000 battle deaths, where the subject of conflict is control of the government. The political instability proxy that measures conflict experience is calculated as the running sum of the conflict dummy over the last 15 years for each country year. There are two implicit assumptions incorporated in this proxy. First, some conflict experience increases the (perceived) probability of losing power, and the more years of conflict a country experiences the higher is that probability. Second, conflict experience depreciates fully, i.e., is forgotten, after 15 years.[12]

To summarize, the measure is based on the assumption that conflict experience, on average, indicates higher (perceived) political instability compared to no conflict. There may be concern that battle deaths, and therefore conflict, are the outcome of repression by the government, and that repression reduced the risk of losing office (see e.g., Besley and Persson (2009a) for the theoretical argument).[13] However,

[10]This is similar to what the World Bank aims to capture in its measure of political stability which is defined as "[..] perceptions of the likelihood that the government will be destabilized or overthrown by unconstitutional or violent means, including politically-motivated violence and terrorism." However, the problem with the World Bank measure is that it only covers the short period $1996 - 2010$.

[11]Some evidence for the link between conflict and risk of losing office is provided by Jones and Olken (2009) who find that assassinations increase not only democracy but also small scale conflict.

[12]Note that the choice of 15 years as "conflict memory" is arbitrary. However, below I show that results are very similar for conflict measures based on the running sum over 10 or 20 years. In addition, I split the data in a subsample of countries that never experience conflict, and a sample that experiences at least some conflict. Again results are very similar. In sum all evidence supports the validity and robustness of results based one a 15 year "conflict memory".

[13]This suggests that the proxy of political instability could theoretically be improved by a measure of repression. Besley and Persson (2011a) introduce an ordered conflict measure that distinguishes between two sided conflict (they also use the civil war measure from the ACD but do not

repression generally is the response to a perceived threat so that on average it should also indicate a higher risk of losing office.

Political instability as measured by conflict experience captures one particular channel through which political power and office is determined. As this paper's focus is on development of institutions in countries that start off with low political institutions, the conflict channel is expected (see the predictions in the next section) to be important. Having said that, there are other, peaceful mechanisms, as specified by a country's constitution or established through practice, that determine the rate of turnover in political office and these may also affect institutions. Section 5.3 provides evidence that one such mechanism, the *de facto* regulation of turnover, does not directly affect institutions. At the same time the effect of political instability (as measured by conflict experience) on institutions remains strong. This finding provides evidence for the relevance and validity of the mechanism and measure proposed here, and I proceed to use conflict experience as the main measure of political instability.

2.1 Predictions

This section summarizes and explains briefly the main theoretical predictions of the institutional literature about the relations between political institutions, economic institutions, and political instability. The predictions will then guide the formulation of and be tested by my empirical model .

Two points need to be discussed before formulating specific hypothesis. The first, concerns the relevant sample. The main focus of the theoretical literature used to derive the predictions below are the emergence and development of institutions under low initial political institutions. Thus all predictions should be understood as being conditional on low levels of constraints on the executive. Previous empirical studies find this conditionality, for example, for the effects of political instability on political institutions (Jones and Olken (2009), and Besley et al. (2013)). The results in this paper also confirm this theoretically implied conditionality. Institutional interactions and effects of political instability are weak or unidentified in the full sample. In contrast, effects are strong in a sample of countries with initially low constraints on the executive.

Second, Acemoglu et al. (2005a) suggest that political institutions depend on lags of economic institutions, while economic institutions depend on current values of political institutions. This distinct time structure arises because the effect of

restrict it to conflict over the government) and repression. Unfortunately the repression data are not freely available.

economic institutions works through their distributional consequences, which take time to manifest. In addition, political institutions tend to be more persistent. Note that this argument can be extended to effects from other variables as well - which is in line with the empirical literature that generally models political institutions as a function of lags of explanatory variables. The other side of the prediction is that economic institutions depend on current values of political institutions because the executive has a formal mandate to influence economic institutions.[14] As economic institutions are decided contemporaneously, other factors also have a contemporaneous effect. The main results presented below provide evidence for this hypothesis. In the robustness section (section 5.1) I also test for different timings, and show that the timing proposed by Acemoglu et al. (2005a) receives stronger support from the data.

The four main predictions, conditional on low initial executive constraints and incorporating the suggested timing, are the following:

1. *Better property rights protection in period $t-1$ increases constraints on the executive in period t.* Better property rights in the sense of well protected impartial rights to all, shifts wealth and therefore economic power towards a majority of people. Groups that are not represented by the government will then seek to have this shift matched in political power.[15] See e.g., Acemoglu et al. (2005a), Acemoglu (2006), and Besley and Persson (2011b).

2. *More constraints on the executive in period t lead to better property rights protection in period t.* This can be explained from two perspectives. First, incentives to invest in property rights are raised, as a constrained future government cannot appropriate the payoffs of the investment. Second, the constraints will lead to a broader representation of different groups in politics that can force the government to invest in economic institutions. See McGuire and Olson (1996), Acemoglu (2006), Besley and Kudamatsu (2007), Besley and Persson (2009b)).

3. *Higher political instability (more conflict experience) in period $t-1$ leads to more constraints on the executive in period t.* This is the effect of risk of losing office (Besley et al. (2013), Besley and Persson (2011b)) or revolutionary threat (Acemoglu and Robinson (2000), Acemoglu and Robinson (2006)). In order to protect economic interests the incumbent initiates reform to constrain its own government, and share political power when faced with such a risk.[16]

[14] This is what Acemoglu et al. (2005a) refer to as "de jure" power.

[15] This channel captures efforts of groups to change political power by means other than conflict.

[16] Remember that all predictions are conditional on low initial constraints on the executive. No

4. *Higher political instability in period t may have a negative or positive effect on property rights in period t.* The classic prediction is the negative effect as consequence of uncertainty about returns on investment in institutions in the future (McGuire and Olson (1996), Besley and Persson (2009b)). However, the theory is conflicting as it suggests the possibility of an opposing positive effect. The positive effect occurs if economic institutions work in a similar way to political institutions to limit despotic behavior (Acemoglu (2006)). Better property rights could then be used to appease the revolutionary threat (Acemoglu and Robinson (2006)) as an alternative (substitute) to political reform.

In addition to the four main predictions, I also test if the effects of economic on political institutions, and vice versa of political on economic institutions, are conditional on the level of political instability. While the theoretical literature has focused less on these interactions it discusses their possibility. For example economic institutions may act as substitutes for political institutions (Acemoglu and Robinson (2006)). If so, constraints on the executive would become less important with better property rights protection, and the effect of political instability on political, or economic, institutions would be dampened. On the other hand, Besley and Persson (2009b) suggest that the negative effects of political instability on economic institutions may be especially large in countries with low constraints on the executive. The question of existence and direction of such interaction effects remains to be answered by the data. In the next section, I present the empirical approach that will allow me to answer the question and test the above predictions.

3 Empirical Strategy

With the measures for political institutions, economic institutions, and political instability, I construct an unbalanced panel data set for 109 countries with five-year observations in the period 1975-2010. A few control variables typically used in the empirical literature[17] are added: GDP per capita (from the Penn World Tables 7.1 (Heston et al. (2012))), a measure for education (from Barro and Lee (2013)), and a dummy for oil producing countries (from Papaioannou and Siourounis (2008a) - see the data appendix for more detail). The panel setup fits my interest in dynamic development of institutions well. On the one hand, it allows the estimation of

claim is made as to the effects of instability under good political institutions. Thus this prediction does not imply that low political instability will cause a reversal of institutional development once constraints on executives are high.

[17]See e.g., Barro (2012), Acemoglu et al. (2009), or Bobba and Coviello (2007).

immediate effects and is thus distinct from papers that investigate the long run effects of institutions (e.g., Acemoglu et al. (2009)). At the same time it enables me to investigate the dynamics and gradual change of institutions over a longer period, in contrast to the before and after study of large institutional reforms (e.g., Persson (2005), and Giavazzi and Tabellini (2005)).

Equation (1) describes the evolution of political institutions PI (measured by constraints on the executive) for country $i = 1, ..., N$ in period $t = 0, ..., T$.[18] The linear empirical model has the advantage of accounting for continuous relations between institutions and is commonly used in the democratization literature.

$$PI_{i,t} = \beta_1 PI_{i,t-1} + \beta_2 EI_{i,t-1} + \beta_3 \Gamma_{i,t-1} + \beta_4 EI_{i,t-1}\Gamma_{i,t-1} + \mathbf{X}_{i,t-1}\boldsymbol{\beta}_5 + u^1_{i,t} \ . \quad (1)$$

Political institutions depend on their own lagged value, economic institutions EI (measured by property rights), and on political instability Γ. The interaction of political instability and economic institutions is added to allow for conditional effects in the empirical model. Control variables are subsumed in the vector $\mathbf{X}$. All regressors are lagged, in accordance with the theoretical prediction about the timing of effects (Acemoglu et al. (2005a)) and previous empirical work, to account for the slow moving nature of political institutions. The composite error term $u^1_{i,t} = \mu^1_i + \varepsilon^1_{i,t}$ consists of a country specific effect μ^1_i and an idiosyncratic shock $\varepsilon^1_{i,t}$. The superscript indicates that the error term is specific to the equation for political institutions (equation (1)).

A similar equation describes economic institutions:

$$EI_{i,t} = \delta_1 EI_{i,t-1} + \delta_2 PI_{i,t} + \delta_3 \Gamma_{i,t} + \delta_4 PI_{i,t}\Gamma_{i,t} + \mathbf{X}_{i,t}\boldsymbol{\delta}_5 + u^2_{i,t} \ , \quad (2)$$

where $u^2_{i,t} = \mu^2_i + \varepsilon^2_{i,t}$. The interaction political institutions and political instability is added to account for the possibility that the effect of political on economic institutions depends on political instability. In contrast to equation (1), all regressors enter as contemporaneous values, reflecting the different timing suggested by Acemoglu et al. (2005a).

For a further discussion of the assumption about the errors, denote $u^J_{i,t}$ for $J \in \{1, 2\}$, indicating the error of equation (1) and (2). I make the standard assumptions that both components of the error term have expected values of zero, $E[\mu^J_i] = E[\varepsilon^J_{i,t}] = 0$, and that the country effects are uncorrelated with the idiosyncratic error $E[\varepsilon^J_{i,t}\mu^J_i] = 0$, for $i = 1, ..., N$ and $t = 0, ...T$. In addition I assume serially

[18] As the data are observed in five-year intervals, one period will refer to five years. The lag $t-1$ then denotes the observation five years before t.

uncorrelated error components $E[\varepsilon^J_{i,t}\varepsilon^J_{i,s}] = 0$ for $i = 1, ..., N$ and $s \neq t$.

There are several features of this empirical model that make identification a challenging task. First, economic and political institutions are endogenous in the sense that they are both dependent variables, i.e., outcomes, in one equation. Their endogeneity requires a strategy to identify their effects when they appear as regressors in the respective other equation. Second, further endogeneity problems cannot be ruled out. Income and education, while affecting institutions, may reversely be caused by those institutions (Papaioannou and Siourounis (2008a)). Third, the dynamic aspect in the evolution of institutions is an additional challenge to identification. In the next section I describe dynamic panel GMM estimators that are designed to deal with these issues and offer identification strategies for the empirical model of institutional development. We will also see that the timing of effects proposed in equations (1) and (2) contributes to solving the endogeneity issue.[19]

3.1 Identification - GMM and Internal Instruments

The presence of fixed effects, lagged dependent variables, and multiple endogenous variables invalidates standard methods for the estimation of equations (1) and (2).[20] Arellano and Bond (1991) and Blundell and Bond (1998) propose GMM estimators for this type of model. The fixed effect is eliminated by first differencing the equations, and endogenous variables are instrumented with their own lagged values. The use of these internal instruments is a pragmatic solution in the absence of good external instruments.

The Arellano-Bond Estimator. The classic Arellano-Bond (henceforth AB) estimator takes the first difference of the equation that is to be estimated (hence also the name Difference GMM) in order to eliminate individual (country) effects μ^J_i. Under the assumption that lagged levels and differences are uncorrelated with future error terms both (levels and differences) are valid instruments for their future differenced values. To maximize the sample size, levels are preferred (see Arellano

[19]This paper follows a strategy to estimate the model in equations (1) and (2), equation by equation. An alternative would be to estimate the two equations jointly as a system. The advantage of joint estimation is its asymptotic efficiency. Having said that, single equation estimation is straight forward and leads to consistent estimation when using appropriate instruments. Furthermore, even though joint estimation is asymptotically efficient, this is not necessarily the case in small samples. In fact, the opposite often holds (Greene (2012) chapter 10.6). Therefore, and due to the advantage of conceptual and computational simplicity I confine myself to equation by equation estimation, and leave the joint estimation as a topic for a future appendix to this paper.

[20]The simple Fixed Effects estimator that transforms the equation by taking first differences to eliminate the individual effects, is biased in the presence of dynamic effects (the lagged dependent variable) - see Nickell (1981). The corrected FE estimator (Bruno (2005)) relies on the assumption that all regressors beside the lagged dependent variable are exogenous, and thus is not a candidate estimator either.

(2003) for details).

To formally describe the moment conditions let me introduce the following notation. The vector of endogenous explanatory variables is denoted by $\omega_{i,t}^J$, the vector of predetermined variables by $\varpi_{i,t}^J$, and that of exogenous variables by $z_{i,t}^J$.[21] Given the assumptions made above Arellano and Bond (1991) suggest estimation based on the following sequential moment conditions:

$$E[\omega_{i,t-s}^J \Delta\varepsilon_{i,t}^J] = 0 \text{ for } t = 2, ..., T \text{ and } s \geq 2 \tag{3}$$

$$E[\varpi_{i,t-s}^J \Delta\varepsilon_{i,t}^J] = 0 \text{ for } t = 1, ..., T \text{ and } s \geq 1 \tag{4}$$

$$E[\Delta z_{i,t}^J \Delta\varepsilon_{i,t}^J] = 0 \text{ for } t = 1, ..., T\ , \tag{5}$$

where Δ denotes first differences, so that $\Delta\varepsilon_{i,t}^J = \varepsilon_{i,t}^J - \varepsilon_{i,t-1}^J$ is the first differenced idiosyncratic error of equation (J). These moment conditions imply that the lagged levels of exogenous and predetermined variables are valid instruments for their own first differences. For endogenous variables the moment conditions in equation (3) specify that lags two and greater are valid, whereas for predetermined variables the first lag is also a valid instrument (equation (4)). The moment conditions in equation (5) entail that exogenous variables are simply instrumented by themselves (for the AB estimator this means, of course, that first differences of exogenous variables are instrumented by themselves).

Endogeneity and predetermination as used here are technical terms that need to be interpreted in the context of the empirical model in equations (1) and (2). Clearly both PI and EI are "endogenous" variables in the sense that they are dependent or outcome variables. However, in light of the technical definitions and given the time structure of the model, this can be differentiated further. In equation (1) both $PI_{i,t}$ and $EI_{i,t}$ are endogenous in the sense that they are allowed to be correlated with current and past, but not with future errors (formally $E[PI_{i,t}\varepsilon_{i,s}^1] = E[EI_{i,t}\varepsilon_{i,s}^1] = 0$ for $s > t$), so that moment condition (3) applies. This implies that lags two and greater are valid instruments. It also means that those lags are valid instruments for the lagged values that appear on the right hand side $PI_{i,t-1}$ and $EI_{i,t-1}$.[22] All other regressors are treated in the same way, i.e., current values are allowed to be endogenous, except for the indicator for oil producing countries, which is assumed to be exogenous (the existence of oil is plausibly exogenous to institutions).[23]

[21] As above, the superscript $J \in \{1, 2\}$ indicates that the set of variables belongs to either equation (1) or (2).

[22] This is equivalent to specifying $PI_{i,t-1}$ and $EI_{i,t-1}$ as predetermined and applying moment conditions (4).

[23] While existence of oil is likely exogenous, its extraction may not be. However, if extraction is positively correlated with institutions a failure of accounting for endogeneity would lead to

In equation (2) where $EI_{i,t}$ is the dependent variable, the regressors enter as contemporaneous values. As $PI_{i,t}$ is a function of lagged economic institutions ($EI_{i,t-1}$) only (see equation (1)), $PI_{i,t}$ is predetermined in equation (2). In addition political instability $\Gamma_{i,t}$ and the interaction are assumed to be predetermined. All predetermined variables can be instrumented with their first, and further lags. However, GDP and education are allowed to be endogenous, and instrumented with lags two and greater.

The instrument strategy laid out here is based on arguably plausible assumptions derived from theoretical literature and considerations. However, below I present tests for the validity of these assumptions, i.e., the exogeneity of the implied instruments. Furthermore, section 5.1 tests the robustness of results with respect to changed timing of effects - the evidence supports the timing in equations (1) and (2).

The Blundell-Bond Estimator. Difference GMM can be biased in small samples due to weak instruments. Intuitively past levels are not necessarily good indicators for current changes (Bond et al. (2001)). Blundell and Bond (1998) suggest a GMM estimator that has been found to perform better in some contexts. Among others, the results presented by Bond et al. (2001), and Hauk and Wacziarg (2009) indicate that this is the case for persistent outcome variables, such as income, in empirical growth models (see below for further discussion).

The idea of the Blundell-Bond (henceforth BB) estimator is to stack the original equations in levels (not transformed into differences) below the differenced equations from the AB estimator and then estimate this system of equations (hence also the name System GMM). Lagged first differences of the endogenous and predetermined variables serve as instruments for the levels equations. This is valid under the additional assumption that the initial change in the dependent variables is uncorrelated with the country effects. Formally, $E[\Delta PI_{i,1}\mu_i^{PI}] = 0,$ and $E[\Delta EI_{i,1}\mu_i^{EI}] = 0,$ for $i = 1, ..., N$, for equation (1) and equation (2) respectively. Bond et al. (2001) (Appendix 6) show that this is equivalent to the assumption of initial stationarity. For all other endogenous (denote the set of endogenous variables excluding the lagged dependent variable of equation J by $\tilde{\omega}_{i,s}^J$) and predetermined variables the additional assumption is that changes are not correlated with the country effects, $E[\Delta\tilde{\omega}_{i,s}^J\mu_i^J] = E[\Delta\varpi_{i,s}^J\mu_i^J] = 0$. These assumptions imply the following (additional)

the conclusion that oil has a positive effect on institutions. Below I find a negative effect of oil on institutions, which may then be seen as an upper bound. Furthermore, my preferred specification is the AB estimator that eliminates any fixed factor including the dummy for oil producing countries. In summary, there is no reason to believe that the exogeneity assumption regarding the oil dummy is driving my results.

moment conditions for the levels equations:

$$E[\Delta\omega_{i,t-1}^{J}\varepsilon_{i,t}^{J}] = 0 \text{ for } t = 2, ..., T \ , \tag{6}$$

$$E[\Delta\varpi_{i,t}^{J}\varepsilon_{i,t}^{J}] = 0 \text{ for } t = 1, ..., T \ , \tag{7}$$

$$E[z_{i,t}^{J}\varepsilon_{i,t}^{J}] = 0 \text{ for } t = 0, ..., T \ , \ . \tag{8}$$

To summarize, Difference GMM follows the strategy to transform (first difference) equation (J) to deal with the fixed effects and use lagged levels as instruments. The System GMM estimator first leaves the equation untransformed and instead transforms (first differences) the instruments. It then combines these equations with the differenced equations, and the estimation is based on this system of difference and levels equations.

3.2 AB vs BB - Properties of the GMM Estimators

Following the observation that the AB estimator can suffer from weak instruments and the development of the BB estimator, a variety of studies compare the small sample properties of both estimators with respect to bias of different sources. For example, Bond et al. (2001) and Hauk and Wacziarg (2009) show that BB outperforms (is less biased than) the AB estimator in the growth context. While this discussion focuses on the convergence rate, the main interest in this paper lies in the institutional interactions. Biases may behave very differently for those parameter estimates.[24] Bun and Windmeijer (2010) show that the biases are related to weak instrument problems that can also be present in the BB estimates.[25] Furthermore, Bazzi and Clemens (2013) find in a simulation study that the BB estimate of the effect of a persistent growth determinant, for instance an institution, can be severely downward biased. Their results suggest that inference bases on the BB estimates can lead to the rejection of a true positive effect, and even to failure to reject a false negative effect of institutions on growth. This finding is especially of concern here where the main interest is in effects of such persistent variables, namely institutions.

The bias problem is related to too many instruments in the AB and BB framework (Bun and Windmeijer (2010), Bazzi and Clemens (2013)), as they are overfitting the equations (see Roodman (2009b) for a detailed discussion). Reducing the number of instruments can be achieved by (i) restricting the number of lags that are used as instruments and by (ii) collapsing the instrument matrix. The GMM

[24]For instance, in the growth context, AB estimates of growth determinants are found to be downward, while BB estimates are upward biased (Hauk and Wacziarg (2009)).

[25]The weak IV problem theoretically disappears N asymptotically but the necessary N turns out to be unrealistically large (Hauk and Wacziarg (2009)).

estimators enter instruments for each period and each lag depth in a separate column of the instrument matrix. Collapsing these into one column for each lag depth can drastically reduce the instrument count.[26] It turns out that this non standard practice of collapsing the instrument matrix (in addition to instrumenting with a limited number of lags) is key for reducing the relative OLS bias in my analysis of institutional development.

Ultimately the performance of the two estimators and the strength of their instruments will have to be tested (see next subsection). The results presented in section 5 indicate that weak instruments in the BB estimator are a problem in my application. The weak instruments cause the BB estimates of institutional interactions to be downward biased, which leads to the conclusion that in this particular context the AB estimator is to be preferred.

3.3 Tests for Instrument Validity and Strength

The previous section discussed the potential problems of the AB and BB estimators with respect to weak and many instruments. This illustrates the importance of testing instrument validity and strength. Tests for overidentification are readily available for the GMM framework. Underidentification and weak instrument tests, however, are not directly applicable to GMM estimates. In this section I describe the available tests and recently developed procedures that apply underidentification and weak instrument tests to GMM estimates, by establishing a parallel between the GMM and the 2SLS estimator (see Bun and Windmeijer (2010), and Bazzi and Clemens (2013)).

Hansen/Sargan Overidentification Test. The first condition of instrument validity is that instruments have to be exogenous in the structural equation. That is, instruments should have no direct effect on the dependent variable. This can be tested formally with a Hansen (1982) J test. The Hansen test is robust to heteroscedasticity but weakened by many instruments. An alternative is the Sargan (1958) statistic which is a special case of the Hansen J test under the assumption of homoscedastic error terms.

The null hypothesis in both tests is that the instruments are valid, i.e., the exclu-

[26] Note that collapsing the instruments for the differenced equations means that the moment conditions in (3) and (4) are replaced by the following moment conditions:

$$E[\omega^J_{i,t-s}\Delta\varepsilon^J_{i,t}] = 0 \text{ for each } s \geq 2 \tag{9}$$

$$E[\varpi^J_{i,t-s}\Delta\varepsilon^J_{i,t}] = 0 \text{ for each } s \geq 1\ . \tag{10}$$

These conditions are based on the same assumptions about the error terms made above. See Roodman (2009b).

sion (order) condition is fulfilled. The Sargan test is not robust to heteroscedasticity but it has the advantage of not being weakened by many instruments. I rely on the Hansen test since I am assuming heteroscedasticity but also report the Sargan test for reference. These tests can be informative. However, Roodman (2009a) warns practitioners not to rely too strongly on either test since they are weak and can generate implausibly high p-values if the number of instruments is large.

Tests for Underidentification. The second condition - the rank condition - for instrument validity is that the instruments must be correlated with the endogenous variables. In the 2SLS context, this means that the parameters of excluded instruments must be jointly significant in the first stage regressions of the endogenous variables. Kleibergen and Paap (2006) propose a Lagrange-Multiplier test which is appropriate for models with multiple endogenous variables, and heteroscedastic errors, such as the one in this paper.[27] The null hypothesis is that the instrumenting equations for all endogenous variables are jointly underidentified. Rejection of the null hypothesis thus indicates valid instruments in the sense of absence of underidentification - there are at least as many instruments correlated with the endogenous variables as there are endogenous variables. Below I discuss how the underidentification test developed for the 2SLS estimator may be applied in the GMM setup.

Weak Instruments. Even if instruments fulfill the order and rank condition (i.e., are exogenous and relevant) identification is not guaranteed. Instruments are weak when they are only weakly correlated with the endogenous variables. Intuitively speaking, weak instruments explain little of the variation in endogenous variables.[28] Formal tests for weak instruments have only been developed recently. Stock and Yogo (2002) propose a test for the detection of weak instruments applicable to models with multiple endogenous variables. The relative bias test explores whether the bias of the IV-2SLS estimator relative to the bias of the OLS estimator exceeds a pre-specified ratio.

The null hypothesis is that the bias is larger than the specified ratio and instruments are considered weak if the null is not rejected. Critical values in Stock and Yogo (2002) are reported for the Cragg and Donald (1993) Wald test assuming i.i.d. errors. I follow Baum et al. (2007) and Bazzi and Clemens (2013) and use the

[27] With more than one endogenous variable, underidentification tests are based on the rank of the matrix of coefficients on excluded instruments in the first stage equation. Intuitively, if the rank of the matrix of the first stage coefficients belonging to the instruments is smaller than the number of endogenous variables then there are not enough relevant instruments to explain all endogenous variables.

[28] As such the weak instrument problem is related to underidentification. The difference is that the null hypothesis for the underidentification test aims for the limit where instruments are not weak any more but have no explanatory power over the endogenous variable at all. Weak instrument tests assume identification but test for the strength of instruments.

heteroscedasticity robust Wald statistic from Kleibergen and Paap (2006) instead.[29] Note that the test does not aim to detect the bias in individual coefficients. Instead the interpretation is that the maximum bias, the bias of the coefficient with the largest bias, is larger or smaller than the ratio specified in the null hypothesis. All other coefficients are equally or less biased.

Under and Weak Identification Tests for GMM Estimates. Testing instrument validity is not standard practice, nor is it straight forward in the GMM framework. The Kleibergen-Paap LM test for underidentification and the Stock and Yogo (2002) weak instrument test are developed for the 2SLS estimator and their results do not formally generalize to GMM (see Bun and Windmeijer (2010)). Bun and Windmeijer (2010) propose to manually construct the instrument matrix implied by the GMM framework, and then run 2SLS and apply underidentification and weak instrument tests. Both Bun and Windmeijer (2010), and Bazzi and Clemens (2013) provide simulation based evidence for the validity of this approach. Their results suggest that the tests applied to the lag instruments in the 2SLS estimator are able to detect weak instrumentation and underidentification in the GMM estimator as well. Since this is a heuristic, practical approach it should be interpreted with caution. However, in the absence of a formal extensions of under and weak IV tests to AB and BB GMM estimators, the results give valuable guidance.

4 Results

4.1 Political Institutions

This section presents the estimates for equation (1) where the dependent variable is political institutions measured by constraints on the executive. Tables 1 and 2 show results for the full sample and for a sample with initially low executive constraints. Here, lfow is defined as a constraints on the executive index below the median of the full sample. Columns 1 and 2 report results obtained from OLS and FE estimators as reference points.[30] Columns 3 and 4 show results computed using the standard (uncollapsed instrument matrix) AB and BB estimators with a maximum of two lag instruments for the AB estimator. The preferred estimator is AB with collapsed instrument matrix, of which the results are reported in column 5 of tables 1 and 2. I find that the effects on political institutions of economic institutions, political

[29]For comparison I also report results from the Cragg-Donald Wald test since the extension to the heteroscedastic case has not been formally proven.

[30]The sample size of AB and BB differ. The OLS and FE results reported are for the sample used for AB estimation.

instability, and the interaction are all significant in both samples.

The estimated coefficient are large in the sample of countries with initially low executive constraints (table 2 column 5). The major results for this sample are as follows. First, the coefficient of property rights is positive and quantitatively large. This confirms one side of the mutually reinforcing relation of economic and political institutions. Second, political instability (as measured by conflict experience) has a positive, albeit smaller coefficient. For the full sample, the estimated coefficients (table 1 column 5) have the same signs but are too small to be economically significant. This implies, that potential gains are much higher for investment in institutions when the executive is initially not very constrained.[31]

In addition to the main results I find a positive effect of income on political institutions, which is also much stronger in the low executive constraints sample. The coefficient on income in table 5 column 5 implies that a 1% increase in GDP per capita strengthens political institutions by 0.06 index points on a scale from 1 to 7.[32] Education has a very small positive effect in the full sample but the effect is insignificant in the low constraints sample. Furthermore, the persistence of political institutions is moderate and slightly larger in the full sample. This is not surprising and indicates that there is convergence of institutional quality. Because of the empirical support and the theoretical prediction of strong effects in the sample of countries with initially low executive constraints, the reminder of this section will discuss results for the preferred specification in this subsample reported in table 2 column 5.

Instrumentation for the AB estimator in column 5 is valid and strong. The Kleibergen-Paap LM test rejects underidentification. The Stock and Yogo (2002) test based on the Kleibergen-Paap Wald statistic rejects, at the 1% level, that the relative bias is larger than 30%. Moreover, the p-value of the Hansen test suggests that the order condition cannot be rejected, i.e., instruments are exogenous. However, the BB estimates in column 6 are invalid since underidentification cannot be rejected for the additional levels equations.[33] This indicates that BB does not (always) improve on AB, in line with the findings of Bun and Windmeijer (2010), and Bazzi and Clemens (2013).[34] Note that the same holds for validity of instruments

[31]The next section shows that this results carries over to the effects of political on economic institutions

[32]This is a very small effect as becomes clear in the discussion below.

[33]The underidentification and weak instrument tests reported in panel B testing the difference equations refer to the instruments used in the AB estimator. Those for the levels equations refer only to the additional instruments used for the BB estimator.

[34]In other applications the opposite might hold. See Hauk and Wacziarg (2009), and Bond et al. (2001).

Table 1: Political Institutions - Estimates for the Full Sample

			Standard IV Matrix		Collapsed IV Matrix	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A Estimator	OLS	FE	AB	BB	AB	BB
Political Inst.$_{t-1}$	0.681***	0.342***	0.445***	0.572***	0.549***	0.679***
	(0.0434)	(0.0713)	(0.0881)	(0.0707)	(0.0945)	(0.0894)
Economic Inst.$_{t-1}$	-0.0729	0.0738	0.207	0.0827	0.280*	0.0750
	(0.0808)	(0.128)	(0.170)	(0.124)	(0.154)	(0.156)
Conflict Exp.$_{t-1}$	0.00396	0.0293	0.0454***	0.0297	0.0692***	0.0555**
	(0.0201)	(0.0225)	(0.0167)	(0.0183)	(0.0268)	(0.0283)
(EI*Conflict Exp.)$_{t-1}$	0.00179	-0.0530*	-0.100***	-0.0319	-0.176**	-0.0814
	(0.0343)	(0.0313)	(0.0314)	(0.0327)	(0.0743)	(0.0557)
log GDP p.c.$_{t-1}$	0.0378***	0.133***	0.121*	0.0662***	0.129**	0.0502*
	(0.0133)	(0.0503)	(0.0665)	(0.0179)	(0.0624)	(0.0260)
Education$_{t-1}$	0.0008	0.0042**	0.0044*	0.0045***	0.0067**	0.0029
	(0.0006)	(0.0016)	(0.0023)	(0.0017)	(0.0030)	(0.0020)
Oil Dummy	-0.118***			-0.121***		-0.0944**
	(0.0346)			(0.0380)		(0.0403)
Constant	-0.0341	-0.793*		-0.385***		-0.282
	(0.0753)	(0.418)		(0.129)		(0.193)
Overidentification Tests:						
Hansen (p-value)			0.341	0.840	0.783	0.281
Sargan (p-value)			0.004	0.000	0.857	0.187
Ar1 (p-value)			0.006	0.004	0.001	0.002
Ar2 (p-value)			0.893	0.752	0.742	0.678
Observations (N*T)	534	534	534	648	534	648
Groups (N)		108	108	109	108	109
No. Instruments			80	128	12	20
Panel B Instruments tested for ... equations			Difference	Level	Difference	Level
Underidentification: Kleibergen-Paap LM Test (p-value)			0.0093	0.0383	0.0000	0.1580
Weak IV Test: rel. OLS bias >30%:						
Kleibergen-Paap Wald stat. (p-value)			0.0000	0.9999	0.0015	0.9821
Cragg-Donald Wald stat. (p-value)			0.8599	0.9999	0.0001	0.9876

Notes: All data is at country level and observed in five-year intervals, thus the first lag $(t-1)$ e.g., for 1975 is 1970. All GMM estimates are two-step. Up to two lags are used as instruments for predetermined and endogenous variables. Standard errors are robust to heteroscedasticity and, for the GMM estimates, include the Windmeijer (2005) correction. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The null hypothesis for the overidentification tests is H_0: *All instruments are exogenous in the structural equation.* Failure to reject indicated validity of instruments. The null for the underidentification test is H_0: *The first stage equations for endogenous variables are jointly underidentified,* and that for the weak instrument tests is H_0: *At least one parameter is only weakly identified.* Both tests suggest validity of instruments if the H_0 is rejected. Ar1 and Ar2 test for the H_0 that there is no first and second order autocorrelation in the differenced residuals.

Panel B columns 3 and 5 test joint validity of all instruments used by the AB estimator. Columns 4 and 6 test not the entire set of instruments used by the BB estimator but only the additional instruments implied by the moment conditions for levels equations in equations (6) and (7). Hence columns 4 and 6 indicate whether the BB estimator strengthens instrumentation compared to the AB estimator.

Table 2: Political Institutions -
Estimates for the Low Executive Constraints Sample

			Standard IV Matrix		Collapsed IV Matrix	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A Estimator	OLS	FE	AB	BB	AB	BB
Political Inst.$_{t-1}$	0.568***	0.438***	0.236	0.289**	0.455**	0.485
	(0.0754)	(0.153)	(0.156)	(0.130)	(0.182)	(0.296)
Economic Inst.$_{t-1}$	-0.247	0.110	0.825***	0.378**	1.376***	0.473
	(0.150)	(0.235)	(0.297)	(0.185)	(0.432)	(0.475)
Conflict Exp.$_{t-1}$	0.0251	0.0518	0.125***	0.0812***	0.245***	0.159**
	(0.0323)	(0.0368)	(0.0411)	(0.0275)	(0.0676)	(0.0752)
(EI*Conflict Exp.)$_{t-1}$	-0.0433	-0.0646	-0.196***	-0.136**	-0.381***	-0.262**
	(0.0651)	(0.0531)	(0.0544)	(0.0583)	(0.112)	(0.120)
log GDP p.c.$_{t-1}$	0.0389**	0.0999	0.388***	0.0635***	0.939***	0.122
	(0.0189)	(0.0753)	(0.136)	(0.0222)	(0.338)	(0.103)
Education$_{t-1}$	0.0012	0.0061*	0.0156**	0.0069***	0.0010	0.0130
	(0.0016)	(0.0032)	(0.0068)	(0.0021)	(0.0122)	(0.0082)
Oil Dummy	-0.169***			-0.191***		-0.188
	(0.0487)			(0.0534)		(0.123)
Constant	0.0648	-0.672		-0.457***		-1.139
	(0.121)	(0.568)		(0.168)		(0.858)
Overidentification Tests:						
Hansen (p-value)			0.809	1.000	0.292	0.009
Sargan (p-value)			0.000	0.000	0.406	0.000
Ar1 (p-value)			0.296	0.285	0.437	0.174
Ar2 (p-value)			0.768	0.140	0.347	0.162
Observations (N*T)	234	234	234	308	234	308
Groups (N)		67	67	74	67	74
No. Instruments			80	128	12	20
Panel B Instruments tested for ... equations			Difference	Level	Difference	Level
Underidentification: Kleibergen-Paap LM Test (p-value)			0.0903	0.345	0.0000	0.256
Weak IV Test: rel. OLS bias >30%:						
Kleibergen-Paap Wald stat. (p-value)			0.0000	0.0000	0.0007	0.9931
Cragg-Donald Wald stat. (p-value)			0.9998	0.9999	0.0503	0.9938

Notes: All data is at country level and observed in five-year intervals, thus the first lag $(t-1)$ e.g., for 1975 is 1970. The sample consists of countries that initially (in period $(t-1)$) have below median constraints on the executive. All GMM estimates are two-step. Up to two lags are used as instruments for predetermined and endogenous variables. Standard errors are robust to heteroscedasticity and, for the GMM estimates, include the Windmeijer (2005) correction. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The null hypothesis for the overidentification tests is H_0: *All instruments are exogenous in the structural equation.* Failure to reject indicated validity of instruments. The null for the underidentification test is H_0: *The first stage equations for endogenous variables are jointly underidentified,* and that for the weak instrument tests is H_0: *At least one parameter is only weakly identified.* Both tests suggest validity of instruments if the H_0 is rejected. See table 1 for further comments.

in the AB and BB estimator with standard uncollapsed IV matrix in columns 3 and 4. However, a remaining concern with these estimators is that the number of instruments is large relative to the number of observations, especially in the smaller sample of countries with initial executive constraints below the median in table 2, which can bias the results (see Roodman (2009b)). Collapsing the instrument matrix drastically reduces the number of instruments which alleviates this concern and suggests AB with collapsed instrument matrix as the preferred estimator.

Having established the validity of my empirical strategy, I now discuss the findings in table 2 column 5 in more detail. In addition to the conditional effects of economic institutions and political instability I find a significant negative interaction effect of these two variables. The presence of this interaction term implies that the coefficients on the constitutive terms - economic institutions and political instability - cannot be interpreted as unconditional marginal effects in the standard way (although the latter remain positive - and large in the case of property rights protection as discussed below). Instead they are interpreted as the effect of property rights when conflict experience is zero, and vice versa as the effect of conflict experience when property rights protection is zero (see Brambor et al. (2006)). Marginal effects can be calculated as the combined effects of the constitutive and the interaction term and evaluated at a point of the constitutive variable's distribution. Table 3 panel A presents the total, or unconditional, marginal effects, effects of a one standard deviation change, and long run effects evaluated at the mean and the median.

Since the median of the conflict measure is zero, the marginal effect of property rights protection in row one of column 2 is simply the coefficient on property rights (= 1.38). An increase in the quality of property rights protection by one standard deviation (= 0.16) increases the executive constraints by 0.82 standard deviations. To understand how important that effect is, note that translated into the not normalized index of constraints on the executive, 0.82 standard deviations imply a change of 1.32 on a scale from 1 to 7.[35] In the long term, a permanent one standard deviation improvement of property rights protection would strengthen constraints on the executive by 1.48 standard deviations (or 2.4 on the not normalized scale).[36] A relatively small improvement in property rights institutions can therefore lead to a tightening of executive constraints in the long term, that corresponds to the difference between, for instance, Senegal or Ukraine and Switzerland or the United

[35]Recall that the indices of property rights and constraints on the executive are normalized to the interval $(0, 1)$. However, the original executive constraints index reaches from 1 to 7 (and that of property rights protection from 0 to 10) in discrete steps of 1. Thus interpretation is more direct in the not normalized index and meaningful for changes of at least a full step (i.e., 1).

[36]Evaluated at the mean the effects are only slightly smaller.

Kingdom in 2010. The large effect implies that establishing property rights can initiate substantial political reform. This finding confirms the prediction in section 2.1, and supports theories that suggest that economic institutions influence political institutions (Acemoglu et al. (2005a), Acemoglu and Robinson (2006)).

Table 3: Political Institutions -
Marginal Effects for Estimates in Table 2 Column 5

	(1)	(2)	(3)	(4)
Panel A				
Effect of	Property Rights		Political Instability	
Evaluated at the:	Mean	Median	Mean	Median
Short term effects:				
Marginal	1.22***	1.38***	0.05**	0.05**
	(0.4071)	(0.4322)	(0.0247)	(0.0247)
One SD Change	0.19	0.22	0.07	0.07
Implied long run effects:				
Marginal	2.24	2.53		
One SD Change	0.35	0.40		
Panel B				
Variable	Political Inst.	Economic Inst.	Conflict Exp.	
Mean	0.46	0.50	0.41	
Median	0.33	0.50	0	
Standard deviation	0.27	0.16	1.37	

Notes: Robust standard errors in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Long run effects are calculated only for a permanent change in economic institutions. Political instability is measured as conflict experience over a moving window of 15 years, and older conflict is "forgotten." Consequently a permanent change of political instability with long run effects is not possible.

The coefficient on conflict experience indicates that one additional year of conflict in the last 15 years, will lead to a tightening of constraints on the executive (normalized score) of 0.25 when property rights are zero. The unconditional marginal effects at the median and mean, reported in table 3, panel A, columns 3 and 4, are much smaller. A one standard deviation change in conflict experience (equivalent to 1.37 years of conflict experience) leads to a change of 0.26 standard deviations in the executive constraints index. For the not normalized index of executive constraints to react with a meaningful change of 1, a country would have to experience conflict in 3 additional years in its recent past (i.e., the last 15 years). Since, I measure conflicts with at least 1000 battle deaths, this means that only experience of severe conflict increases the perceived political instability enough to pressure the government into concession in terms of constraints on the executive. The effect is consistent with the theory that change of political institutions is induced by the threat to an incumbent's political power, which forces the government to make political concessions

(Acemoglu and Robinson (2006), Besley and Persson (2011b)).[37]

Finally, the following paragraph discusses the negative coefficient on the interaction of property rights and conflict experience. The interpretation is that the positive effect of political instability on political institutions is weaker if property rights are high. This indicates that property rights can, to a certain extent, substitute for political institutions. Political institutions as measured by constraints on the executive protect citizens against the despotic behavior of the government by placing it under checks and balances. If an independent judiciary and police protect property rights, then this constitutes an indirect limit on despotism. It is indirect in the sense that it is guaranteed by an entity that does not directly control decisions of the executive. When economic institutions substitute for political institutions, the latter are less decisive for economic outcomes, and thus, one would expect, less influenced by political instability (as both incumbent and opposition stand less to profit or lose from lower executive constraints). However, independence is fragile as the executive has influence (through its formal decision making power) over judiciary and police. Thus substitution can only work to the extent that property rights are independently guaranteed.

The positive unconditional effect of political instability indicates that independence is indeed limited and substitution far from perfect - the positive effect of political instability on constraints on the executive dominates the negative interaction effect. Importantly, the conclusion that the economic and political institutions are complementary investments remains unaltered.

4.2 Economic Institutions

This section turns to the estimates of equation (2) with economic institutions, measured by property right protection, as the dependent variable. The analysis in tables 4 and 5 follows that of tables 1 and 2. As before, my preferred estimator is AB with collapsed instrument matrix in column 5 of tables 4 and 5.[38]

Most importantly the paper finds a positive but quantitatively moderate coefficient for political institutions for countries with initially low executive constraints (table 5 column 5). Together with the results from the previous section, this indicates that the relation of political and economic institutions is indeed mutually reinforcing. This is confirmed when looking at effects of institution, rather than coefficients in tables 3 and 6. In contrast, in the full sample (table 4 column 5)

[37] Besley et al. (2013) provide further empirical support, showing that positive shocks to political instability increase executive constraints in countries where those are initially low.

[38] Again the number of lags used as instruments for the AB estimator is limited to two.

the coefficient of political institutions is insignificant. The finding that the relation is driven by countries with low executive constraints is in line with the theoretical prediction, and emphasizes the finding in the previous section, where this is also the case for the inverse effect from economic to political institutions.

The effects of control variables, income and education, on economic institutions are very small and statistically insignificant in the low executive constraints sample. In addition the signs of the respective coefficients are negative. However, in the full sample (table 4 column 5) income has a statistically significant coefficient. The effect is positive but so small that it is economically insignificant. The small coefficient on lagged economic institutions indicates that these converge relatively quickly. Overall, the effects of institutions and political instability are mostly present in the sample of countries with low initial constraints on the executive as predicted. Therefore, in the remainder of this section, I discuss the results for this sample and the preferred specification in table 5 column 5.

The tests for validity and strength of instruments indicates that identification is achieved by the AB estimator in columns 3 and 5, but not the BB estimator in columns 4 and 6. An exception to validity of the AB estimator is, that for the low executive constraints sample the AB estimator with standard (uncollapsed) instrument matrix suffers from too many (weak) instruments (table 5 column 3). Collapsing the instrument matrix, however, reduced the instrument count, and the p-value of the Stock and Yogo (2002) test based on the Kleibergen-Paap Wald statistic in table 5 column 5 indicates that we can reject, that the relative OLS bias is larger than 30%, at the 5% significance level. This demonstrates how collapsing the instrument matrix contributes, through the reduction of instruments, to the reduction of bias and the achievement of strong identification. Consequently, the AB estimator with collapsed instrument matrix in column 5 is to be preferred.

In addition to the effect of political on economic institutions I find the following: First, there is a negative and significant conditional effect of political instability on political institutions. Second, the interaction of political institutions and instability has a positive but insignificant coefficient. The positive sign is intuitive. If the future government is more constrained, losing office will less affect the ability of elites to protect economic interests, and investment in property rights institutions should still be high.[39] The negative conditional effect of political instability is also intuitive as a higher risk of losing power will reduce incentives of the state to invest in

[39]In the full sample both the interaction of political institutions and instability, and political instability by itself have significant coefficients. This observation points to a role of conflict for all levels of institutions. Sign and size of the coefficients are similar in both samples, however, so that the discussion of political instability in the main text applies.

Table 4: Economic Institutions - Estimates for the Full Sample

			Standard IV Matrix		Collapsed IV Matrix	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A Estimator	OLS	FE	AB	BB	AB	BB
Economic Inst.$_{t-1}$	0.666***	0.270***	0.081	0.449***	0.231**	0.315***
	(0.0326)	(0.0514)	(0.0575)	(0.0598)	(0.111)	(0.101)
Political Inst.	-0.00745	0.0207	0.0552	0.0229	0.0701	0.0304
	(0.0178)	(0.0284)	(0.0479)	(0.0274)	(0.0692)	(0.0417)
Conflict Exp.	0.00216	-0.00579	-0.0201	-0.0166	-0.0292**	-0.0407***
	(0.00987)	(0.0102)	(0.0130)	(0.0110)	(0.0116)	(0.0133)
PI*Conflict Exp.	-0.00299	0.00636	0.0196	0.0133	0.0366**	0.0420***
	(0.0128)	(0.0133)	(0.0145)	(0.0139)	(0.0157)	(0.0159)
log GDP p.c.	0.0379***	0.0592***	0.1393***	0.00555***	0.0846*	0.0647***
	(0.00555)	(0.0205)	(0.0366)	(0.0105)	(0.0440)	(0.0194)
Education	-0.0001	-0.0012	-0.0008	-0.0011	-0.0006	-0.0007
	(0.0003)	(0.0009)	(0.0019)	(0.0009)	(0.0029)	(0.0022)
Oil Dummy	-0.0630***			-0.0794***		-0.0775**
	(0.0154)			(0.0201)		(0.0324)
Constant	-0.109***	-0.0757		-0.1464*		-0.166
	(0.0367)	(0.176)		(0.0784)		(0.163)
Overidentification Tests:						
Hansen (p-value)			0.119	0.827	0.0002	0.0019
Sargan (p-value)			0.0000	0.0000	0.0000	0.0000
Ar1 (p-value)			0.002	0.0000	0.0006	0.0001
Ar2 (p-value)			0.803	0.174	0.380	0.256
Observations (N*T)	551	551	551	666	551	666
Groups (N)		109	109	109	109	109
No. Instruments			80	128	12	20
Panel B Instruments tested for ... equations			Difference	Level	Difference	Level
Underidentification: Kleibergen-Paap LM Test (p-value)			0.0013	0.0062	0.0000	0.678
Weak IV Test: rel. OLS bias >30%:						
Kleibergen-Paap Wald stat. (p-value)			0.0000	0.9611	0.0000	0.9999
Cragg-Donald Wald stat. (p-value)			0.9732	0.9999	0.0001	0.9998

Notes: All data is at country level and observed in five-year intervals, thus the first lag $(t-1)$ e.g., for 1975 is 1970. All GMM estimates are two-step. Up to two lags are used as instruments for predetermined and endogenous variables. Standard errors are robust to heteroscedasticity and, for the GMM estimates, include the Windmeijer (2005) correction. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. See table 1 and table 2 for general comments on the reported tests.

Table 5: Economic Institutions -
Estimates for the Low Executive Constraints Sample

			Standard IV Matrix		Collapsed IV Matrix	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A Estimator	OLS	FE	AB	BB	AB	BB
Economic Inst.$_{t-1}$	0.615***	0.222**	0.0791	0.491***	0.255***	0.533***
	(0.0483)	(0.0873)	(0.0636)	(0.0659)	(0.0978)	(0.120)
Political Inst.	-0.0234	0.0280	0.0916**	0.0194	0.175***	0.0750
	(0.0277)	(0.0378)	(0.0520)	(0.0297)	(0.0491)	(0.0587)
Conflict Exp.	-0.00122	-0.0133	-0.0294**	-0.0138	-0.0303***	-0.0363**
	(0.0115)	(0.0105)	(0.0119)	(0.0085)	(0.0113)	(0.0150)
PI*Conflict Exp.	-0.00176	0.0146	0.0218	0.0024	0.0284	0.0297
	(0.0159)	(0.0145)	(0.0177)	(0.0097)	(0.0189)	(0.0269)
log GDP p.c.	0.0371***	0.0954***	0.0714	0.0290***	-0.00336	0.00840
	(0.00691)	(0.0280)	(0.0441)	(0.0131)	(0.0533)	(0.0299)
Education	0.0000	-0.0006	-0.0022	-0.0012	-0.0065	-0.0027
	(0.0006)	(0.0019)	(0.0025)	(0.0010)	(0.0047)	(0.0023)
Oil Dummy	-0.0734***			-0.0549*		-0.0274
	(0.0200)			(0.0284)		(0.0555)
Constant	-0.0698	-0.375		0.051		0.198
	(0.0507)	(0.228)		(0.0625)		(0.234)
Overidentification Tests:						
Hansen (p-value)			0.856	1.000	0.374	0.0250
Sargan (p-value)			0.0000	0.0000	0.282	0.0006
Ar1 (p-value)			0.003	0.0000	0.0006	0.0001
Ar2 (p-value)			0.856	0.519	0.426	0.407
Observations (N*T)	245	245	245	310	245	310
Groups (N)		67	67	75	67	75
No. Instruments			80	128	12	20
Panel B Instruments tested for ... equations			Difference	Level	Difference	Level
Underidentification: Kleibergen-Paap LM Test (p-value)			0.0277	0.149	0.0000	0.567
Weak IV Test: rel. OLS bias >30%:						
Kleibergen-Paap Wald stat. (p-value)			1.0000	0.0157	0.0348	0.9997
Cragg-Donald Wald stat. (p-value)			0.9999	0.9999	0.1814	0.9834

Notes: All data is at country level and observed in five-year intervals, thus the first lag $(t-1)$ e.g., for 1975 is 1970. The sample consists of countries that initially (in period $(t-1)$) have below median constraints on the executive. All GMM estimates are two-step. Up to two lags are used as instruments for predetermined and endogenous variables. Standard errors are robust to heteroscedasticity and, for the GMM estimates, include the Windmeijer (2005) correction. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

The null hypothesis for the overidentification tests is H_0: *All instruments are exogenous in the structural equation.* Failure to reject indicated validity of instruments. The null for the underidentification test is H_0: *The first stage equations for endogenous variables are jointly underidentified,* and that for the weak instrument tests is H_0: *At least one parameter is only weakly identified.* Both tests suggest validity of instruments if the H_0 is rejected. See table 1 for further comments.

good economic institutions. Moreover, conflict experience is likely to have a direct destructive effect on economic institutions and hinder the protection of property rights. The effect confirms the theoretical predictions (McGuire and Olson (1996), Besley and Persson (2009b), and Besley and Persson (2011b)) presented in section 2.1. The conditional effects have limited value for interpretation. The unconditional effects are, however, qualitatively very similar and will lead to the same conclusions.

Unconditional marginal effects are reported in table 6. The unconditional marginal effect of political instability (table 6 columns 3 and 4) is statistically significant at the 10% level. A one standard deviation increase in political instability, corresponding to 1.64 years with conflict occurrence, leads to a decrease in economic institutions of 0.19 standard deviation - about 0.3 points on the property rights index. Thus political instability has a negative, but quantitatively not very important effect on economic institutions.[40]

Table 6: Economic Institutions -
Marginal Effects for Estimates in Table 6 Column 3

	(1)	(2)	(3)	(4)
Panel A				
Effect of	Political Institutions		Political Instability	
Evaluated at the:	Mean	Median	Mean	Median
Short term effects:				
Marginal	0.19***	0.18***	-0.02*	-0.02**
	(0.0493)	(0.0491)	(0.0105)	(0.0100)
One SD Change	0.05	0.05	-0.03	-0.03
Implied long run effects:				
Marginal	0.26	0.24		
One SD Change	0.06	0.05		
Panel B				
Variable	Economic Inst.	Political Inst.	Conflict Exp.	
Mean	0.49	0.46	0.53	
Median	0.50	0.33	0	
Standard deviation	0.16	0.27	1.64	

Notes: Robust standard errors in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Long run effects are calculated only for a permanent change in economic institutions. Political instability is measured as conflict experience over a moving window of 15 years, and older conflict is "forgotten." Consequently a permanent change of political instability with long run effects is not possible.

The unconditional marginal effect of political institutions is of more importance. Evaluated at the median (Panel A, column 2, row 1) is simply the coefficient on political institutions reported in table 5 column 5 (as conflict experience is zero

[40]Note that for the full sample the unconditional effect of political instability is negative at the mean but positive at the median. However effects are economically insignificant in the full sample as they are smaller in absolute size by a factor of roughly 10, compared to effect in the low executive constraints sample.

at the median). It indicates that a one standard deviation change in the index of constraints on the executive leads to a change in economic institutions of 0.32 standard deviation. The effects at the mean, and the long run effects are quantitatively similar. To further illustrate the effect, it can be translated into the underlying untransformed indices. The untransformed index of constraints on the executive has to improve by more than 3 (half way of its scale from 1 to 7) for the property rights index to improve by 1 (on a 0 to 10 scale). The direction and the moderate size of this effect is consistent with existing evidence that large political reforms lead to better economic institutions in the broader sense (Persson (2005)).

To give an example, the more than 3 points on the executive constraints index, necessary to affect economic institutions, correspond to the difference between the Republic of Congo and France, or Syria and the United States, and Norway in 2010. These differences are large but reforms of that scope do happen. In my sample there are 42 reforms that strengthened constraints on the executive by more than 3 index points. For example Brazil moved 4 points on the index in the period $1980 - 85$, and Nepal moved by 5 points more recently in $2005 - 2010$. To summarize, though this effect seems small, large reforms of political institutions are not unrealistic and can lead to meaningful changes in economic institutions. What is more, these changes may then lead to a further improvement of political institutions in the future, as suggested by the results in section 4.1. This demonstrates how the two effects combine to produce the mutually reinforcing dynamic relation of political and economic institutions.

5 Robustness of Results

This section explores the robustness of the main results presented in table 2 column 5 and table 5 column 5. Thus all findings in this section derive from the sample of countries with low initial executive constraints using the AB estimator with collapsed instrument matrix.

5.1 The Timing of Institutional Determinants

The first point examined here is the timing of institutional interactions and effects of covariates. The empirical model specifies political institutions as a function of lagged values of regressors (equation (1)), while economic institutions depend on contemporaneous values (equation (2)). This is based on the theoretical suggestion

of Acemoglu et al. (2005a).[41] However, different timings are perceivable. On the one hand, economic institutions could be slow moving like political institutions and thus also depend on lags. On the other hand, Besley and Persson (2011b) suggest that institutions are simultaneously decided one period ahead. According to this theory, period t's political institutions are set taking into account expected economic institutions at time t and vice versa. This would imply that political institutions also depend on current values - if those are plausible proxies for expected values.

In table 7, columns 1 to 3, I explore different timings for equation (1). Column 1 presents results when all variables are contemporaneous, column 2 when economic institutions, political instability and their interaction are contemporaneous only, and column 3 when only control variables are contemporaneous.[42] The point estimates for economic institutions are much larger. Assuming the model in (1) and (2) is correctly specified this is not surprising. The coefficient picks up the effect of past economic institutions and the reverse causal effect of current political on economic institutions. However, the estimates are very imprecise - the coefficient is not significant in column 1 and only marginally significant in column 2. Finally, in column 3, the estimated effects of (lagged) economic institutions, political instability and their interaction are very close to those of the main results in table 2 column 5.

Having said that, identification is weak, and in column 2 the tests do not reject underidentification. Therefore these results are suggestive at best. Weak instruments can be anticipated here. The longer the time distance of the explanatory variable to the instrument, the less explanatory power can be expected from the instruments. When using contemporaneous values as regressors the first available instrument is the second lag, the observation 10 years earlier (while it was 5 years before the lagged regressors). In summary these results should be interpreted with care due to the weak instrumentation. We cannot conclude that there is no contemporaneous relationship. However, given the strongly identified effects of lagged values in table 2, the evidence supports the timing proposed by Acemoglu et al. (2005a) and the existing empirical literature.

Table 8, columns 1 to 3, reports results for changed timing in equation (2). Column 1 lags all regressors one period (5 years), column 2 lags only control variables, and column 3 only political institutions, instability and their interaction.[43] Column 1 provides strong evidence that contemporaneous, not lagged values of regressors

[41] And, concerning equation (1), the standard practice of the empirical literature.

[42] Note that the assumption about endogeneity of regressors is unchanged and thus second lags are used as instruments for contemporaneous regressors.

[43] Assumptions of predetermination or endogeneity of contemporaneous regressors in equation (2) are not changed. If regressors are lagged then lags are predetermined.

determine economic institutions. The estimated effects are all very small. With the exception of the coefficient on political institutions, which is marginally significant, none of the coefficients are statistically significant. Most importantly, the results are well identified.

Column 2 confirms the main results as the coefficients on current political institutions, instability and the interaction are very similar in size and precision to those in table 6 column 5. Interestingly, the same holds for estimated effects of lagged values of political institutions and stability in column 3. However, the coefficient of political instability changes its sign. A possible explanation is that political instability has an immediate negative effect on property rights. Political instability disincentivizes investment into property rights, and conflict directly hinders their upholding. In the long run positive effects might occur through a mechanism of political pressure, similar to the process that positively affects political institutions. The results in columns 2 and 3 do not allow for excluding the possibility of a different timing. However, identification is weak for these specifications leaving them suggestive at best. Overall the results in table 8 columns 1 through 3 support the main findings and the timing proposed in Acemoglu et al. (2005a), especially contrasting the well identified effects in table 6 column 5 with the also strong instrumented small or absent effects in table 8 column 1.

5.2 Period Fixed Effects and Additional Control Variables

A common practice in similar empirical studies is the inclusion of time effects, to control for global shocks or trends. But time effects also eliminate large parts of "good variance," which is common to countries but that we would like to keep in the data. For instance, they would absorb much of the breakdown of the Soviet Union, which is exactly the kind of internal process this paper studies. The preferred approach does not include time effects based on the conviction that they eliminate more good than bad variation. In this context it is not surprising that instrumentation becomes very weak when a set of period dummies is added to the set of regressors (table 8 column 4), and even underidentified in the case of equation (1) (table 7 column 4). The estimated coefficients indicate that the main results are not robust to the inclusion of time effects. However, due to the adverse effects of including period dummies, and weak instruments, these results are not very convincing.

The remaining columns in tables 7 and 8 introduce additional controls and an alternative measure of education. Columns 5 and 6 evaluate the role of population size, and life expectancy. Effects of economic institutions, and political instability on political institutions are precisely estimated. They retain their sign and are quan-

Table 7: Political Institutions - Robustness for Preferred Specification (Table 2 Column 5).

	All Contemp. (1)	lag GDP, Educ. (2)	lag EI, Conflict (3)	Time FE (4)	Popu-lation (5)	Life Exp. (6)	Alt. Educ. (7)
Political Inst.$_{t-1}$	0.352	0.231	0.527**	0.212	0.460**	0.503**	0.498**
	(0.378)	(0.217)	(0.210)	(0.224)	(0.218)	(0.206)	(0.231)
Economic Inst.$_{t-1}$			1.248***	0.507	1.271**	1.601***	1.761***
			(0.435)	(0.724)	(0.604)	(0.608)	(0.461)
Conflict Exp.$_{t-1}$			0.214***	0.0937	0.237***	0.230***	0.217***
			(0.0727)	(0.0822)	(0.0754)	(0.0647)	(0.0605)
(EI*Conflict Exp.)$_{t-1}$			-0.360***	-0.157	-0.375***	-0.351***	-0.368***
			(0.124)	(0.140)	(0.125)	(0.102)	(0.101)
log Population$_{t-1}$					0.115		
					(0.695)		
log Life Expectancy$_{t-1}$						-0.998	
						(1.528)	
Education (mean years)$_{t-1}$							-0.0840
							(0.0519)
log GDP p.c.$_{t-1}$		0.788***		-0.203	0.795	1.038**	1.260***
		(0.295)		(0.690)	(0.711)	(0.463)	(0.392)
Education$_{t-1}$		0.0038		0.0019	0.0034	0.0080	
		(0.0093)		(0.0132)	(0.0174)	(0.0119)	
Contemporaneous Values:							
Economic Inst.$_t$	2.144	2.320*					
	(1.553)	(1.214)					
Conflict Exp.$_t$	0.292*	0.178					
	(0.175)	(0.145)					
(EI*Conflict Exp.)$_t$	-0.546	-0.243					
	(0.451)	(0.374)					
log GDP p.c.$_t$	0.590*		0.551**				
	(0.335)		(0.242)				
Education$_t$	0.0041		0.0001				
	(0.0173)		(0.0148)				
Overidentification Tests:							
Hansen (p-value)	0.455	0.883	0.313	0.0435	0.277	0.322	0.396
Sargan (p-value)	0.420	0.716	0.326	0.103	0.343	0.445	0.265
Ar1 (p-value)	0.121	0.148	0.204	0.129	0.307	0.276	0.515
Ar2 (p-value)	0.143	0.0588	0.931	0.480	0.406	0.693	0.532
Observations (N*T)	294	293	235	234	234	234	234
Groups (N)	72	72	67	67	67	67	67
No. Instruments	12	12	12	19	14	14	12
Underidentification:							
K-P LM Test (p-value)	0.0665	0.353	0.0155	0.1576	0.0058	0.0049	0.0984
Weak IV Test: rel. OLS bias >30%:							
K-P Wald stat. (p-value)	0.9957	0.9998	0.7753	0.9947	0.9269	0.9097	0.9875

Notes: Robust, Windmeijer (2005) corrected standard errors in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All results are Arellano-Bond estimates with up to two lags as instruments and collapsed instrument matrix. The dependent variable in all specifications is political institutions measured by constraints on the executive. Columns 1-3 explore different timing of effects. Assumptions about endogeneity of regressors remain unchanged, which means that second and third lags are used for all contemporaneous values. Column 4 adds time dummies. Column 5 adds log of population density, and column 6 the log of life expectancy at birth as regressors, both are allowed to be endogenous (i.e., lags are predetermined). Column 7 replaces the main measure of education with average years of schooling in the population aged 25 and above. See tables 1 and 2 for general comments on the tests.

Table 8: Economic Institutions - Robustness for Preferred Specification (Table 5 Column 5).

	All Lags (1)	lag GDP, Educ. (2)	lag PI, Conflict (3)	Time FE (4)	Population (5)	Life Exp. (6)	Alt. Educ. (7)
Economic Inst.$_{t-1}$	0.386***	0.291***	0.375***	0.346***	0.165	0.0985	0.281***
	(0.132)	(0.0946)	(0.130)	(0.109)	(0.104)	(0.121)	(0.0996)
Political Inst.$_t$		0.182***		0.0455	0.119**	0.134**	0.0765
		(0.0473)		(0.0627)	(0.0561)	(0.0596)	(0.0562)
Conflict Exp.$_t$		-0.0299**		-0.0323**	-0.0262**	-0.0350**	-0.0282**
		(0.0117)		(0.0128)	(0.0121)	(0.0163)	(0.0112)
(PI*Conflict Exp.)$_t$		0.0243		0.0406**	0.0245	0.0437	0.0330**
		(0.0169)		(0.0177)	(0.0156)	(0.0290)	(0.0151)
log Population$_t$					0.216*		
					(0.122)		
log Life Expectancy$_t$						1.124**	
						(0.535)	
Education (mean years)$_t$							0.0213
							(0.0185)
log GDP p.c.$_t$			0.151**	-0.00818	-0.106*	-0.131	-0.102
			(0.0689)	(0.149)	(0.0580)	(0.0880)	(0.0751)
Education$_t$			-0.0057	-0.0081	-0.0104*	-0.0111**	
			(0.0042)	(0.0055)	(0.0062)	(0.0056)	
Lagged Values:							
Political Inst.$_{t-1}$	0.0921*		0.136**				
	(0.0508)		(0.0584)				
Conflict Exp.$_{t-1}$	0.0153		0.0178**				
	(0.0107)		(0.00875)				
(PI*Conflict Exp.)$_{t-1}$	0.0142		0.00860				
	(0.0198)		(0.0176)				
log GDP p.c.$_{t-1}$	0.0756	-0.0691					
	(0.0575)	(0.0702)					
Education$_{t-1}$	-0.0008	-0.0024					
	(0.0041)	(0.0041)					
Overidentification Tests:							
Hansen (p-value)	0.0245	0.329	0.128	0.926	0.244	0.266	0.294
Sargan (p-value)	0.0552	0.172	0.226	0.950	0.299	0.346	0.214
Ar1 (p-value	0.001	0.000	0.001	0.000	0.003	0.009	0.000
Ar2 (p-value)	0.710	0.306	0.996	0.194	0.425	0.455	0.316
Observations (N*T)	242	244	243	245	245	245	245
Groups (N)	67	67	67	67	67	67	67
No. Instruments	12	12	12	19	14	14	12
Underidentification:							
K-P LM Test (p-value)	0.0000	0.0001	0.0027	0.0412	0.0087	0.0061	0.0000
Weak IV Test: rel. OLS bias >30%:							
K-P Wald stat. (p-value)	0.0041	0.5554	0.5472	0.9758	0.9227	0.7079	0.2324

Notes: Robust, Windmeijer (2005) corrected standard errors in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All results are Arellano-Bond estimates with up to two lags as instruments and collapsed instrument matrix. The dependent variable in all specifications is economic institutions measured by property rights protection. Columns 1-3 explore different timing of effects. Assumptions about endogeneity of regressors remain unchanged. Column 4 adds time dummies. Column 5 adds log of population density, and column 6 the log of life expectancy at birth as regressors, both are assumed to be predetermined. Column 7 replaces the main measure of education with average years of schooling in the population aged 25 and above. See tables 1 and 2 for general comments on the tests.

titatively similar. The same holds for effects of political institutions, and instability on economic institutions. Note that the additional covariates have no significant effect on political institutions (table 7) but seem to positively affect economic institutions (table 8).[44] In both cases instruments are weak, which may be due to the increased number of endogenous/predetermined variables and instruments. This again leaves the results inconclusive. Nevertheless, in tendency, the main findings are confirmed. Finally, column 7 replaces the main measure of education by the also commonly used[45] average years of schooling for the population above age 25. Somewhat puzzling, instruments are weak for this measure of education, especially in estimates of equation (1) in table 7. The effect from political to economic institutions is not robust (table 8 column 7). However, the effects of economic institutions and political instability on political institutions (table 7 column 7), and of political instability on economic institutions (table 8 column 7) are robust.

5.3 The Political Instability Proxy

This subsection explores several issues related to the role of conflict experience as a proxy of political instability. Specifically, it analyzes a subsample of countries that never experience conflict, different windows for conflict experience, the role of small scale conflict, and effects of regulated turnover which also determines the survival probability of governments.

Almost 75% of countries in the low executive constraints sample never experience any conflict, thus challenging external validity of the main results. To explore the issue, table 9 present results for a subsample that never experiences conflict (i.e., the political stability measure is always zero) in column 1, and a subsample with at least one conflict year in the sample period in column 2. For the small sample of countries with some conflict experience in column 2, instrumentation is very weak - which is to be expected given that the number of instruments is large relative to the number of countries and observations. Columns 1 and 2 of table 10 present results for equation (2) of economic institutions that are similar in trend. Overall this confirms the main results of table 2 column 5 and table 5 column 5.

The choice of 15 years as "conflict memory," i.e., the window over which conflict experience is summed, is arbitrary. Columns 3 and 4 of table 9 and 10 show that computing the conflict experience measure over a 10 or a 20-year window, leads to

[44] Population size and life expectancy are treated as predetermined in table 8. When they are allowed to be endogenous their effects disappear - estimated coefficients are much smaller and not statistically significant. Effects of political institutions and instability are very similar (results not reported).

[45] E.g., Glaeser et al. (2004), Acemoglu et al. (2005b), and Papaioannou and Siourounis (2008b).

Table 9: Political Institutions - Robustness with Alternative Political Instability Proxies.

	Confl.=0	Confl.>0	10year Window	20year Window	Small Scale Conflict	Turnover	Turnover and Interaction
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Political Inst.$_{t-1}$	0.624**	-0.000535	0.539**	0.513**	0.521***	0.163	0.227
	(0.264)	(0.280)	(0.224)	(0.208)	(0.196)	(0.281)	(0.262)
Economic Inst.$_{t-1}$	1.184*	1.432**	1.272***	1.304***	1.363***	1.444***	-0.689
	(0.611)	(0.679)	(0.483)	(0.444)	(0.517)	(0.441)	(0.920)
Conflict Exp.$_{t-1}$ (main)		0.207**				0.211***	0.149***
		(0.0883)				(0.0613)	(0.0560)
(EI*CEx.)$_{t-1}$ (main)		-0.342**				-0.334***	-0.266***
		(0.150)				(0.100)	(0.0787)
CEx.$_{t-1}$ (10year)			0.148***				
			(0.0486)				
(EI*CEx.)$_{t-1}$ (10year)			-0.278***				
			(0.0916)				
CEx.$_{t-1}$ (20year)				0.241**			
				(0.101)			
(EI*CEx.)$_{t-1}$ (20year)				-0.386***			
				(0.142)			
Small Scale Conflict$_{t-1}$					0.130**		
					(0.0570)		
(EI*CEx.)$_{t-1}$ (small)					-0.221**		
					(0.111)		
Turnover$_{t-1}$						0.515	-0.495
						(0.422)	(0.488)
(EI*Turnover)$_{t-1}$							2.820**
							(1.234)
Overidentification Tests:							
Hansen (p-value)	0.171	0.957	0.192	0.149	0.352	0.358	0.769
Sargan (p-value)	0.187	0.969	0.352	0.178	0.321	0.331	0.586
Ar1 (p-value)	0.313	0.617	0.342	0.186	0.333	0.310	0.401
Ar2 (p-value)	0.628	0.919	0.512	0.314	0.251	0.305	0.487
Observations (N*T)	163	71	234	234	234	234	234
Groups (N)	45	22	67	67	67	67	67
No. Instruments	8	12	12	12	12	14	16
Underidentification:							
K-P LM Test (p-value)	0.0002	0.1328	0.0000	0.0264	0.0000	0.0378	0.0153
Weak IV Test: rel. OLS bias >30%:							
K-P Wald stat. (p-value)	0.0068	0.9576	0.0009	0.6870	0.0023	0.9772	0.9534

Notes: Robust, Windmeijer (2005) corrected standard errors in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All specifications control for income and education. All results are Arellano-Bond estimates with up to two lags as instruments and collapsed instrument matrix. The dependent variable in all specifications is political institutions measured by constraints on the executive. Column 1 uses a subsample of countries that never experience conflict in the sample period, column 2 a subsample of countries that experience at least some conflict. Column 3 changes the "conflict memory", i.e., the window over which conflict experience is summed to 10 years, column 4 extends the window to 20 years. Column 5 uses a conflict definition that includes small conflicts with at least 25 battle deaths per year. Column 6 adds regulated turnover (assumed to be endogenous), and column 7 adds regulated turnover and its interaction with economic institutions as explanatory variables. See tables 1 and 2 for general comments on the tests.

Table 10: Economic Institutions - Robustness with Alternative Political Instability Proxies.

	Confl.=0	Confl.>0	10year Window	20year Window	Small Scale Conflict	Turnover	Turnover and Interaction
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Economic Inst.$_{t-1}$	0.171	0.313	0.219**	0.268***	0.256**	0.244**	0.247**
	(0.112)	(0.470)	(0.103)	(0.102)	(0.107)	(0.0961)	(0.0967)
Political Inst.	0.197***	0.233	0.183***	0.166***	0.177***	0.177**	0.315*
	(0.0602)	(0.146)	(0.0479)	(0.0521)	(0.0554)	(0.0736)	(0.161)
Conflict Exp.$_t$ (main)		-0.0350				-0.0291***	-0.0251**
		(0.0224)				(0.0108)	(0.0117)
(PI*CEx.)$_t$ (main)		0.0343**				0.0249	0.0208
		(0.0170)				(0.0164)	(0.0189)
CEx.$_t$ (10year)			-0.0245**				
			(0.0095)				
(PI*CEx.)$_t$ (10year)			0.0068				
			(0.0163)				
CEx.$_t$ (20year)				-0.0240			
				(0.0150)			
(PI*CEx.)$_t$ (20year)				0.0401			
				(0.0259)			
Small Scale Conflict$_t$					-0.0079		
					(0.0112)		
(PI*CEx.)$_t$ (small)					0.0047		
					(0.0151)		
Turnover$_t$						-0.0104	0.0325
						(0.0778)	(0.0875)
(PI*Turnover)$_{t-1}$							-0.186
							(0.209)
Overidentification Tests:							
Hansen (p-value)	0.154	0.732	0.279	0.317	0.158	0.416	0.457
Sargan (p-value)	0.161	0.861	0.142	0.271	0.103	0.185	0.126
Ar1 (p-value	0.004	0.204	0.001	0.001	0.002	0.001	0.001
Ar2 (p-value)	0.492	0.736	0.469	0.386	0.414	0.472	0.665
Observations (N*T)	165	80	245	245	245	245	245
Groups (N)	44	23	67	67	67	67	67
No. Instruments	8	12	12	12	12	14	16
Underidentification:							
K-P LM Test (p-value)	0.0029	0.2878	0.0000	0.0000	0.0038	0.0000	0.0270
Weak IV Test: rel. OLS bias >30%:							
K-P Wald stat. (p-value)	0.2547	0.9970	0.0155	0.0422	0.5067	0.1325	0.9893

Notes: Robust, Windmeijer (2005) corrected standard errors in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All specifications control for income and education. All results are Arellano-Bond estimates with up to two lags as instruments and collapsed instrument matrix. The dependent variable in all specifications is economic institutions measured by property rights protection. Column 1 uses a subsample of countries that never experience conflict in the sample period, column 2 a subsample of countries that experience at least some conflict. Column 3 changes the "conflict memory", i.e., the window over which conflict experience is summed to 10 years, column 4 extends the window to 20 years. Column 5 uses a conflict definition that includes small conflicts with at least 25 battle deaths per year. Column 6 adds regulated turnover (assumed to be endogenous), and column 7 adds regulated turnover and its interaction with economic institutions as explanatory variables. See tables 1 and 2 for general comments on the tests.

very similar results for institutional interactions, in both equations (1) and (2).[46] However, using the 20-year window results in weakly identified effects on political institutions (table 9 column 4) and imprecise estimates for effects on economic institutions (table 10 column 4). Finally, column 5 uses a proxy for political instability that also counts small scale conflicts with at least 25, instead of 1000, battle deaths per year. The main results for equation (1) are confirmed (table 9 column 5). But political instability seems to have no effect on economic institutions when it includes small scale conflict (table 10 column 5). Moreover, I cannot reject weak instruments in table 10 column 5. However the effect of political on economic institutions seems fairly robust.

As noted in section 2, political instability as proxied by conflict experience is not the only mechanism that determines the survival probability of an incumbent government. Conflict experience aims to capture probability of unregulated change in government. An alternative is that regulated government turnover affects institutions. In column 6, the model includes as regressor an index that classifies de facto regulation of the *Turnover* of governments (see data appendix for details on the turnover measure). Effects of economic institutions, conflict experience and their interaction on political institutions in table 9 are robust. The same holds for effects of political institutions and instability on economic institutions in table 10 column 6. Moreover, turnover has no significant effects on institutions. The results are encouraging. However, they remain indicative due weak instrumentats. Weak instruments can be expected here, since additional predetermined variables make identification more challenging, and increase the instrument count.

Column 7 further adds an interaction of turnover and institutions, parallel to the interaction of political instability and institutions. This leaves the results for equation (2) unchanged. However, in equation (1), the effect of economic on political institutions is now insignificant - even turns negative, while the effects of political instability remain precisely estimated and retain the same sign. On the other hand, the interaction of turnover and economic institutions is now statistically significant. It is positive and very large indicating that there is an interesting mechanism at work here. However the estimates suffer from weak instruments. Therefore, there is no strong evidence for the effect of turnover on institutions, indicating that my main findings are the robustness.

[46] In estimates not reported I also explore the issue of whether current conflict should be included in the sum. As the conflict data are available on a yearly basis, the sums for 1, 2, and 3-year lags are used instead, with virtually no difference in coefficient estimates, significance, or instrument validity.

6 Conclusions

The first and major result of this paper is the empirical support for the reinforcing relation between economic and political institutions. The estimated effect of property rights on constraints on the executive is statistically significant, positive and large. The reverse effect is also positive and statistically significant but smaller. Only reforms that lead to large increases in constraints on the executive have a sizable effect on property rights. With respect to timing, the paper finds that the small effect of political on economic institutions is contemporaneous. In contrast, the large effect of economic on political institutions occurs with a time lag (5 years).

These results provide valuable guidance to future reformers. Specifically, they suggest that overall institutional development can be strongly affected by the reform towards better and equal property rights protection. Furthermore, larger gains can be achieved by institutional reform in countries with initially low constraints on the executive. A next step will be to further analyze the feedback process by estimating equations (1) and (2) as a system (in addition to the equation by equation estimation presented above).

A second result is that political instability affects both types of institutions. It has a clear positive effect on political institutions, but a small negative effect on economic institutions. Political instability is defined as the perceived probability of an incumbent government losing office by unregulated, mostly violent means, and is measured by conflict experience.[47] In addition, political instability interacts with economic institutions to affect political institutions negatively, indicating a limited substitutability of economic and political institutions. But as the overall effect remains positive, the conclusion that political and economic institutions are complementary investments remains unaltered.

Concerning political instability the findings presented in the robustness section of the paper hint towards two interesting relations: (i) the timing of effects of political instability on economic institutions, and (ii) the dependence of effect of economic on political institutions on turnover. Both relations could inspire future research.

On the technical side I find that the problem of too many and weak instruments, associated with the Arellano-Bond estimator, can be alleviated - at least in this application. Restricting the number of lag instruments, and, in addition, collapsing the instrument matrix diminishes the instrument proliferation problem and achieves strong identification. The practical empirical application in this paper underlines the importance of careful evaluation and testing of such issues.

[47] This is not the effect of conflict or violence itself. See section 2 for a discussion of the political instability measure.

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A Data Description

All data are observed in five-year intervals in the period $1950 - 2010$. The panel is unbalanced and some data series start at a later date than 1950. Note that the main data set starts at 1975 due to the availability of the measure for economic institutions. However, since my estimation strategy relies on lag instruments, data from periods earlier than 1975 is utilized as instruments if available.

Political Institutions. The measure is *Constraints on the Executive* from the Polity IV dataset (Marshall et al. (2013)), $1950 - 2010$. This is a classification of "institutionalized constraints on the decision making powers of chief executives" into seven categories. These reach from the lowest category "Unlimited Authority" with no such rules to the highest constraint category "Executive Parity of Subordinates". The seven Categories are: (1) "Unlimited Authority", (2) "Intermediate Category", (3) "Slight to Moderate Limitation [...]", (4) "Intermediate Category", (5) "Substantial Limitations [...]", (6) "Intermediate Category", and (7) "Executive Parity of Subordinates". Country years classified as "interruption", "interregnum", and "transition" are treated as missing values.

The original index quantifies the categories in an ordered variable that ranges from 1 to 7, where 1 denotes the lowest and 7 the highest constraints. For all empirical work in this paper, the index is normalized to the (0,1) interval and treated as a continuous measure. To extend the time series for certain countries the following changes are made. Germany after reunification in 1990 is treated as a continuation of West Germany (FRG). The Czech Republic and the Slovak Republic are treated as continuation of Czechoslovakia. The Russian Federation is treated as continuation of the UDSSR.

Economic Institutions. Economic institutions are measured by the *Property Rights* index provided by the liberal Canadian think tank Fraser Institute, and its international collaboration of liberal think tanks Economic Freedom Network, in Economic Freedom of the World (Gwartney et al. (2012)), $1970 - 2010$. The measure for property rights used in this paper is the Area 2 combined index of "Legal System and Property Rights". The index summarizes the following subcategories stemming from different sources (World Economic Forum Global Competitiveness Report, PRS Group International Country Risk Guide, World Bank Doing Business): (A)"Judicial Independence", (B) "Impartial courts," (C) "Protection of property rights," (D) "Military interference in rule of law and politics," (E) Integrity of the legal system," (F) "Legal enforcement of contracts," (G) "Regulatory restrictions on the sale of real property," (H) "Reliability of police", and (I)"Business costs of crime." The original index ranges from 0 to 10 on a continuous scale, where 10

indicates the best protection of property rights. For all empirical work in this paper the index is normalized to the (0,1) interval and treated as a continuous measure.

Political Instability. $1950-2010$. The proxy for *Political Instability* is based on the UCDP/PRIO Armed Conflict Dataset (Gleditsch et al. (2002), Harbom and Wallensteen (2012)). Specifically, I make use of the Onset Dataset's dummy variable, which is equal to one for each country year of internal conflict over government (i.e. where conflict is over government, or over government and territory) with at least 1000 (alternatively 25) battle deaths. The political instability measure is constructed as a country's conflict experience over a window of J years. Conflict experience Γ at time t for country i is the sum of the conflict dummy, denote it CD, over that window $\Gamma_{i,t} = \sum_{j=0}^{J-1} CD_{i,t-j}$. The main measure sets $J = 15$ and focuses on a strict measure of civil war with at least 1000 battle deaths per country year. In the robustness test section I explore alternative J and the effect of smaller scale conflict with at least 25 battle death.

Turnover. This is a proxy for alternative measures that determine political survival available for the period $1950-2010$. The variable combines two Polity IV indices, one of "Competitiveness of Recruitment", and the other of "Openness of Recruitment" of the executive branch of government. As such it classifies *de facto* rules (rules in practice) that determine regulated turnover of government. The measure is the unweighted sum of both indexes normalized to the (0,1) interval.

Control Variables. (1) Income is measured by purchasing power parity adjusted GDP per capita (*lrgdpch*) from the Penn World Tables 7.1 (Heston et al. (2012)) $1950-2010$. (2) The main measure for education is the percentage of the population above 15 years of age that has primary education, and for robustness checks average years of schooling in the population aged 25 and above is used. Both measures from Barro and Lee (2013) $1950-2010$. (3) To control for resource abundance I use a dummy for oil producing countries from Papaioannou and Siourounis (2008a) $1950-2010$.

Control Variables for Robustness Section. (1) Log of population size from the Penn World Tables 7.1. (2) Log of life expectancy at birth from the World Bank's World Development Indicators (World Bank (2012)).

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