



Rebundling Institutions: How property rights and contracting institutions combine for growth

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ARTICLE INFO

JEL classification:

C23
H13
O11
O17
O43
P48

Keywords:

Legal institutions
Property rights
Contracting
Interrelated incentives
Joint effects
Economic development

ABSTRACT

This study provides empirical evidence for the joint workings of property rights and contracting institutions as fundamental causes of growth. In a two-step panel estimation procedure that uses data from 130 countries over the period 2005–2015, I find that the income effects of legal reforms in property rights institutions vary with the prevalent quality of legal contracting institutions and vice versa. Decomposing the interaction effects for groups of countries with different quality combinations reveals that the quality fit of the two types of institutions matters for not only the size but also the direction of the interaction effects. In countries with absent or bad legal institutions, legal reforms considering only one type can even reduce income. Piecemeal reforms work best when they close quality gaps and they can backfire when the gap widens. The findings remain robust after tackling endogeneity issues, extending the period of analysis, estimating alternative models, and using alternative estimators. The findings imply that legal reforms have to be coordinated across different types of institutions and consider possible interferences with extra-legal rules and practices.

1. Introduction

The state plays a crucial role in the building of institutions that North (1991):97 defines as “*humanly devised constraints that structure political, economic and social interaction*”. It has the authority to issue and enforce laws and regulations that put incentives and constraints on a wide area of human interaction. Acemoglu and Johnson (2005) open *Unbundling Institutions* with a reference to North (1981), pointing at two spheres of state regulation that ascribe the state a different role for organizing national economies: The first, the “predatory theory” of the state, emphasizes the state’s role in distributing political power and allocating resources in the society, underlining the importance of property rights institutions for economic development (Jones, 2003; De Long and Shleifer, 1993; Olson, 2000; Besley and Ghatak, 2010). The second, the “contract theory” of the state, emphasizes the state’s role in providing a legal framework that enables private contracts and facilitates economic transactions (Coase, 1960; Williamson, 1989), underlining the importance of contracting institutions for economic development (Grossman and Hart, 1986; Hart and Moore, 1990; Hart, 1995).

While Acemoglu and Johnson (2005):950 acknowledge that the state is responsible for providing a legal framework that regulates both the distribution of power and resources and the enforcement of private contracts, they “*attempt to unbundle the broad cluster of institutions and learn more about the relative importance of contracting versus property rights institutions at the macro level*”. In a cross-country study on former European colonies, Acemoglu and Johnson (2005) find strong and significant effects of legal property rights institutions and much weaker and for non-financial outcomes non-significant effects of legal contracting institutions. They conclude that individuals may find informal ways to avoid the adverse effects of a legal system that poorly enforces private contracts but

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<https://doi.org/10.1016/j.jce.2023.02.001>

Received 24 June 2022; Received in revised form 7 December 2022; Accepted 1 February 2023

Available online 1 March 2023

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find it harder to mitigate the risk of government expropriation. *Unbundling Institutions* was a starting point for a number of studies aiming to isolate and compare the economic effects of different types of institutions (Fernandez and Kraay, 2005; Bhattacharyya, 2009; Williamson and Kerekes, 2011; Asongu, 2016).

This study questions the assumption of independent effects and brings the investigation on how property rights and contracting institutions affect economic development back to North (1981), Milgrom and Roberts (1994), Ogilvie and Carus (2014), and towards the Varieties of Capitalism literature (e.g., Soskice and Hall, 2001; Amable, 2003; Rougier, 2015). This stream of literature suggests that it is not single (sets of) institutions but bundles of (sets of) institutions that together and in their combination organize production, exchange, and income distribution. Voigt and Gutmann (2013) have taken one step towards rebundling the effects of legal property rights institutions. They argue that precisely defined property rights are unlikely to have any income effects unless accompanied by some credible commitment of the government to enforce the rights. Voigt and Gutmann (2013) provide empirical evidence that property rights increase growth rates only if the judicial system is independent enough to guarantee their enforcement. This paper goes further and provides arguments and empirical evidence that implementing or improving the legal enforcement of property rights does not suffice to spur economic development and can be ineffective – even countereffective – when a legal enforcement of private contracts is absent or of bad quality. This is because property rights and contracting institutions provide interrelated incentives and constraints on economic decisions and productive activities. A lack in the specification and enforcement of only one type of institutions as well as a poor fit of the incentives and constraints they provide inhibit private investment and therefore economic development.

I draw on data from 130 countries and implement a two-step panel estimation procedure to test the hypothesis that property rights and contracting institutions are jointly effective. The two-step estimation strategy builds on growth theory and allows different channels and timespans of effects. In the first step, I use a fixed effects (FE) least squares estimator to identify short-term individual and interaction effects of legal property rights and contracting institutions on real GDP per capita levels whilst controlling for country-specific unobserved heterogeneity, time, and a set of control variables. In the second step, I use a between effects (BE) least squares estimator to identify individual and interaction effects of legal property rights and contracting institutions on countries' long-term income levels. I use the estimate of the country-specific unobserved heterogeneity term obtained in the first-step regression as a proxy for countries' long-term income levels. I follow Acemoglu and Johnson (2005) in the choice of measures of property rights and contracting institutions. I use Polity IV's variable *Executive Constraints* as baseline measure of legal property rights institutions and the World Bank's indicator *Enforcing Contracts* as baseline measure of legal contracting institutions.

The baseline estimation results and a number of robustness tests support the hypothesis of joint effects. I find positive individual and interaction effects of legal property rights and contracting institutions on countries' long-term income levels, all at considerable sizes and statistically highly significant. The marginal effects of increases in executive constraints on income levels vary to a considerable degree among countries in both direction and size dependent on how efficiently the prevalent legal system enforces private contracts. Moreover, and building on Djankov et al. (2003), who find systematic differences in the quality of the legal system dependent on the legal origin, I find significantly smaller marginal effects of increases in executive constraints for countries with a French legal origin. In 27 sample countries, the marginal effects are even negative.

Decomposing the interaction effects for groups of countries with different quality combinations confirms negative marginal effects of increases in executive constraints for 23 countries with absent or bad legal qualities of both types of institutions. This concerns Sub-Saharan African and Asian but also North African and Middle East countries at lower levels of economic development and implies that if extra-legal rules and practices organize a society, legal reforms in single institutions can backfire. This result relates to literature on lawlessness (Dixit, 2011) and second-best institutions (Rodrik, 2008) arguing that piecemeal reforms towards a best practice model can do more harm than good if the installed rules disturb the functioning of prevalent institutional set-ups or if the installed rules are politically captured (Diwan et al., 2019). Yet, the results suggest that piecemeal reforms do work when they close existing quality gaps across institutions. Also, and in accordance with Acemoglu and Johnson (2005), I find that a bad quality of legal property rights institutions constitutes a larger bottleneck for economic development compared to a bad quality of legal contracting institutions. Italy, India, and some Latin American countries seem to have found extra-legal ways to enforce private contracts that work with legal property rights institutions. In Western democracies, good legal qualities of both types of institutions have generated large positive interaction effects that have significantly contributed to raising long-term income levels.

The study is structured as follows: In Section 2, I discuss why the assumption of independent effects of property rights and contracting institutions is too strong. In Section 3, I make theoretical considerations on the channels and timespans of effects, discuss estimation challenges, and present the identification strategy and the empirical models. In Section 4, I address issues that are considered in the choice of the institutional measures and describe the dataset. In Section 5, I present the baseline and decomposition estimation results. In Section 6, I present the robustness tests. In Section 7, I discuss limitations and in Section 8, I conclude.

2. The argument

Property rights and contracting institutions are among the most important institutions in a market economy (North, 1981). They enable coordination among a large number of participants in many markets. Property rights assign asset ownership to individuals, groups, or the state and regulate the relationship between ordinary citizens and those with access to political power (Acemoglu et al., 2005). Different property rights regimes produce a specific but predictable allocation and utilization of resources in a society (Lueck and Miceli, 2004). Contracting institutions help to organize private economic exchange. They reduce incentives for contract breach and increase certainty on how others behave. They enable to transfer or modify property rights on assets across ordinary citizens and alternative uses.

Successful economic development requires an adequate quality, i.e., specification and enforcement, of property rights and contracting rules. Property rights and contracting institutions jointly reduce transaction costs and uncertainties and thereby jointly incentivize or constrain private investment in physical capital, human capital, and technology (North and Thomas, 1973; Jones, 2003). While property rights institutions secure the fruits of investment from being seized by others (notably the government), contracting institutions enable the fruits to be traded with others (mostly ordinary citizens). A bad quality of only one type of institutions can cause an inefficient allocation and utilization of resources: Despite a clear and enforceable title to property, an inefficient legal enforcement of private contracts constrains non-simultaneous transactions and the transfer of assets to high-valued uses. Despite the possibility of having private contracts efficiently enforced at court, ordinary citizens will not invest if they fear being expropriated by their government.

For contracting and property rights institutions, informal mechanisms can organize an economy until a certain degree of complexity. Property rights and private contracts can be defined and enforced by customs, norms, and in repeated interactions (Ellickson, 1991). However, as investment becomes large, long-lived, and asset-specific and as trade in goods and services occurs outside of repeated exchange relationships, informal contract enforcement mechanisms become an increasingly imperfect institutional solution and need to be supplemented or replaced by formal ones (Trebilcock and Leng, 2006; Libecap, 1993; Lueck and Miceli, 2004). The state has the authority to define property and contracting law and provide courts as legal mechanisms to enforce these laws. Complex economies rely on legal institutions to coordinate the usage, maintenance, and investment in assets (Demsetz, 1967).

As property rights and contracting institutions provide interrelated incentives and constraints for private investment, it is not possible to consider the return from one type of institutions independently of the quality of the other type. A certain quality of property rights institutions requires an incentive-compatible quality of contracting institutions to enable private investment. Different qualities and quality combinations are possible as we have learned from the Varieties of Capitalism literature (Soskice and Hall, 2001; Amable, 2003; Rougier and Combarrous, 2017). Societies at different stages but also at the same stage of economic development can be successfully organized by alternative institutional set-ups. What matters for the return from a single institution is its quality fit with interrelated institutions within and across different domains of the society.

3. Implementation

The steady evolution of GDP per capita levels in many countries around the world indicates a strong influence of constant factors. The huge and persistent cross-country differences in GDP per capita levels indicate that the constant factors are country-specific. Yet, there is some short-term variation. A glance into growth theory helps to understand the features of time series on GDP per capita levels and motivates the estimation strategy taken to study the joint effects of legal property rights and contracting institutions.

3.1. Theoretical background and channels of influence

Neoclassical growth theory explains differences in output with differences in physical and human capital accumulation, which in turn stem from differences in saving rates that are either exogenously given (Solow, 1956; Swan, 1956) or evolve endogenously (Ramsey, 1928; Cass, 1965; Koopmans, 1965). The remainder is understood as differences in total factor productivity. North and Thomas (1973):2 argue that factor accumulation and productivity “(...) are not causes of growth; they are growth”. In line with this, Acemoglu et al. (2005) distinguish between proximate and fundamental causes of growth. Proximate causes are the input factors in production. Fundamental causes underlie the proximate causes and drive investment in physical capital, human capital, and technology. While also considering geography, culture, and luck, Acemoglu et al. (2005) put emphasis on institutions.

To analyze the role of institutions in neoclassical growth models, reconsider a human capital augmented version of the Solow–Swan model of long-term economic growth with a constant returns to scale Cobb–Douglas production function and a labor-augmented technological progress:

$$Y(t) = K(t)^\alpha H(t)^\beta (A(t)L(t))^{1-\alpha-\beta}. \tag{1}$$

$Y(t)$ represents the output, $K(t)$ the stock of physical capital, $H(t)$ the stock of human capital, and $A(t)L(t)$ represents the stock of effective labor. A and L grow from initial levels $A(0)$ and $L(0)$ at exogenous rates g and n such that $A(t) = A(0)e^{gt}$ and $L(t) = L(0)e^{nt}$. $L(0)$ refers to the initial size of labor force. $A(0)$ stands for the initial state of technology. K and H grow endogenously. The stocks of physical and human capital increase over time via saving a fraction of output $s = s_K + s_H$, where s_K is the fraction of output invested in physical capital and s_H is invested in human capital. Physical and human capital depreciate at a constant rate δ . In equilibrium, physical capital per effective unit of labor, $k(t) = K(t)/A(t)L(t)$, and human capital per effective unit of labor, $h(t) = H(t)/A(t)L(t)$, are constant. Actual physical and human capital investment equal the break-even investment needed to prevent $k(t)$ and $h(t)$ from falling. The steady state values of k and h are determined by:

$$\begin{aligned} k^* &= \left(s_K^{1-\beta} s_H^\beta / (n + g + \delta) \right)^{1/(1-\alpha-\beta)}, \\ h^* &= \left(s_K^\alpha s_H^{1-\alpha} / (n + g + \delta) \right)^{1/(1-\alpha-\beta)}. \end{aligned} \tag{2}$$

Based on (1), output per worker can be written as:

$$Y(t)/L(t) = A(0)e^{gt} k(t)^\alpha h(t)^\beta. \tag{3}$$

As $k(t)$ converges to k^* and $h(t)$ converges to h^* , the growth rate of $Y(t)/L(t)$ converges to the growth rate of $A(0)$ which is g . The economy moves alongside a balanced growth path with $Y(t)/L(t)$ steadily growing at g . Yet, the balanced growth path needs to be found and it can temporarily be disturbed. In transition periods, the growth rate differs from g . A change in any right-hand side term of (2) causes a transition. $k(t)$ and $h(t)$ need to adjust to new steady state values. When the new steady state values are reached, the growth rate of $Y(t)/L(t)$ goes back to g .

Neoclassical growth theory misses to discuss the role of institutions. It bases on the assumption that institutions are optimal. From Coase (1937, 1960), North (1981, 1991), Acemoglu et al. (2005) and Nelson (2005) it has become apparent that institutions can be ill-defined, not adapt to market forces, and cause frictions with vast consequences for economic development. This makes it necessary to reevaluate theory. Consider (3). Mankiw et al. (1992) argue that $A(0)$ not only reflects technology but also resource endowments, climate, and institutions. If institutions that determine $A(0)$ are country-specific, then the aggregate production function and the steady state income levels are country-specific too. Moreover, institutions can affect $Y(t)/L(t)$ via $k(t)$ and $h(t)$. Institutions can determine any right-hand side variable of (2). Institutional changes that alter saving rates s_K and s_H , the population growth rate n , the technology growth rate g , or the depreciation rate δ change the optimal stock of physical and human capital per unit of effective labor k^* and h^* and therefore cause a temporary deviation of the growth rate of $Y(t)/L(t)$ from g .

3.2. Empirical model

To take account of the different channels and timespans of effects, I apply a two-step panel data approach that exploits within country variation to explain short-term variation in observed GDP per capita levels in a first step and between country variation to explain cross-country differences in long-term income levels in a second step. The two steps are linked. I estimate the time-invariant components of GDP per capita levels that are determined by country-specific constant factors in the first step and use these estimates as proxies for countries' long-term income levels in the second step. This empirical strategy complies with Islam (1995)'s approach of estimating "country effects" and constructing country-specific measures $A(0)_i$ to allow cross-country differences in the aggregate production function and steady state income levels.¹

Panel data has the potential advantage of utilizing within and between country variation for identification. Yet, (i) the features of time series on GDP per capita levels, (ii) the endogenous relationship between income and institutions, and (iii) the persistence of institutions make a proper identification complicated. The strong serial correlation of GDP per capita levels requires formulating a model that takes unobserved heterogeneity caused by country-specific constant factors into account. The endogenous relationship between income and institutions and the theoretical considerations on the channels of influence require allowing the country-specific constant factors and the explanatory variables – especially the measures of the proximate causes of growth – to be correlated. Challenges (i) and (ii) require the application of a FE estimator. There are, however, two drawbacks of the FE estimator in this setting: It uses within country variation only and it cannot estimate the effects of observable country-specific constant factors since there is no way to distinguish them from the effects of unobservable country-specific constant factors. This is problematic because of (iii), the persistence of institutions.

The two-step estimation approach allows dealing with these issues. In the first step, I take full account of the panel structure of the underlying dataset and use the FE least squares estimator to identify individual and interaction effects of legal property rights and contracting institutions on GDP per capita levels whilst controlling for country-specific constant factors, time, and some controls. The first-step model is given by:

$$\ln(y_{it}) = \beta_1 PR_{it} + \beta_2 C_{it} + \beta_3 PR_{it} \times C_{it} + z'_{it} \zeta + \mu_i + \theta_t + e_{it}, \tag{4}$$

where $\ln(y_{it})$ is the natural logarithm of the real GDP per capita level of country i at period t . PR_{it} refers to legal property rights institutions, C_{it} refers to legal contracting institutions, $PR_{it} \times C_{it}$ is their interaction, and β_1 , β_2 and β_3 are the coefficients and parameters of interest. z_{it} is a vector of control variables that includes measures of the proximate causes of growth and ζ is a vector capturing their effects. μ_i is the unobserved heterogeneity term that captures the effects of country-specific constant factors in $A(0)_i$. θ_t is a set of dummies capturing year fixed effects. e_{it} are robust idiosyncratic errors. As μ_i explains long-term cross-country differences in the GDP per capita levels, the remainder is short-term variation in GDP per capita levels that is left to be explained by institutional changes in legal property rights and contracting institutions, changes in saving rates and the stocks of physical and human capital, changes in some observed control variables, time, and unobserved time-variant factors captured in the errors.

In the second step, I use the BE least squares estimator and variation across countries to identify the effects of legal property rights and contracting institutions on long-term income levels. As dependent variable, I use the estimate of the unobserved heterogeneity term μ_i obtained in the first-step regression. The corresponding second-step model is:

$$\hat{\mu}_i = \alpha + \gamma_1 \overline{PR}_i + \gamma_2 \overline{C}_i + \gamma_3 \overline{PR}_i \times \overline{C}_i + \overline{z}'_i \eta + (\alpha_i - \alpha + \bar{e}_i), \tag{5}$$

where bars indicate mean values and dots formally define that time has been averaged out. I regress $\hat{\mu}_i$ on the random intercept α , the means of the legal qualities of the two types of institutions and their interaction, the set of control variables including proximate

¹ Acemoglu and Johnson (2005) proxy countries' long-term income levels with time-averaged GDP per capita levels and estimate a cross-sectional model to assess the effects of institutions. I prefer the two-step approach as it allows to distinguish between channels and short-term versus long-term growth effects, which can give very different policy implications (Rodrik, 1999; Rodrik et al., 2004; Hausmann et al., 2005). Also, estimates may suffer from omitted variable bias if the effects of constant country-specific factors are ignored. Islam (1995):1132 states that it is only possible to correct for this bias in panel data frameworks. Yet, since Acemoglu and Johnson (2005) constitutes the benchmark for this study, I estimate their model in a robustness test.

causes of growth, an error that consists of country-specific random effects α_i , the random intercept α , and robust mean idiosyncratic errors $\bar{\epsilon}_i$. γ_1 , γ_2 and γ_3 are the parameters of interest and η captures the effects of the control variables in the second-step model. An alternative variant of the second-step model would be taking first-period values. I prefer using mean values rather than first-period values in the baseline model because mean values allow to capture the cumulative effects of institutions on income levels over the period 2005–2015, put less weight on, and rely less heavily on accurate ratings at single points in time.²

3.3. Marginal effects

In linear regression models that exclude the interaction term, the marginal effects of one type of institutions are its partial derivative of the income measure. This coincides with β_1 for the short-term marginal effects and γ_1 for the long-term marginal effects of legal property rights, and β_2 and γ_2 for legal contracting institutions, respectively. Such a model, however, assumes independence of the effects of the two types of institutions. The arguments put forward in Section 2 give reason to relax this assumption and allow the marginal effects of one type of institutions to vary with the quality level of the other. For the second-step model given in (5), the marginal effects of improvements in institutions' legal qualities on countries' long-term income levels become:

$$\begin{aligned}\frac{\partial \hat{\mu}_i}{\partial PR_i} &= \gamma_1 + \gamma_3 \times \bar{C}_i, \\ \frac{\partial \hat{\mu}_i}{\partial \bar{C}_i} &= \gamma_2 + \gamma_3 \times \overline{PR}_i.\end{aligned}\tag{6}$$

The marginal effects consist of two parts: The first parts, γ_1 and γ_2 , capture the individual effects of an increase in the average legal quality of one type of institutions. The second parts, $\gamma_3 \times \bar{C}_i$ and $\gamma_3 \times \overline{PR}_i$, capture the interaction effects that depend on the country-specific average legal quality of the other type of institutions. One can easily see that including the interaction term produces country-specific marginal effects. In models that exclude the interaction term, γ_3 is zero by assumption and the estimated marginal effects are the same for all countries.

4. Data and summary statistics

I utilize panel data from 130 countries for the period 2005–2015. Table A.1 in the Appendix reports the sample countries and country-specific information including $\hat{\mu}_i$ and the 2005–2015 averages for per capita income and institutional quality ratings. The sample covers a quite even split of low, middle, and high income countries from all world regions: 20 Western democracies including Japan, 23 countries from Eastern Europe and the former Soviet Union, 14 Asian countries, 14 countries from North Africa and the Middle East, 37 Sub-Saharan African countries, and 22 countries from Latin America and the Caribbean. The dataset includes variables on institutions, macroeconomic outcomes, demographic and cultural factors. Table A.2 in the Appendix reports the definitions of variables and sources of data.

4.1. Measurement issues

Literature points to the issue of finding accurate measures of institutions. Glaeser et al. (2004) name two characteristics that have to be considered for measurement: (i) institutions constrain behavior, and (ii) institutions are persistent. The usage of “rule of law” and other standard measures of political institutions provided by the World Bank, Polity Project, and the International Country Risk Guide has been criticized.³ The problem with multidimensional indices is that they capture a number of different constraints. It is hard to grasp what they measure and what policy recommendations can be drawn from their estimates.

Voigt (2013) adds further considerations on what should influence how we define and measure institutions. One is that an institution consists of two components: The first component is the content of a rule, e.g., the specification of property or contracting rights. The second is the factual implementation of the rule, which critically depends on the means available to sanction those who violate the specified rule. The factual implementation is important. It helps to make a de jure rule effective (Voigt and Gutmann, 2013). Put differently by Aoki (2001), it helps to “institutionalize” a rule, i.e., turn a de jure rule into a de facto rule. While non-compliance with rules regulating the relationship between ordinary citizens can be checked by political institutions, the factual implementation of rules regulating the relationship between ordinary citizens and those with political power is often precarious. Checks and balances on governments and ruling elites including an independent legislature and judiciary help to commit executives to the promise to not expropriate ordinary citizens (Voigt and Gutmann, 2013). Checks and balances thereby help to turn de jure political rules into de facto political rules and eventually into economic growth.

² Using first-period values, however, has the advantage of being less prone to endogeneity issues. Therefore, I present the results when using this alternative variant of the second-step model as a robustness test alongside and as a reduced form of an instrumental variable estimation.

³ A number of authors critically evaluate frequently used indicators, see Woodruff (2006) on the issue of multicollinearity among different institutional indicators, Keefer (2004) on the issue of measurement errors, Cheibub et al. (2010) on the need for a clear theoretical formulation on the phenomenon that should be measured, Munda and Nardo (2005) on the aggregation rules to construct indicators, and Oman and Arndt (2010) on the lack of transparency in the construction of indicators.

Table 1
Summary statistics for the main regressions.

	Obs	Mean	St.dev	Min	Max
<i>Dependent variables</i>					
Log real income per capita ($\ln(y_{it})$)	1246	8.576	1.517	5.726	11.425
Long-term income level ($\hat{\mu}_i$)	1246	0.000	1.495	-2.639	2.969
<i>Institutional measures</i>					
Executive Constraints (EC)	1246	0.000	1.000	-2.196	0.920
Legal Contract Enforcement (LCE)	1246	0.000	1.000	-2.868	2.662
Interaction (EC \times LCE)	1246	0.000	1.000	-3.586	3.263
<i>Covariates</i>					
Investment (% of GDP)	1246	23.908	6.784	1.525	61.469
Population (per sqkm)	1246	180.777	681.271	2.468	7807
Trade (% of GDP)	1246	88.497	45.961	19.101	441.604
Cultural fractionalization	1246	0.312	0.212	0	0.733
Years of schooling	1246	9.308	2.071	4	15
Capital stock/GDP	1208	1.869	0.656	0.414	4.947

4.2. Institutions data

I choose measures that focus on the factual implementation and the de facto qualities of legal property rights and contracting institutions. As baseline measure of legal property rights institutions, I use Polity IV's *Executive Constraints* variable that is also the preferred property rights institutions measure in [Acemoglu and Johnson \(2005\)](#). Initially referred to as “decision rules”, the variable measures to what extent institutionalized rules constrain the decision-making powers of chief executives, whether individuals or collectives ([Gurr, 1997](#)). The constraints may be imposed by any accountability groups. In Western democracies, these are usually legislators and a strong independent judiciary. Others are the ruling party in a one-party state, councils of nobles or powerful advisors in monarchies, or the military in coup-prone polities. Experts monitor and rate countries yearly alongside a 1-to-7 scale. Since the variable measures the institutionalized rules protecting citizens against the power of governments and ruling elites, it captures to what extent private property is protected from being taken by authorities.

As baseline measure of legal contracting institutions, I make use of World Bank's ‘Enforcing Contracts’ indicator, thereafter called *Legal Contract Enforcement*. The indicator is constructed from a number of questions taken from the Doing Business survey. It measures the time and costs of resolving a commercial dispute in court. It includes an assessment of the quality of the judicial process, that is an assessment of whether a country has adopted a series of good practices to promote quality and efficiency in the court system. The data is collected through studies of codes of civil procedure and other court regulations as well as questionnaires completed by local litigation lawyers and judges. A country's final score in a given year is the average score of three indicator components in that year: time, costs, and quality of the judicial process. This methodology builds on [Djankov et al. \(2003\)](#).

4.3. Dependent and control variables data

In the first-step regression, I use the natural logarithm of real GDP per capita levels in constant 2010 US Dollars as dependent variable. In the second-step regression, I use $\hat{\mu}_i$, the estimate of the unobserved heterogeneity term obtained in the first-step regression, as dependent variable. As described in Section 3.2, $\hat{\mu}_i$ captures the country-specific constant part of GDP per capita levels and serves as a proxy for countries' long-term income levels.

There is a broad literature on factors influencing countries' income levels and growth rates. In a cross-country study, [Barro \(1996\)](#) finds significant effects of physical capital investment, human capital investment, macroeconomic policies, trade openness, fertility, life expectancy, and the rule of law. [Tabellini \(2010\)](#) stresses the role of culture and institutions. Following the existing literature, I include a set of observable neoclassical growth variables (investment, education, population), a trade variable, and a variable measuring cultural fractionalization as controls.

4.4. Descriptive statistics

[Table 1](#) reports the summary statistics for the main regressions using [Acemoglu et al. \(2005\)](#)'s baseline measures of institutions. The two dependent variables are highly, almost perfectly, correlated with a coefficient of 0.9961. This confirms that income levels are strongly influenced by constant factors. The variation in both income measures between countries is substantial. To exemplify: Over 2005–2015, the Netherlands's average income per capita was 50423 US\$. This is 50 times the average income per capita in Senegal which was 998 US\$. Accordingly, with a $\hat{\mu}_i$ value of +2.211, the Netherlands reach place 8 on the list that ranks the 130 sample countries according to their level of economic development. Senegal ranks 103 with a $\hat{\mu}_i$ value of -1.693. In comparison, countries with an average income per capita of around 5000 US\$ (Iraq, Azerbaijan, Jamaica, and China) rank around place 60 and are close to the sample's average long-term income level where $\hat{\mu}_i = 0$.

The measures of institutions also show a substantial variation between countries. I adjust the scalings, subtract the means, and divide by the standard deviations of the two measures and their interaction term, respectively. I do this for two reasons: First,

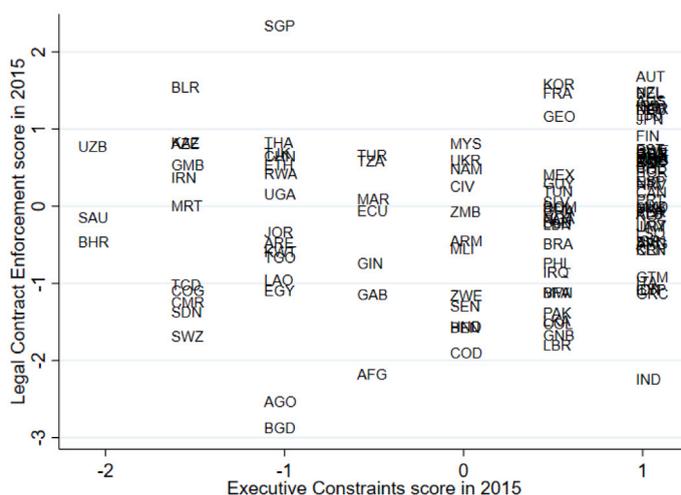


Fig. 1. Distribution of *Executive Constraints* and *Legal Contract Enforcement*.

demeaning the values solves the issue of multicollinearity that occurs when interaction terms are included alongside the interacted variables in regression analysis.⁴ Second, demeaning and normalizing the standard deviations of the institutional measures to one makes interpretation of the results easier. The coefficient estimates then correspond to the marginal effects on income levels after a one standard deviation increase in the legal quality. Investment and trade as percentage of GDP show between and within country variation. Population density has a positive linear trend over the years. Cultural fractionalization is time-invariant data for the year 2003 provided by Fearon (2003). The index uses the structural distance between languages spoken in a country to proxy for the cultural distance between groups in a country.⁵ Since the index only provides data for one year, it vanishes into μ_i in the first-step estimation. However, cultural distance is an important predictor of cross-country differences in long-term income levels. As a proxy for human capital, I use a variable on years of compulsory educational attainment provided by the World Bank. Since there are missing values over time, I follow Voigt and Gutmann (2013) in using 3-year moving averages.

In order to be able to disentangle the effects of the two types of institutions and their interaction term, the measures of legal property rights and contracting institutions must capture different phenomena that are not too strongly correlated. Following Acemoglu et al. (2005), I choose the quality of a political institution as an indirect measure of legal property rights protection to emphasize the close relationship between property rights and political institutions. *Executive Constraints* certainly captures a different phenomenon, has different aims, and is subject to different determinants compared to *Legal Contract Enforcement*, which is a more direct measure of legal contracting institutions. A strong correlation between *Executive Constraints* and *Legal Contract Enforcement* could still be an issue. A strong correlation would be in accordance with Acemoglu and Robinson (2006, 2008), who model economic institutions as equilibrium outcomes of political institutions. Good (bad) economic institutions could be the consequence of good (bad) political institutions.

Fig. 1 reassures that there is enough variation in the quality combination of the two institutional measures to enable a proper identification of individual and interaction effects. Countries' *Executive Constraints* ratings in 2015 are plotted against countries' *Legal Contract Enforcement* scores in 2015. The 130 sample countries show all sorts of quality combinations. While rich Western countries cluster at high levels of *Executive Constraints* and show some variation around medium and high qualities of *Legal Contract Enforcement*, non-Western countries of various income levels are spread throughout the plot. While India shows the highest quality of *Executive Constraints* and the third lowest quality of *Legal Contract Enforcement*, Singapore shows the highest quality of *Legal Contract Enforcement* but is rated at a below sample average quality of *Executive Constraints*. Uzbekistan, Saudi Arabia, and Bahrain apply the least checks and constraints on executives but show a sample average efficiency in the legal enforcement of private contracts. Angola and Bangladesh, and also Chad, the Republic of Congo, Cameroon, Sudan, and Eswatini are characterized by few checks on executives and an inefficient legal enforcement of private contracts.

⁴ Not demeaning the values of the institutional measures increases the variance inflation factors (vif) significantly and clearly above the threshold of 20, especially for the interaction term. This indicates severe multicollinearity. Demeaning the values solves this issue while leaving the coefficient estimates qualitatively unchanged.

⁵ The cultural fractionalization index is highly correlated with the ethnic fractionalization index (correlation coefficient: 0.78) provided by Fearon (2003) and also with the ethnolinguistic fractionalization index (ELF) from Soviet Atlas (correlation coefficient: 0.82). The three indices are substitutable in cross-section analyses.

Table 2
Results of the baseline models.

	1st step: FE		2nd step: BE			
	(1)		(2)		(3)	
	Coeff.	SE	Coeff.	SE	Coeff.	SE
Executive Constraints	0.016	(0.015)	0.352***	(0.119)	0.302**	(0.136)
Legal Contract Enforcement	0.037	(0.032)	0.492***	(0.101)	0.483***	(0.105)
EC × LCE	0.023	(0.018)	0.346***	(0.095)	0.327***	(0.110)
Investment (% of GDP)	0.004***	(0.001)	0.008	(0.016)	0.007	(0.017)
Population (per sqkm)	−0.000	(0.000)	0.000	(0.000)	0.000	(0.000)
Trade (% of GDP)	−0.001***	(0.000)	0.006**	(0.003)	0.005	(0.003)
Years of schooling	0.018***	(0.005)	0.170***	(0.049)	0.178***	(0.053)
Cultural fractionalization			−1.260***	(0.455)	−0.997**	(0.478)
Capital stock/GDP					0.468***	(0.178)
Observations	1246		130		125	
Groups	130		.		.	
R ²	0.5195		0.5375		0.5541	

Notes: Dependent variables: *Log real income per capita* in the first-step FE regression in (1) and $\hat{\mu}_i$ in the second-step BE regressions in (2) and (3). *, **, *** indicate 10, 5, 1% significance levels. Robust standard errors in parentheses for (1) and bootstrap standard errors for (2) and (3). Constants are included in both steps but not reported. The first-step FE regression controls for time and country-specific constant factors.

5. Results

The results are based on the model given in (4) using a FE least squares estimator and the model given in (5) using a BE least squares estimator to disentangle the effects of legal property rights and contracting institutions' income levels as described in Section 3.2. I find strong and significant individual and interaction effects on countries' long-term income levels, which underlines the crucial role of institutions as fundamental causes of growth. Because of the second part of the right-hand side in (6), the marginal effects are country-specific. A decomposition of the interaction effects shows that the size and the direction of the interaction effects vary among groups of countries with different quality combinations of the two types of institutions.

5.1. Baseline results

Table 2 presents the estimation results of the baseline models. Columns (1) show the coefficient estimates and standard errors for the first-step FE estimation. I cannot relate variation in real GDP per capita levels over 2005–2015 to changes in the institutional qualities over that period. I do find significant positive effects of increases in investment rates and increases in years of compulsory schooling, which capture changes in the stocks of physical and human capital over the period. This finding is in accordance with the theoretical considerations presented in Section 3.1, which explain short-term variation in income levels with input factor accumulation.⁶

Columns (2) show the coefficient estimates and standard errors for the baseline second-step model. I find positive and highly significant effects of legal property rights, legal contracting institutions, and their interaction term on countries' long-term income levels. This points not only to the important role of institutions but also to the important role of institutional complementarities as fundamental causes of growth. For the factor input variables, I find no significant effect of capital investment and, in accordance with Wilson and Briscoe (2004), a positive and significant effect of education. The estimates on the other control variables are as expected: In accordance with Brunner (2003), I find a positive and significant effect of trade openness. In accordance with Alesina et al. (2003) and Fearon (2003), I find a strong and significant negative effect of cultural fractionalization.

μ_i not only captures $A(0)_i$ but also the physical capital stock \bar{K}_i and the human capital stock \bar{H}_i that have been accumulated until the time point of the analysis. *Years of schooling* is a stock variable and captures the parts of μ_i that can be explained by \bar{H}_i . *Investment (% of GDP)* captures saving rates but not stocks. Therefore, I add estimates of capital stocks provided by the IMF for 125 sample countries to control for the \bar{K}_i parts of μ_i . What is then left to be explained are the $A(0)_i$ parts. Columns (3) show that the estimates of the institutional measures do not change much when the capital stock weighted by the size of the economy is added as a control for \bar{K}_i . This suggests that legal property rights and contracting institutions are fundamental causes of growth that affect long-term income levels both individually and in their combination via growth theory's $A(0)$ term.

Fig. 2 presents a set of residual plots for the second-step estimates presented in columns (2). The residual-versus-fitted plot in (a) suggests no severe issues of non-linearity and unequal error variances in the model. Oil-rich countries, Moldova, and Italy show up as somewhat outliers but are kept in the sample since their omission does not change the estimates significantly. (b), (c), and (d) are partial residual plots that show the partial correlations of legal property rights institutions, legal contracting institutions, and their

⁶ Also following the theoretical considerations, I tested whether reforms in legal property rights and contracting institutions in 2005–2015 can be related to input factor accumulation in 2005–2015. I could not find reliable evidence. There is little variation in institutional qualities over the observed period, which constrains identification.

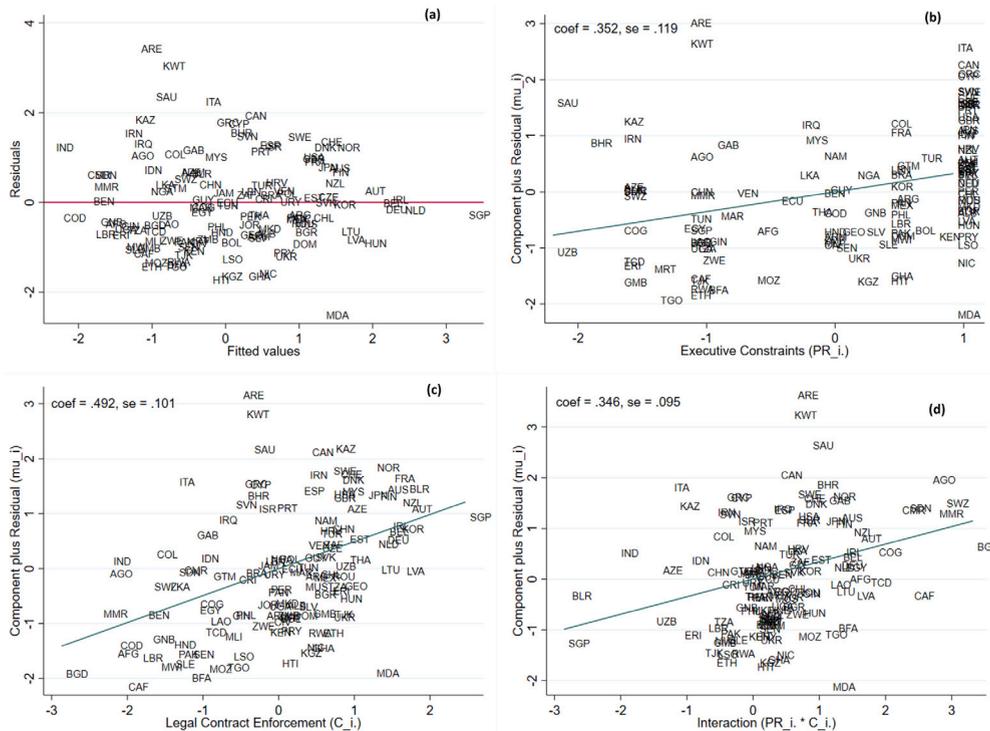


Fig. 2. Residual plots for the baseline second-step estimation.

interaction with μ_i . The fitted lines in (b), (c), and (d) have slopes equal to the estimated coefficients in the baseline second-step model. The distribution of the sample countries among the quality range of *Legal Contract Enforcement* is somewhat more dispersed and less clustered in world regions than their distribution among the quality range of *Executive Constraints*. A number of non-Western countries including Singapore, Korea, Latvia and also low-income countries such as Eritrea and Ethiopia score above while Italy and Greece score below sample average qualities in the legal enforcement of private contracts. With respect to the interaction term, half of the sample countries are clustered in the range of -1 to 1 . For countries within this range, individual effects are closer to marginal effects. For countries outside of this range, interactions cause a significant spread between individual and marginal effects, as discussed in the following. Fig. A.1 in the Appendix shows that replacing the proxy for countries’ long-term income levels, μ_i , with the logarithm of countries’ 2005–2015 averaged real GDP per capita levels, $\ln(\bar{y}_i)$, produces almost identical plots.

5.2. Country-specific marginal effects

The baseline estimation results entail cross-country differences in marginal effects. Recall (6). While the individual effects are the same for all countries, the interaction effects depend on prevalent institutional qualities in the countries. The difference in countries’ marginal effects due to the interaction effects can be remarkable as Fig. A.2 in the Appendix and the following country examples show. Consider the long-term income effects of improving legal property rights institutions for two different African countries: Chad and Gambia. Both score 2 on the 1-to-7 ranking of Polity IV’s *Executive Constraints* variable throughout the period. The baseline estimation results suggest that the same one standard deviation increase in checks and balances on executives yields an increase in the long-term income level of more than 30% in Gambia and roughly zero in Chad.⁷ While the individual effects increase both countries’ long-term income levels by 23.55%, the above sample average legal efficiency of enforcing private contracts in Gambia, that scores 0.397 for \bar{C}_i , and the below sample average legal efficiency of enforcing private contracts in Chad, that scores -1.021 for \bar{C}_i , produces interaction effects that are positive and supplement the individual effects in Gambia by 9.20% and that are negative and diminish the individual effects in Chad by 23.63%.

Studying further examples of African countries suggests that the legal origin matters for whether a country benefits more or less from increases in executive constraints. Mali, Cote d’Ivoire, Namibia, and Zambia all score 5 on Polity IV’s *Executive Constraints* variable throughout the period. They, however, differ widely in their \bar{C}_i scores: -0.762 for Mali, -0.117 for Cote d’Ivoire, -0.069 for Zambia, and 0.418 for Namibia. The same increase in checks and balances on executives is suggested to raise long-term income

⁷ The marginal effects can easily be calculated by plugging the coefficient estimates of γ_1 , γ_3 , and countries’ mean *Legal Contract Enforcement* scores into (6) and divide the outcome by the standard deviation of $\hat{\mu}_i$, which yields the formula: $\frac{0.352 + 0.346 \times \bar{C}_i}{1.495}$.

Table 3
Two-sample t test comparing means of marginal effects.

Group	Obs	Mean	Boot. St. Err.	Std. Dev.	[95% Conf. Interval]	
Legal origin: Other	71	.2827	.0297	.2483	.2244	.3410
Legal origin: French	59	.1367	.0264	.2040	.0850	.1884
combined	130	.2164	.0210	.2398	.1748	.2580
diff		.1460	.0404		.0661	.2259
diff = mean(0) - mean (1)			t = 3.6145			
H0: diff = 0			degrees of freedom = 128			
Ha: diff < 0			Ha: diff != 0		Ha: diff > 0	
Pr(T < t) = 0.9998			Pr(T > t) = 0.0004		Pr(T > t) = 0.0002	

levels by 5.93% in Mali, 20.86% in Cote d'Ivoire, 21.95% in Zambia, and 33.23% in Namibia. Mali and Cote d'Ivoire are like Chad former French colonies, apply civil law, and have a legal efficiency below the sample average. Namibia and Zambia are like Gambia former British colonies, apply common law, and have a legal efficiency above the sample average.

To test whether having a French legal origin is disadvantageous when intending to gain in long-term income levels via improvements in legal property rights institutions, I perform a two-sample t test on the means of the country-specific marginal effects for the group of countries with a French legal origin as compared to the group of countries with other legal origins. [Table 3](#) presents the results.

The average marginal effects are 13.67% for 59 sample countries with a French legal origin. For 71 sample countries with British, German, Scandinavian, or other legal origins they are 28.27%, i.e., twice as large. The difference is statistically significant. The difference stems from smaller or negative interaction effects in countries with a French legal origin that show worse qualities of legal contracting institutions. This finding extends [Djankov et al. \(2003\)](#), who find an adverse effect of French civil law on the efficiency and quality of legal dispute resolution working through the channel of formalism. Formalism is systematically greater in civil than in common law countries. [Djankov et al. \(2003\)](#) associate a higher degree of formalism not only with a higher expected duration of judicial proceedings but also with less consistency, less honesty, less fairness in judicial decisions, and more corruption.

Strikingly, the net effects of increasing checks and balances on executives are negative for 27 out of the 130 sample countries. This concerns 16 Sub-Saharan African, 7 Asian, 2 Latin American, 1 Western (Italy), and 1 North African country (Egypt) with absent or bad legal qualities of one or both institutions. The unfortunate finding that improvements in legal property rights can yield negative income effects is in accordance with [Dixit \(2011\)](#), who uses game theory to analyze private modes of governance, the interaction of extra-legal rules and practices with the government's law, and problems of transitioning from relation-based to legal-based systems of property rights and contract enforcement. [Dixit \(2011\)](#) shows theoretically for what [Libecap \(1993\)](#), [Ensminger \(1997\)](#), and [Johnson et al. \(2002\)](#) provide empirical evidence: Legal reforms aiming to secure property rights and spur growth can be ineffective and even reduce income if the formalized rights are misfitted to prevalent relations of production, distributional norms, and conventions of exchange.⁸ Moreover, political capture may diminish or neutralize the positive growth effects of legal reforms in property rights and reinforce persisting market distortions. [Diwan et al. \(2019\)](#) discuss how in the frame of liberalization efforts in Arab societies, privatized assets ended up in the hands of political cronies and afforded new opportunities for reconfiguring the power distribution among political and economic elites.

5.3. Marginal effects for different quality combinations

It is possible that the baseline results are driven by subsets of countries with a distinct quality combination of legal property rights and contracting institutions. This section digs deeper into this consideration. The plots in [Fig. 3](#) take a closer look at the interaction effects that are estimated in the baseline second-step model. The margins plots in (a) and (b) suggest that the marginal effects increase in the legal quality of one institution if the other institution has a sample average legal quality or higher. The marginal effects become negative, i.e., improvements in the legal quality of an institution lead to a reduction in income, if the other institution is not or only badly secured by legal means.

The contour plot in (c) provides further evidence that the quality fit of property rights and contracting institutions is crucial. Income levels are suggested to be highest if legal property rights and contracting institutions are of high quality. Income levels are lowest when there is a mismatch of quality levels, that is, when a high legal quality of one type of institutions is matched with a low legal quality of the other type of institutions. In the absence or with bad legal qualities of both types of institutions, extra-legal institutional arrangements seem to allow achieving a higher income level as compared to mixed arrangements. (d) plots the countries with their 2005–2015 averaged legal property rights and contracting institutions quality combinations. For Bangladesh, Myanmar, Angola, The Central African Republic, but also for Eswatini, Sudan, Congo, Cameroon and Chad, legal improvements in one type of institutions have to come with improvements in the other type to not backfire. For Uzbekistan, Bahrain and Belarus as well as for

⁸ In a compelling study of formal and informal rights to land in Africa, [Ensminger \(1997\)](#) shows that the formalization of land tenure in many African countries widely failed to increase agricultural investment and productivity for the new land laws' incompatibility with the prevalent customary norms and their incapability of replacing the prevalent customary norms.

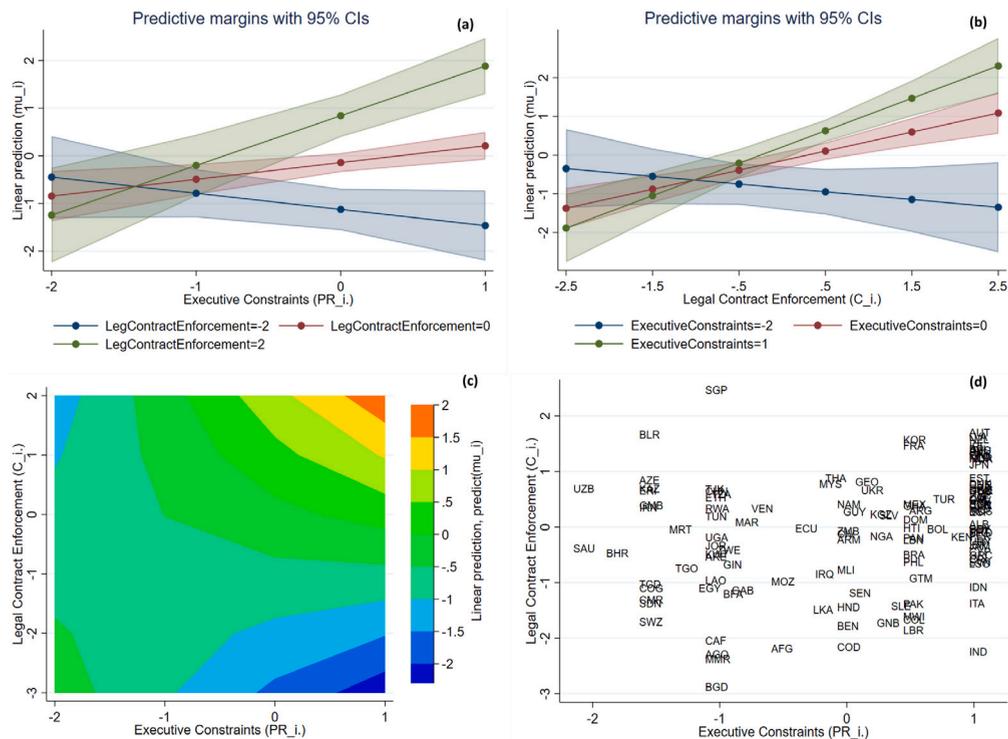


Fig. 3. Interaction plots for the baseline second-step estimation.

India, Liberia, Guinea-Bissau, Malawi, Colombia, and to some degree also for Italy and Indonesia, prevalent institutional mismatches seem to constitute a bottleneck for economic development.

In the next step, I decompose the interaction effects. I divide the sample into four groups of countries with different combinations of above and below sample average legal qualities. 43 countries display above sample average and 36 below sample average legal qualities of both types of institutions. 31 countries have an above sample average quality of legal property rights but a below sample average quality of legal contracting institutions. 20 countries demonstrate the opposite. To measure if the interaction effects differ among the four groups, I estimate:

$$\hat{\mu}_i = \alpha + \gamma_1 \overline{PR}_i + \gamma_2 \overline{C}_i + \gamma_3 \overline{PR}_i \times \overline{C}_i \times \mathcal{D}_{qq} + z'_i \eta + (\alpha_i - \alpha + \bar{\epsilon}_i). \tag{7}$$

\mathcal{D}_{qq} assigns each country to one of the four groups of quality combinations. The subscript q refers to the legal quality of each type of institutions. The first q refers to the quality of legal property rights institutions, and the second q refers to the quality of legal contracting institutions. + refers to an above and – refers to a below sample average quality. Columns (1) in Table 4 show the estimates when three groups enter the model and the fourth is omitted to serve as a reference. The results suggest that the baseline estimate of the interaction effects is driven by two groups of countries: The group with above sample average legal qualities of both types of institutions for which the interaction effects are significant, large, and positive, and the group with below sample average legal qualities of both types of institutions for which the interaction effects are significant, large, and negative.⁹

Columns (2) to (5) in Table 4 report the results when the interaction effects are estimated for each group separately. This allows the slope and the intercept to vary among quality groups. Based on the group-specific estimation results, Fig. 4 presents the marginal effects of *Executive Constraints* and *Legal Contract Enforcement* in two plots. The results support previous findings. What is more, they provide further evidence for the quality fit argument. In 43 rich and predominately Western countries with good legal qualities in both types of institutions, large positive interaction effects have significantly raised income levels. The results suggest that ignoring interaction effects is only appropriate for the group of 31 countries that provide sufficient checks on executives but show a bad legal contract enforcement. For this group, that includes some Latin American countries but also Italy and India, marginal effects are close to individual effects and, therefore, more similar. People in this group of countries may have found ways to compensate for the bad quality of legal contracting institutions with extra-legal arrangements that work with legal property rights institutions. This is in accordance with the conclusions drawn in Acemoglu and Johnson (2005).

⁹ Note that a positive estimate of γ_3 produces negative interaction effects for countries with a below sample average legal quality of institutions. This can be seen from (6).

Table 4
Results of the decomposition models.

	2nd step: BE									
	(1)		(2)		(3)		(4)		(5)	
	Coeff.	SE	Coeff.	SE	Coeff.	SE	Coeff.	SE	Coeff.	SE
Executive Constraints	0.331**	(0.157)	0.227*	(0.127)	0.501***	(0.134)	0.340**	(0.144)	0.165	(0.165)
Legal Contract Enforcement	0.575***	(0.131)	0.264**	(0.115)	0.637***	(0.104)	0.449***	(0.118)	0.589***	(0.123)
EC × LCE × \mathcal{D}_{++}	0.771**	(0.351)	1.188***	(0.327)						
EC × LCE × \mathcal{D}_{--}	0.641***	(0.188)			0.730***	(0.177)				
EC × LCE × \mathcal{D}_{+-}	-0.418	(0.322)					0.023	(0.373)		
EC × LCE × \mathcal{D}_{-+}									0.616*	(0.351)
Investment (% of GDP)	0.007	(0.015)	0.006	(0.016)	0.007	(0.015)	0.001	(0.017)	0.003	(0.017)
Population (per sqkm)	0.000	(0.000)	0.000	(0.000)	0.000	(0.000)	0.000	(0.000)	0.000	(0.000)
Trade (% of GDP)	0.007**	(0.003)	0.007**	(0.003)	0.007**	(0.003)	0.007**	(0.003)	0.006**	(0.003)
Years of schooling	0.176***	(0.049)	0.171***	(0.050)	0.170***	(0.049)	0.150***	(0.051)	0.150***	(0.050)
Cultural fractionalization	-1.250***	(0.425)	-1.366***	(0.440)	-1.206***	(0.435)	-1.376***	(0.462)	-1.360***	(0.465)
Observations	130		130		130		130		130	
R ²	0.5601		0.5332		0.5402		0.4932		0.5112	

Notes: Dependent variable: $\hat{\mu}_i$ as a proxy for countries' long-term income levels obtained in the first-step regression. *, **, *** indicate 10, 5, 1% significance levels. Bootstrap standard errors in parentheses. Constants included but not reported. The group dummies allow the interaction effects to vary among groups of countries with different combinations of above (+) and below (-) sample average qualities of *Executive Constraints* and *Legal Contract Enforcement*. In columns (1), three group dummies are included in the regression, the fourth group of countries with a - + quality combination is omitted and serves as a reference. Columns (2) to (5) show the estimates when the group dummies enter the regressions separately.

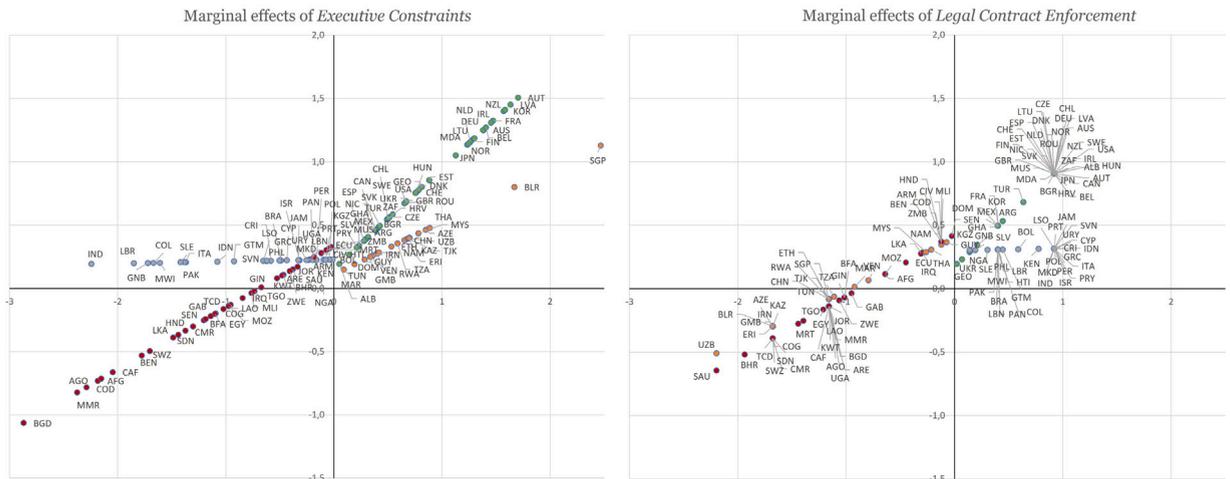


Fig. 4. The left diagram plots the estimated marginal effects of a one standard deviation increase in the quality of *Executive Constraints* and the right diagram plots the estimated marginal effects of a one standard deviation increase in the quality of *Legal Contract Enforcement* for each of the 130 sample countries. The interaction effects are estimated separately for groups of countries with different quality combinations as presented in Table 4. Countries represented by green dots have a ++ quality combination. Countries represented by red dots have a -- quality combination. Countries represented by grey dots have a +- quality combination. The y-axis gives the marginal effects of the respective institution in scope that varies with the quality level of the other type of institutions that is given on the x-axis. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

Also in accordance with Acemoglu and Johnson (2005), it seems to be harder to compensate for a bad quality of legal property rights institutions. The estimates for the - + quality group presented in columns (5) provide the strongest support for the quality fit argument. When institutional changes increase the quality fit, positive interaction effects can be achieved. When institutional changes decrease the quality fit, the interaction effects become negative. Consider Belarus with a -1.676 rating for *Executive Constraints* and a 1.672 score for *Legal Contract Enforcement*: Increasing the legal quality of property rights institutions produces large increases in income levels for closing the quality gap to legal contracting institutions. Increasing the legal quality of contracting institutions instead increases the quality gap which is suggested to have negative income effects.

For the group of 36 countries with absent or bad qualities of *Executive Constraints* and *Legal Contract Enforcement*, legal reforms in any type of institutions produces negative interaction effects that may outweigh positive individual effects if not matched with complementary legal reforms in the other type of institutions. This concerns countries at low-income levels in Sub-Saharan Africa, Asia, North Africa, and the Middle East. Increasing the quality of legal contract enforcement for one standard deviation while leaving executives unchecked is suggested to be roughly ineffective in Morocco, Gambia, Guinea, and Tanzania, while it is suggested to lead to a reduction in income levels in Chad, Sudan, Eswatini, Cameroon, and the Republic of Congo.

Table 5
Results of restricted and extended models.

	2nd step: BE					
	(1)		(2)		(3)	
	Coeff.	SE	Coeff.	SE	Coeff.	SE
Executive Constraints	0.335***	(0.126)	0.198**	(0.099)	0.211**	(0.104)
Legal Contract Enforcement	0.449***	(0.110)	0.243**	(0.096)	0.208**	(0.103)
EC × LCE			0.186**	(0.083)	0.276**	(0.129)
Investment (% of GDP)	0.000	(0.017)	0.003	(0.012)	0.007	(0.013)
Population (per sqkm)	0.000	(0.000)	0.000	(0.000)	0.000	(0.000)
Trade (% of GDP)	0.007**	(0.003)	0.007***	(0.002)	0.007***	(0.002)
Years of schooling	0.149***	(0.051)	0.117***	(0.045)	0.102**	(0.046)
Cultural fractionalization	−1.386***	(0.458)	−0.443	(0.434)	−0.527	(0.417)
North Africa/Middle East			−0.585*	(0.352)	−0.509	(0.360)
Latin America			−1.663***	(0.239)	−1.575***	(0.266)
Eastern Europe			−1.710***	(0.253)	−1.350***	(0.303)
Asia			−1.725***	(0.343)	−1.549***	(0.355)
Sub-Saharan Africa			−2.496***	(0.303)	−2.368***	(0.308)
Socialist legacy					−0.358*	(0.197)
EC × LCE × Socialist legacy					−0.151	(0.186)
Observations	130		130		130	
R ²	0.4872		0.7642		0.7736	

Notes: Dependent variable: $\hat{\mu}_i$ as a proxy for countries' long-term income levels. *, **, *** indicate 10, 5, 1% significance levels. Bootstrap standard errors in parentheses. Constant included but not reported. The Western democracies and Japan region is omitted and serves as a reference in the second-step regressions.

That improvements in the legal quality of single institutions can produce negative income effects, constitutes a burden for policymakers who aim for legal reforms to achieve economic development. Understanding the relationships and interactions between formal and informal institutions is crucial, as already suggested by existing literature: Partial legal reforms can foil systems of incentives and constraints and enhance market distortions (Diwan et al., 2019). Transforming a relation-based system into a legal-based system conducive to private investment may take time and fail if old beliefs, expectations, norms and practices do not change (Dixit, 2011). Consequently, at low levels of economic development, heterodox approaches including reforms off the legal path may be more effective than attempts to replicate (parts of) the legal institutional set-ups of high-income countries. It is the function, not the form, of institutions that matters (Rodrik et al., 2004).

6. Robustness

The baseline results are robust to a number of estimation exercises. Section 6.1 presents the estimation results of a benchmark model that ignores the interaction term and two extended models that control for region fixed effects and countries' socialist legacy. Section 6.2 presents the results of an instrumental variable (IV) estimation, two reduced form estimations, and a “false positive” estimation exercise to address endogeneity between income and institutions. Section 6.3 shows that the results are robust to using an alternative measure of legal contracting institutions that allows extending the study period to 2000–2018. Section 6.4 shows that the results are robust to substituting $\hat{\mu}_i$ with $\ln(\bar{y}_i)$ as the dependent variable in the second-step estimation and also to running first-difference (FD) models.

6.1. Restricted and extended models

In their assessment of the relative importance of legal property rights and contracting institutions, Acemoglu and Johnson (2005) rely on the estimates of individual effects. In a restricted model, I estimate the first-step and second-step models considering only individual effects to evaluate how much the γ_1 and γ_2 estimates change when assuming $\gamma_3 = 0$. Columns (1) in Table 5 show that the estimates remain quantitatively and qualitatively similar. A one standard deviation increase in executive constraints increases long-term income levels by 22%, a one standard deviation increase in the efficiency of legal contract enforcement increases long-term income levels by 30%. The results section has revealed that these estimates are *only* accurate for 20 countries with a +− quality combination and for countries with mean values of the other type of institutions.

Rich and poor countries are clustered in world regions. Countries from the same world region share similarities in institutions, geographic and climate conditions, contemporary and historical political events, and cultural traits, i.e., the fundamental causes of growth. In an extended model, I include dummies that control for region-specific effects. This enables to analyze the marginal effects of legal property rights and contracting institutions in a more homogeneous setting. Columns (2) in Table 5 show the results. Region-specific effects absorb half of the effects of legal property rights institutions, legal contracting institutions, and their interaction term as compared to the baseline results. Yet, there are still significant individual and interaction effects left to explain variation in income levels within the regions.

Columns (3) present the results of a model that takes account of countries' socialist legacy. During socialism, countries have developed distinctive systems of formal and informal institutions that have proven highly persistent and complicate the transition

Table 6
Results of models tackling endogeneity issues.

	2nd step: BE					
	(1)		(2)		(3)	
	Coeff.	SE	Coeff.	SE	Coeff.	SE
Executive Constraints	1.565***	(0.333)	0.364***	(0.135)	0.370***	(0.110)
Legal Contract Enforcement	0.629***	(0.238)	0.527***	(0.132)	0.498***	(0.010)
EC × LCE	0.999***	(0.365)	0.193*	(0.120)	0.326***	(0.095)
Investment (% of GDP)	0.008	(0.028)	0.002	(0.016)	0.014	(0.012)
Population (per sqkm)	0.000	(0.001)	0.000	(0.001)	0.001	(0.000)
Trade (% of GDP)	0.005	(0.004)	0.004	(0.003)	0.003	(0.002)
Years of schooling	0.066	(0.079)	0.147**	(0.065)	0.186***	(0.053)
Cultural fractionalization	−0.259	(0.715)	−1.071**	(0.540)	−1.158***	(0.446)
Observations	114		101		130	
(Centered) R ²	0.5901		0.4988		0.5378	
Kleibergen–Paap rk LM statistic	10.249					
Chi-sq(2) P-val	0.0060					
Hansen J statistic	0.031					
Chi-sq(1) P-val	0.8612					

Notes: Dependent variable: $\hat{\mu}_i$, as a proxy for countries' long-term income levels obtained in the respective first-step regressions. *, **, *** indicate 10, 5, 1% significance levels. Bootstrap standard errors in parentheses. Constant included but not reported. In (1), the institution variables are instrumented with past values. In (2), values of the year 2005 are used for the right-hand side variables. In (3), values of the first observed year for each country are used.

to functioning market economies. In the presence of these institutions and country-specific factors, (former) socialist countries have evolved different modes of capitalism with some being more and some being less conducive to growth (Rougier and Combarrous, 2017). To take account of the effects of a socialist history, I control for differences in the long-term income levels and allow the interaction effects to vary between a group of 53 sample countries that have experienced a socialist period and a group of 77 sample countries that have not. While I find that (former) socialist countries on average have a lower long-term income level, I do not find a significant difference in the interaction effects. This suggests that the estimates of the interaction effects are not driven by socialist countries. Complementarities among property rights and contracting institutions matter for countries with a socialist legacy as much as for countries without.

6.2. Endogeneity

Endogeneity between income and institutions is a critical issue that makes a causal interpretation of models (4) and (5) problematic. What is more, reverse causality may bias the estimation results presented above. For endogeneity troubles cross-country studies on economic development, empirical literature has shifted to single country analysis and context-specific identification techniques. Yet, if the determinants of interest are country-specific and persistent, the possibilities to work around cross-country studies are limited. Property rights and contracting institutions fall into this category. I cannot guarantee that the presented estimates are not upward biased due to reverse causality but I can present the results of three robustness test to convince the reader that the results are not driven by reversed causality and that there is substantial influence running from institutions to income, as it is suggested in Acemoglu et al. (2001, 2005), Rodrik et al. (2004), and Fatás and Mihov (2013).

I start with presenting the results of an IV estimation. I follow Voigt and Gutmann (2013) in using past institutions as instruments for present institutions. Besides for what Albouy (2012) puts forward against the use of colonial history data, I use past institutions because there exists panel data. Panel data allows me to construct $m > k$ instruments for k endogenous institutional variables and perform an overidentification test on whether the instruments are valid, i.e., uncorrelated with the error term. Also, colonial history data fails to reject the null hypothesis that the equation is underidentified. This suggests that while colonial history data may be a valid instrument for a sample of ex-colony countries, it is a poor instrument for the underlying world sample.¹⁰ Table A.3 in the Appendix provides summary statistics for the instruments. Table A.4 in the Appendix presents the results of the first-stage IV estimation. The institutional variables show a strong correlation over time, which confirms the persistence of institutions.

Columns (1) in Table 6 present the results of the second-stage IV estimation. The individual and interaction effects of the two types of institutions remain economically and statistically significant. The increase in the size of the estimates is in accordance with Acemoglu and Johnson (2005) and Voigt and Gutmann (2013). The Kleibergen–Paap rk LM and Sargan statistics in the lower part of Table 6 provide information on whether the equation is identified. The null hypothesis can be rejected, which indicates that the instruments are relevant. The Hansen J statistic is a test of overidentifying restrictions. I cannot reject the joint null hypothesis that the instruments are valid and that the excluded instruments are correctly excluded from the structural equation. Columns (2) and (3) present the results when estimating (5) using first-period values rather than mean values, which constitutes a reduced form

¹⁰ See Eicher and Leukert (2009) on parameter heterogeneity and the suitability of different sets of instruments for institutions dependent on the underlying sample.

Table 7
Results of the “false positive” estimation exercise.

	2nd step: BE					
	(1)		(2)		(3)	
	Coeff.	SE	Coeff.	SE	Coeff.	SE
Executive Constraints	0.257*	(0.141)	0.352**	(0.146)	0.373***	(0.141)
Legal Contract Enforcement	0.546***	(0.119)	0.483***	(0.120)	0.100	(0.221)
Legal Enforcement of Contracts					0.460**	(0.196)
LCE × LEC	0.118	(0.094)	−0.035	(0.110)	−0.036	(0.105)
EC × LCE			0.391***	(0.144)	0.420***	(0.138)
Investment (% of GDP)	−0.003	(0.018)	0.005	(0.017)	−0.001	(0.018)
Population (per sqkm)	0.000	(0.001)	0.000	(0.000)	0.000	(0.000)
Trade (% of GDP)	0.008***	(0.003)	0.006**	(0.003)	0.006**	(0.003)
Years of schooling	0.178***	(0.055)	0.177***	(0.054)	0.181***	(0.054)
Cultural fractionalization	−1.472***	(0.468)	−1.218***	(0.483)	−1.209***	(0.478)
Observations	124		124		124	
R ²	0.4938		0.5302		0.5545	

Notes: Dependent variable: $\hat{\mu}_i$ as a proxy for countries' long-term income levels. *, **, *** indicate 10, 5, 1% significance levels. Bootstrap standard errors in parentheses. Constant included but not reported.

of the IV estimation approach. The results presented in columns (2) refer to a specification using values of the year 2005. The results presented in columns (3) refer to a specification using values of countries' first observed year and considering the same set of sample countries as the baseline second-step model.

Rich (poor) countries tend to be organized by institutions with good (bad) legal qualities throughout different domains of the society. If the positive and significant estimates of the interaction term are driven by reverse causality, we should obtain positive estimates of the interaction of any two institutional measures positively correlated with income, even for those for which there should be none. One interaction for which we do not want to see an effect is the interaction of two alternative measures of the *same* institution. Fortunately, the Fraser Institute provides an alternative measure of legal contracting institutions that is close but different enough to the baseline measure in order to conduct this robustness test. A positive and significant estimate of the interaction of two alternative measures of legal contracting institutions would hint to the presence of severe reversed causality. Such a result would cast doubts on previous findings.

Table 7 presents the results. $LCE \times LEC$ is the interaction term of the two measures of legal contracting institutions and the variable of interest in this endogeneity test. Columns (1) show that its estimate remains insignificant if included into the second-step model instead of the baseline interaction term of the two different types of institutions. Columns (2) show that its estimate remains insignificant if included alongside the baseline interaction term while the baseline interaction term turns significant and positive. Columns (3) show that its estimate also remains insignificant if included alongside the baseline interaction term and the two alternative measures of contracting institutions. As two alternative measures of legal contracting institutions are included in the model, the effects load on one. Here, the effects load on the alternative measure of legal contracting institutions provided by the Fraser Institute. The results of this estimation exercise are in favor of previous results. Reverse causality may eventually amplify the estimates but they do not drive the estimates.

6.3. Longer time period

Information on the quality of legal contract enforcement was collected on a yearly basis only after the turn of the second millennium. This restricts the analysis to be carried out on a short panel. The short study period constrains especially the first-step estimation for there was little change in institutions over 2005–2015 and fixed effects absorb much of their explanatory power. To enlarge the period of analysis, I draw on the alternative measure of legal contracting institutions introduced in the previous section which has records for some more years. Using Fraser Institute's *Legal Enforcement of Contracts* variable instead of the baseline measure allows extending the study period to 2000–2018. Table A.5 in the Appendix reports the summary statistics for the longer time period regressions using the alternative legal contracting institutions measure. The summary statistics look similar to those of the main regressions. The main difference is that the number of observations increases by 80% from 1246 to 2260.

Table 8 presents the results of the baseline models using 2000–2018 data. Columns (1) show the estimates for the first-step estimation and columns (2) for the second-step estimation. The estimates support previous finding. Moreover, extending the time period increases within variation in the institutional measures and allows detecting short-term income effects of institutional changes. Yet, the short-term effects are fully conditional. Legal reforms in one type of institutions produce short-term income effects *only* for countries with a high legal quality of the other type. In the second-step estimation, the estimates of the institutional measures remain positive and highly significant. The magnitudes of the estimates change slightly, which is also due to using an alternative measure of legal contracting institutions.

Table 8
Results of longer time period regressions.

	1st step: FE		2nd step: BE	
	(1)		(2)	
	Coeff.	SE	Coeff.	SE
Executive Constraints	−0.007	(0.017)	0.219*	(0.117)
Legal Enforcement of Contracts	0.038	(0.025)	0.652***	(0.089)
EC × LEC	0.023*	(0.013)	0.304***	(0.101)
Investment (% of GDP)	0.003	(0.002)	0.001	(0.015)
Population (per sqkm)	−0.000	(0.000)	0.000	(0.000)
Trade (% of GDP)	−0.001	(0.001)	0.007***	(0.002)
Years of schooling	0.013*	(0.007)	0.157***	(0.046)
Cultural fractionalization			−1.511***	(0.399)
Observations	2260		130	
(Centered) R ²	0.5686		0.5886	

Notes: Dependent variables: *Log real income per capita* in the first-step regression in (1) and $\hat{\mu}_i$ in the second-step regression in (2). *, **, *** indicate 10, 5, 1% significance levels. Robust standard errors in parentheses for (1) and bootstrap standard errors in parentheses for (2). Constants are included in both steps but not reported. The first-step FE regression controls for time and country-specific constant factors.

Table 9
Results of alternative models.

	2nd step: BE		FD models			
	(1)		(2)		(3)	
	Coeff.	SE	Coeff.	SE	Coeff.	SE
Executive Constraints	0.372***	(0.120)	0.007	(0.006)	0.004	(0.005)
Legal Enforcement of Contracts	0.523***	(0.101)	0.028***	(0.009)	0.005	(0.009)
EC × LEC	0.370***	(0.096)	0.009**	(0.005)	0.006	(0.004)
Investment (% of GDP)	0.013	(0.016)	0.002***	(0.001)	0.001**	(0.000)
Population (per sqkm)	0.000	(0.000)	−0.000**	(0.000)	−0.000	(0.000)
Trade (% of GDP)	0.005*	(0.003)	−0.000	(0.000)	0.001***	(0.000)
Years of schooling	0.189***	(0.049)	0.005**	(0.002)	0.001	(0.002)
Cultural fractionalization	−1.239***	(0.456)				
Log real income per capita, $t - 1$					0.739***	(0.029)
Observations	130		1982		1840	
R ²	0.5481		0.0470		0.5850	

Notes: Dependent variable: $\ln(\bar{y}_i)$ in the second-step regression in (1) and $\Delta \ln(y_{it})$ in the FD regressions without a lagged dependent variable in (2) and with a lagged dependent variable in (3). *, **, *** indicate 10, 5, 1% significance levels. Bootstrap standard errors in parentheses for (1) and robust standard errors in parentheses for (2) and (3). Constants are included in both steps but not reported.

6.4. Alternative models

I am confident that the models presented in Section 3.2 are well chosen to investigate how property rights and contracting institutions combine for growth. I am also confident that the income and institutional measures presented in Section 4 are good choices given the alternative measures that are available. Yet, in a last set of robustness tests, I want to address some concerns with the chosen proxy for long-term income levels and the estimation approach.

Islam (1995) provides a theoretical foundation and empirical work for estimating μ_i in a first step and using it as a dependent variable to study cross-country differences in long-term income levels in a second step. When estimating unit fixed effects, having a short panel is not ideal. A small T may bias estimates. The problem of small T becomes less severe the more groups are included in the analysis and the more inert the dependent variable is. Having real GDP per capita levels observed for 130 countries alleviates the threat of small T -bias in the μ_i estimates. Yet, to give consideration to the possibility of a small T -bias, I ran an estimation using $\ln(\bar{y}_i)$ as the dependent variable in the second-step model. Columns (1) in Table 9 present the results of this model, which coincides with the one of Acemoglu and Johnson (2005) and which has become the conventional model for cross-country analyses on the role of institutions for economic development.

Moreover, I present the estimates of first-difference models without a lagged dependent variable in columns (2) and with a lagged dependent variable in columns (3). Similar to the FE estimator, the FD estimator removes the effects of unobserved time-invariant variables and therefore reduces the threat of omitted variable bias. In contrary to the FE estimator, it does not require strict exogeneity between the error term and the explanatory variables. Yet, it does not allow to estimate the effects of time-invariant variables. This is a problem when the research interest lies on the effects of institutions, which tend to be persistent. To alleviate this problem, I use the FD estimator on the longer time period dataset with the alternative legal contracting institutions measure as this increases the extent of within variation of the institutional measures.

Columns (1) show that the estimates remain quantitatively and qualitatively unchanged when using $\ln(\bar{y}_i)$ as dependent variable rather than $\hat{\mu}_i$. The results of the FD model presented in columns (2) support the finding that short-term variation in income levels can be explained with input factor accumulation. This model also suggests positive individual and interaction effects of increases in the quality of legal contracting institutions. The effects of increases in the quality of legal property rights institutions are fully conditional on the quality of legal contracting institutions. However, controlling for last period's income levels, the effects of institutions vanish while the effects of increases in the saving rate remain significant. The results of the alternative models are in line with the findings of the baseline models and the Solow growth theory. Short-term variation in income levels can be explained by capital accumulation. Long-term economic development is determined by the $A(0)$ factors, including institutions.

7. Limitations

The short study period is unfavorable for identifying short-term income effects of institutional changes in the first-step estimation. A longer time period would help. For the analysis of long-term income levels that exploits cross-country variation and forms the heart of the analysis, I am confident that the short study period is no serious constraint. My confidence bases on the strong serial correlation of GDP per capita levels, which can be seen from comparing columns (2) and (3) in Table 9. That great parts of GDP per capita levels are determined by constant factors is also suggested by the almost perfect correlation between $\ln(y_{it})$ and the unobserved heterogeneity term μ_i . Literature provides abundant empirical evidence on the persistence of geography and climate conditions, institutions, and cultural traits. The majority of factors that enter μ_i in 2005–2015 almost certainly persist for decades, eventually centuries. I am convinced that a longer study period would not change the estimates of μ_i and therefore the results of the second-step analysis significantly.

What remains a threat is the possibility of omitted variable bias. Countries' income levels and legal qualities of property rights and contracting institutions may be affected by other fundamental causes of growth captured in growth theory's $A(0)$ term. Moreover, the underlying growth theory assumes that technological progress is a public good to be equally shared and g is the same for all countries. This may not be true. Institutions that determine $A(0)$ may also determine its growth rate g . Partialling out the effects of constant factors in a first step and then estimating the effects of institutions on the constant factors in a second step does not take account of this contingency. To take account of these issues, we need more theoretical and empirical work on the coevolution process of economic outcomes, institutions, and other fundamental causes of growth.

This study limits its scope to two institutions. Two institutions is enough to provide empirical evidence for the existence of institutional complementarities and alternative institutional set-ups. Two institutions is not enough to capture all important institutional interrelations and describe countries' overall institutional set-ups. Econometric approaches do not allow to put estimates on the relationships of a large number of institutions. The Varieties of Capitalism literature uses cluster analyses to study the mutual presence of many institutions throughout different domains of the society. This study should be relevant for this literature. Property rights and contracting institutions are pervasive. Their qualities matter for the economic decisions in many if not all domains. Countries with more similar quality combinations of property rights and contracting institutions should be characterized by more similar institutional features throughout the different domains.

8. Conclusion

Motivated by Acemoglu and Johnson (2005), this study has reevaluated the effects of legal property rights and contracting institutions on economic development. Unlike previous literature that assumes independent effects, this study has considered that the two different types of institutions may be effective in their combination. The underlying argument is that property rights and contracting institutions provide interrelated incentives and constraints on private investment and thereby jointly determine the accumulation of physical and human capital as well as the utilization of existing and adoption of new technologies.

This study provides empirical evidence that property rights and contracting institutions affect countries' long-term income levels as fundamental causes of growth entering growth theory's $A(0)$ term, which is country-specific. The assumption of independent effects is too strong. Interaction effects cause a spread between individual and marginal effects of legal reforms in one type of institutions, occasionally to a substantial degree. Moreover, the study reveals that the quality fit of the two types of institutions is crucial for the size and the direction of marginal effects. For countries with absent or bad legal qualities of both types of institutions, improving the legal quality of only one type produces negative interaction effects. The negative interaction effects can exceed the positive individual effects and reduce income levels if the change is not complemented with a legal reform in the other type of institutions. This applies mostly to countries at lower levels of economic development. The study adds to literature that addresses the difficulties of institution building and institutional change. It helps to get one step closer to understanding why legal reforms in property rights institutions have not produced the hoped-for income effects in many low-income and transition economies.

The policy implications that can be drawn from this study favor deviating from piecemeal reforms that follow best practices and that do not sufficiently take account of institutional complementarities. Instead, institutional reforms should be coordinated within and across different domains of the society and tailored to local challenges, needs, and conditions, including prevalent institutional set-ups. Such an approach, however, requires understanding all important institutionalized rules and practices that put incentives and constraints on economic activities as well as the relationships among them. Understanding the workings of complex institutional systems is a difficult task. As the optimal institutional set-up is country-specific, there are limits to learning from others. Yet, the optimal institutional set-up is more alike for countries facing more similar conditions. Variation in the combinations of different institutional qualities across countries allows to analyze the relationships among as well as the suitability and economic effects of single institutional qualities in a given system. Theoretical and empirical work building on this study may look closer into the workings of different property rights regimes and the institutional qualities they have evolved. This could help us better understand which reforms are growth-conducive under which conditions.

Appendix

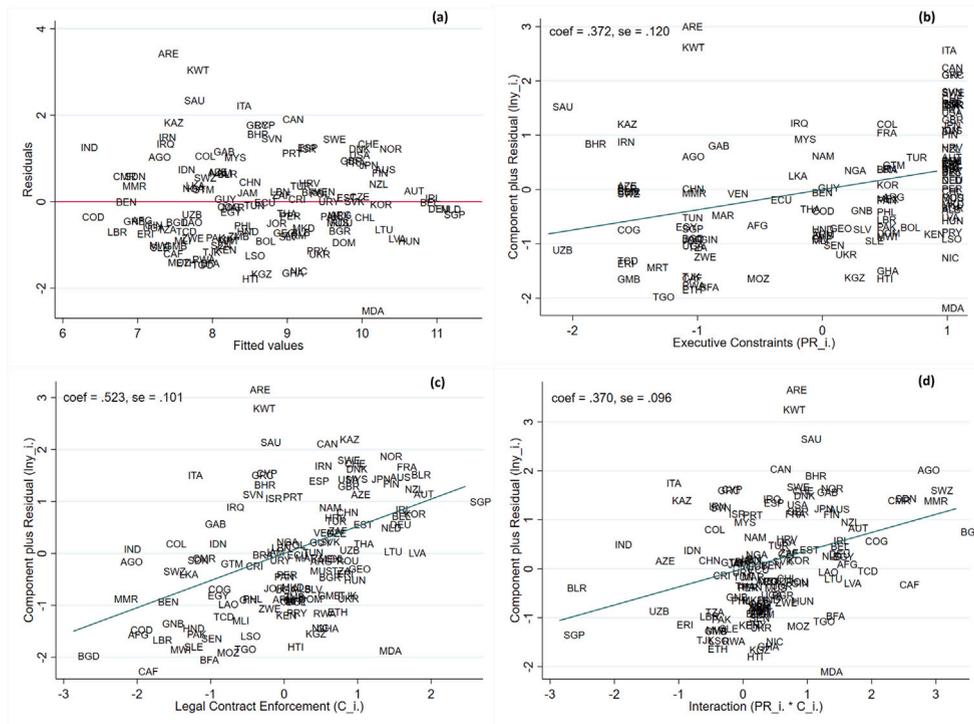


Fig. A.1. Residual plots for the second-step estimation with $\ln(\bar{v}_i)$ as dependent variable.

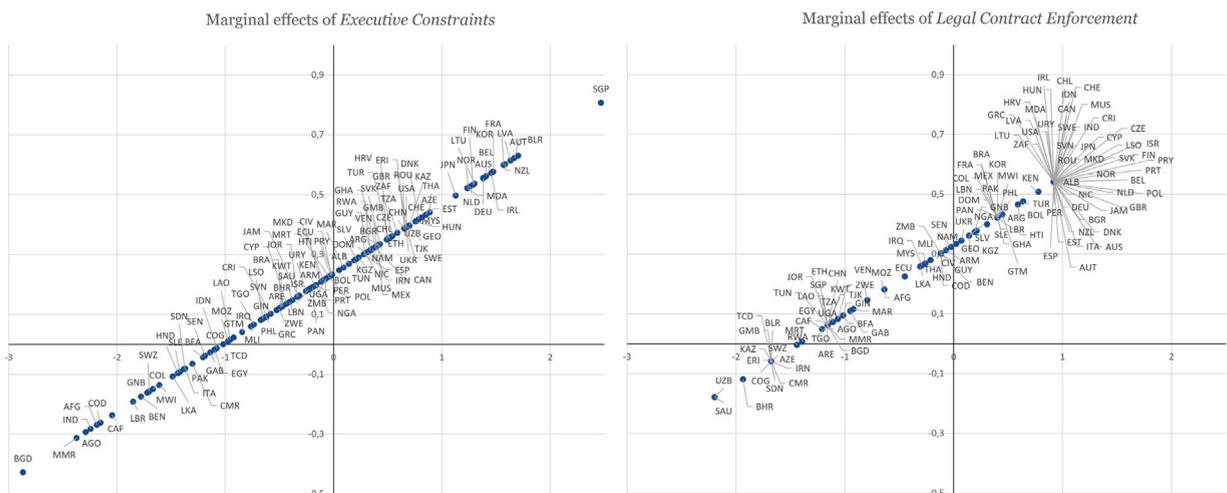


Fig. A.2. The left diagram plots the estimated marginal effects of a one standard deviation increase in the quality of *Executive Constraints* and the right diagram plots the estimated marginal effects of a one standard deviation increase in the quality of *Legal Contract Enforcement* for each of the 130 sample countries. The marginal effects are calculated using the second-step baseline model coefficient estimates as presented in Table 2 columns (2) and the 2005–2015 averaged ratings of institutions qualities for each country as presented in Table A.1. The y-axis gives the marginal effects of the respective institution in scope that varies with the quality level of the other type of institutions that is given on the x-axis.

Table A.1
Sample composition.

ID	Countryname	\bar{y}_i	$\hat{\mu}_i$	\overline{PR}_i	\bar{C}_i	$\overline{PR}_i \times \bar{C}_i$	\mathcal{D}_{qq}	FrLeg	SoLeg	Region
AFG	Afghanistan	625	-2.169	-0.638	-2.184	1.393	--	1	1	Asia
AGO	Angola	3396	-0.306	-1.157	-2.288	2.648	--	1	1	SSA
ALB	Albania	3976	-0.332	0.920	0.052	0.048	++	1	0	EE+fSU
ARE	UAE	42187	2.214	-1.157	-0.525	0.608	--	0	0	NA+ME
ARG	Argentina	10014	0.513	0.448	0.298	0.133	++	0	1	LA+C
ARM	Armenia	3392	-0.521	-0.119	-0.217	0.026	--	1	0	EE+fSU
AUS	Australia	52193	2.114	0.920	1.382	1.271	++	0	0	WD+J
AUT	Austria	47191	2.095	0.920	1.705	1.568	++	0	0	WD+J
AZE	Azerbaijan	5307	0.011	-1.676	0.852	-1.428	-+	1	0	EE+fSU
BEL	Belgium	44318	2.070	0.920	1.408	1.295	++	0	1	WD+J
BEN	Benin	772	-1.835	-0.119	-1.776	0.211	--	1	1	SSA
BFA	Burkina Faso	583	-2.246	-1.016	-1.205	1.224	--	0	1	SSA
BGD	Bangladesh	947	-1.680	-1.157	-2.868	3.319	--	1	0	Asia
BGR	Bulgaria	6786	0.218	0.920	0.415	0.382	++	1	0	EE+fSU
BHR	Bahrain	21393	1.561	-1.936	-0.466	0.902	--	0	0	NA+ME
BLR	Belarus	5769	0.115	-1.676	1.672	-2.803	-+	1	0	EE+fSU
BOL	Bolivia	2022	-1.012	0.589	-0.034	-0.020	+-	0	1	LA+C
BRA	Brazil	11611	0.583	0.400	-0.489	-0.196	+-	0	1	LA+C
CAF	Centr. Afr. Rep.	440	-2.456	-1.157	-2.045	2.366	--	0	1	SSA
CAN	Canada	48485	2.135	0.920	0.385	0.354	++	0	0	WD+J
CHE	Switzerland	74584	2.582	0.920	0.763	0.701	++	0	0	WD+J
CHL	Chile	13167	0.789	0.920	0.499	0.459	++	0	1	LA+C
CHN	China	6302	-0.015	-1.157	0.654	-0.757	-+	1	0	Asia
CIV	Cote d'Ivoire	1306	-1.445	-0.119	-0.117	0.014	--	0	1	SSA
CMR	Cameroon	1338	-1.331	-1.676	-1.303	2.184	--	0	1	SSA
COD	Congo. Dem. Rep.	349	-2.600	-0.119	-2.150	0.255	--	0	1	SSA
COG	Congo. Rep.	2686	-0.627	-1.676	-1.092	1.831	--	1	1	SSA
COL	Colombia	6397	0.190	0.400	-1.667	-0.668	+-	0	1	LA+C
CRI	Costa Rica	8252	0.415	0.920	-0.581	-0.534	+-	0	1	LA+C
CYP	Cyprus	28982	1.721	0.920	-0.434	-0.399	+-	0	0	NA+ME
CZE	Czech Rep.	19883	1.324	0.920	0.516	0.475	++	1	0	EE+fSU
DEU	Germany	42606	1.957	0.920	1.381	1.270	++	1	0	LA+C
DNK	Denmark	59263	2.376	0.920	0.786	0.723	++	0	0	WD+J
DOM	Dominican Rep.	5424	-0.076	0.400	0.142	0.057	++	0	1	LA+C
ECU	Ecuador	4831	-0.179	-0.449	-0.020	0.009	--	0	1	LA+C
EGY	Egypt	2471	-0.749	-1.209	-1.099	1.329	--	1	1	NA+ME
ERI	Eritrea	492	-2.323	-1.676	0.656	-1.100	-+	1	1	SSA
ESP	Spain	30738	1.681	0.920	0.279	0.256	++	0	1	WD+J
EST	Estonia	16243	1.101	0.920	0.884	0.813	++	1	0	EE+fSU
ETH	Ethiopia	424	-2.639	-1.157	0.533	-0.617	-+	1	1	SSA
FIN	Finland	46526	2.069	0.920	1.288	1.185	++	0	0	WD+J
FRA	France	41056	1.927	0.400	1.474	0.590	++	0	1	WD+J
GAB	Gabon	9105	0.527	-0.950	-1.138	1.081	--	0	1	SSA
GBR	United Kingdom	40021	1.944	0.920	0.670	0.616	++	0	0	WD+J
GEO	Georgia	3087	-0.587	0.023	0.816	0.019	++	1	0	EE+fSU
GHA	Ghana	1377	-1.395	0.400	0.386	0.155	++	1	0	SSA
GIN	Guinea	665	-2.006	-1.016	-0.670	0.680	--	1	1	SSA
GMB	Gambia. The	533	-2.281	-1.676	0.397	-0.666	-+	0	0	SSA
GNB	Guinea-Bissau	546	-2.201	0.193	-1.719	-0.331	+-	1	1	SSA
GRC	Greece	26094	1.600	0.920	-0.504	-0.464	+-	0	1	WD+J
GTM	Guatemala	2861	-0.587	0.448	-0.922	-0.413	+-	0	1	LA+C
GUY	Guyana	3081	-0.459	-0.072	0.285	-0.020	-+	1	0	LA+C
HND	Honduras	1966	-0.932	-0.119	-1.438	0.171	--	0	1	LA+C
HRV	Croatia	13847	0.937	0.920	0.493	0.453	++	1	0	EE+fSU
HTI	Haiti	703	-1.961	0.400	-0.012	-0.005	+-	0	1	LA+C
HUN	Hungary	13567	0.915	0.920	0.757	0.696	++	1	0	EE+fSU
IDN	Indonesia	3760	-0.449	0.920	-1.077	-0.991	+-	0	1	Asia
IND	India	1702	-1.132	0.920	-2.241	-2.061	+-	1	0	Asia
IRL	Ireland	52489	2.278	0.920	1.456	1.339	++	0	0	WD+J
IRN	Iran. Islam. Rep.	6158	0.105	-1.676	0.357	-0.599	-+	0	1	NA+ME
IRQ	Iraq	5059	-0.005	-0.292	-0.844	0.246	--	1	1	NA+ME
ISR	Israel	30617	1.719	0.920	-0.316	-0.290	+-	0	0	NA+ME
ITA	Italy	35863	1.918	0.920	-1.368	-1.259	+-	0	1	WD+J
JAM	Jamaica	4829	-0.011	0.920	-0.327	-0.301	+-	0	0	LA+C
JOR	Jordan	3546	-0.365	-1.157	-0.335	0.388	--	0	1	NA+ME

(continued on next page)

Table A.1 (continued).

ID	Countryname	\bar{y}_i	$\hat{\mu}_i$	\overline{PR}_i	\bar{C}_i	$\overline{PR}_i \times \bar{C}_i$	\mathcal{D}_{qq}	FrLeg	SoLeg	Region
JPN	Japan	46824	1.982	0.920	1.129	1.038	++	0	0	WD+J
KAZ	Kazakhstan	9197	0.552	-1.676	0.699	-1.171	-+	1	0	EE+fSU
KEN	Kenya	977	-1.712	0.778	-0.169	-0.131	+ -	0	0	SSA
KGZ	Kyrgyz Rep.	894	-1.767	0.141	0.233	0.033	++	1	0	EE+fSU
KOR	Korea. Rep.	21895	1.362	0.400	1.570	0.629	++	0	0	Asia
KWT	Kuwait	42157	2.134	-1.157	-0.481	0.556	--	0	1	NA+ME
LAO	Lao PDR	1167	-1.458	-1.157	-0.954	1.104	--	1	0	Asia
LBN	Lebanon	7684	0.399	0.400	-0.232	-0.093	+ -	0	1	NA+ME
LBR	Liberia	345	-2.536	0.400	-1.849	-0.740	+ -	0	0	SSA
LKA	Sri Lanka	2876	-0.613	-0.308	-1.485	0.457	--	1	0	Asia
LSO	Lesotho	1254	-1.378	0.920	-0.652	-0.600	+ -	0	0	SSA
LTU	Lithuania	13018	0.866	0.920	1.301	1.196	++	1	0	EE+fSU
LVA	Latvia	12685	0.752	0.920	1.635	1.504	++	1	0	EE+fSU
MAR	Morocco	2823	-0.660	-0.921	0.092	-0.085	- +	0	1	NA+ME
MDA	Moldova	1676	-1.209	0.920	1.236	1.137	++	1	0	EE+fSU
MEX	Mexico	9627	0.388	0.400	0.412	0.165	++	0	1	LA+C
MKD	Macedonia	4481	-0.204	0.920	-0.093	-0.086	+ -	1	0	EE+fSU
MLI	Mali	692	-2.021	-0.119	-0.762	0.091	--	1	1	SSA
MMR	Myanmar	1225	-1.502	-1.157	-2.373	2.746	--	1	0	Asia
MOZ	Mozambique	382	-2.505	-0.638	-0.972	0.620	--	1	1	SSA
MRT	Mauritania	1237	-1.459	-1.440	-0.042	0.061	--	1	1	SSA
MUS	Mauritius	7964	0.404	0.920	0.293	0.270	++	0	1	SSA
MWI	Malawi	441	-2.397	0.400	-1.608	-0.644	+ -	0	0	SSA
MYS	Malaysia	9242	0.679	-0.260	0.784	-0.204	- +	0	0	Asia
NAM	Namibia	5264	0.026	-0.119	0.418	-0.050	- +	0	0	SSA
NGA	Nigeria	2563	-0.827	0.141	-0.162	-0.023	+ -	0	0	SSA
NIC	Nicaragua	1609	-1.194	0.920	0.317	0.292	++	1	1	LA+C
NLD	Netherlands	50423	2.211	0.920	1.261	1.160	++	0	1	WD+J
NOR	Norway	89185	2.693	0.920	1.244	1.144	++	0	0	WD+J
NZL	New Zealand	34482	1.722	0.920	1.584	1.457	++	0	0	WD+J
PAK	Pakistan	1126	-1.642	0.400	-1.381	-0.553	+ -	0	0	Asia
PAN	Panama	8469	0.432	0.400	-0.194	-0.078	+ -	0	1	LA+C
PER	Peru	5000	-0.171	0.920	-0.161	-0.148	+ -	0	1	LA+C
PHL	Philippines	2164	-0.850	0.400	-0.620	-0.248	+ -	0	1	Asia
POL	Poland	12491	0.829	0.920	-0.043	-0.040	+ -	1	0	EE+fSU
PRT	Portugal	22083	1.425	0.920	-0.076	-0.069	+ -	1	1	WD+J
PRY	Paraguay	3214	-0.534	0.920	-0.026	-0.024	+ -	0	1	LA+C
ROU	Romania	8424	0.370	0.920	0.654	0.601	++	1	0	EE+fSU
RWA	Rwanda	568	-2.191	-1.157	0.337	-0.390	- +	0	1	SSA
SAU	Saudi Arabia	20090	1.347	-2.196	-0.380	0.834	--	0	0	NA+ME
SDN	Sudan	1563	-1.228	-1.676	-1.373	2.302	--	0	0	SSA
SEN	Senegal	998	-1.693	-0.024	-1.186	0.029	--	1	1	SSA
SGP	Singapore	46412	2.969	-1.157	2.469	-2.857	- +	0	0	Asia
SLE	Sierra Leone	426	-2.473	0.306	-1.418	-0.434	+ -	1	0	SSA
SLV	El Salvador	3069	-0.544	0.212	0.212	0.045	++	0	1	LA+C
SVK	Slovak Republic	16467	1.162	0.920	0.427	0.393	++	1	0	EE+fSU
SVN	Slovenia	23452	1.566	0.920	-0.622	-0.572	+ -	1	0	EE+fSU
SWE	Sweden	52461	2.244	0.920	0.667	0.613	++	0	0	WD+J
SWZ	Eswatini	3815	-0.245	-1.676	-1.702	2.853	--	0	0	SSA
TCD	Chad	875	-1.811	-1.676	-1.021	1.711	--	1	1	SSA
TGO	Togo	499	-2.326	-1.393	-0.735	1.024	--	0	1	SSA
THA	Thailand	5059	-0.023	-0.213	0.888	-0.189	- +	0	0	Asia
TJK	Tajikistan	770	-1.930	-1.157	0.685	-0.792	- +	1	0	EE+fSU
TUN	Tunisia	3960	-0.243	-1.157	0.192	-0.222	- +	1	1	NA+ME
TUR	Turkey	11422	0.668	0.636	0.508	0.324	++	0	1	NA+ME
TZA	Tanzania	712	-2.017	-1.110	0.589	-0.654	- +	1	0	SSA
UGA	Uganda	608	-2.166	-1.157	-0.183	0.211	--	0	0	SSA
UKR	Ukraine	3041	-0.591	0.070	0.670	0.047	++	1	0	EE+fSU
URY	Uruguay	11715	0.699	0.920	-0.254	-0.233	+ -	0	1	LA+C
USA	United States	51403	2.017	0.920	0.672	0.618	++	0	0	WD+J
UZB	Uzbekistan	1396	-1.364	-2.196	0.696	-1.528	- +	1	0	EE+fSU
VEN	Venezuela	13831	0.844	-0.794	0.341	-0.270	- +	0	1	LA+C
ZAF	South Africa	7276	0.258	0.920	0.544	0.500	+ +	0	0	SSA
ZMB	Zambia	1561	-1.273	-0.119	-0.069	0.008	--	1	0	SSA
ZWE	Zimbabwe	802	-1.794	-1.063	-0.407	0.433	--	0	0	SSA

Table A.2

Definitions of variables and sources of data.

Variable	Description
Log real per capita income	Logarithm of GDP per capita in constant 2010 US Dollars. <i>Source:</i> World Bank national accounts data and OECD national accounts data files.
Executive Constraints	The extent of institutionalized constraints on the decision-making powers of executives imposed by any accountability groups. Originally on a seven-category scale, where (1) Unlimited Authority, (3) Slight to Moderate Limitation on Executive Authority, (5) Substantial Limitations on Executive Authority, (7) Executive Parity or Subordination, and (2), (4) and (6) are Intermediary Categories. I demean the scores and divide them by the variable's standard deviation. <i>Sources:</i> Polity IV's and Polity V's <i>xconst</i> variable, following Eckstein and Gurr (1975) .
Legal Contract Enforcement	Index measuring the efficiency and quality of commercial dispute resolution. It considers cases where the value of the claim is equal to 200% of the economy's income per capita or US\$5000, whichever is greater. The original score ranges between 0 and 100 and is a simple average of the scores for each of the three component variables (time, cost, quality of judicial process). The methodology builds up on Djankov et al. (2003) . I multiply the index scores by 0.1 to obtain values between 0 and 10. I then demean the values and divide them by the variable's standard deviation. <i>Source:</i> World Bank Doing Business's <i>Enforcing contracts (DB04-15 methodology)</i> - Score. Data bases on studies of codes of civil procedure, court regulations, questionnaires completed by local litigation lawyers and judges.
Legal Enforcement of Contracts	Index measuring the efficiency of collecting a commercial debt equal 200 percent of the country's per capita income or worth US\$5000, whichever is greater. Time cost and monetary costs are considered. The former is measured in number of calendar days required from the moment the lawsuit is filed until payment, the later as a percentage of the debt. The formula used to calculate the ratings is $(V_{max} - V_i)/(V_{max} - V_{min})$ multiplied by 10. V_i represents the time or money cost value. The values for V_{max} and V_{min} are set at 725 days and 82.3 percent (1.5 standard deviations above average in 2005) and 62 days (1.5 standard deviations below average in 2005) and 0 percent, respectively. Countries with values outside the range from V_{min} and V_{max} receive ratings of either 0 or 10, accordingly. The two scores get averaged into one. I demean the variable's scores and divide them by the variable's standard deviation. <i>Source:</i> Fraser Institute's <i>Legal Enforcement of Contracts</i> , an indicator of the Economic Freedom index, based on the World Bank's Doing Business estimates.
Investment (% of GDP)	Gross capital formation as percentage of GDP (formerly gross domestic investment). <i>Source:</i> World Bank national accounts data and OECD national accounts data files.
Population (per sqkm)	Population density (people per sq. km of land area), midyear population. <i>Source:</i> Food and Agriculture Organization and World Bank population estimates.
Trade (% of GDP)	Sum of exports and imports of goods and services measured as a share of GDP. <i>Source:</i> World Bank Open Data, data from World Federation of Exchanges database.
Cultural fractionalization	0-to-1 scale index on the structural distance between the languages spoken in a country as a proxy for the cultural distance of a country's population in the year 2003. The score is closer to 0 in countries with one language group or a set of different groups that all speak highly similar languages. The score is closer to 1 in countries with a large number of groups that speak structurally unrelated languages. <i>Source:</i> Fearon (2003) 's <i>cdiv</i> variable.
Years of schooling	Duration of compulsory education (years), that is the number of years children are legally obliged to attend school, 3-year moving averages. <i>Source:</i> World Bank Open Data. Data from the UNESCO Institute for Statistics.
Capital stock/GDP	The sum of private capital stock and general government capital stock weighted by countries' gross domestic product: $(kpriv-rppp + kgov-rppp) / (gdp-rppp)$. The IMF constructs the capital stock variables based on historical investment flows and reports in billions of constant 2011 international dollars. <i>Source:</i> IMF's Investment and Capital Stock Dataset, 1960–2015. Version January 2017.
Quality group (\mathcal{D}_{qq})	4 dummies assigning countries to one of four combinations of above and below sample average quality levels of <i>Executive Constraints</i> and <i>Legal Contract Enforcement</i> .
French legal origin (FrLeg)	Dummy identifying countries that have a French legal origin.
Socialist legacy (SoLeg)	Dummy identifying countries that have experienced a socialist period.
Region	6 dummies assigning countries to one of six world regions. Asia; EE+fSU = Eastern Europe and former Soviet Union; LA+C = Latin America and Caribbean; NA+ME = North Africa and Middle East; SSA = Sub-Saharan Africa; WD+J = Western Democracies and Japan.

Table A.3

Summary statistics for instruments used in the IV estimation.

	Obs	Mean	St.dev	Min	Max
EC1985–1995	989	0.000	1.000	–1.557	1.083
EC1985–1995sq	989	0.000	1.000	–1.258	1.182
CE1995	989	0.000	1.000	–2.566	2.025
EC1985–1995 * CE1995	989	0.000	1.000	–1.988	3.266

Notes on instruments: For *Executive Constraints*, I use ratings for the period 1985–1995 and their square as instruments for the 2005–2015 ratings. Unfortunately, 2000 is the first year in which questions on time, money, and quality of contract enforcement were included in the World Bank's Doing Business survey on a yearly basis. For the years before, the Fraser Institute provides information on the quality of legal contracting institutions in 5-year intervals. Since the coverage of countries decreases rapidly as one goes back in time, I use time-invariant information of 1995 to instrument 2005–2015 legal contracting institutions. To instrument the interaction term, I interact the 1985–1995 ratings for *Executive Constraints* with Fraser Institute's 1995 scores for *Legal Enforcement of Contracts*.

Table A.4
IV estimation results: First stage.

Dependent Variable	Executive Constraints		Legal Contract Enforcement		EC * LCE (2005–2015)	
	Coeff.	SE	Coeff.	SE	Coeff.	SE
Investment (% of GDP)	–0.013	(0.014)	–0.012	(0.010)	–0.009	(0.012)
Population (per sqkm)	–0.000**	(0.000)	–0.000**	(0.000)	–0.000**	(0.000)
Trade (% of GDP)	0.002	(0.002)	0.003**	(0.001)	0.003	(0.003)
Years of schooling	–0.030	(0.036)	0.050	(0.033)	–0.037	(0.058)
Cultural fractionalization	–0.439	(0.348)	–0.242	(0.322)	–0.427	(0.341)
EC1985–1995	–0.380	(0.625)	–0.310	(0.502)	0.884	(0.739)
EC1985–1995square	0.937*	(0.450)	0.238	(0.508)	–0.778	(0.646)
CE1995	0.036	0.081	0.752***	(0.076)	–0.008	(0.126)
EC1985–1995 * CE1995	0.000	(0.085)	0.094	(0.072)	0.529***	(0.094)
Observations		114		114		114
Partial R ² of excl. instruments		0.3268		0.6818		0.2658
F (4,91)		19.68		42.27		14.34

Notes: Dependent variables: Baseline measures of legal property rights and contracting institutions. Excluded instruments: Executive constraints 1985–1995, its square, contract enforcement in 1995, and the interaction of the past executive constraints and contract enforcement scores. *, **, *** indicate 10, 5, 1% significance levels. Robust standard errors in parentheses. Constants are included but not reported.

Table A.5
Summary statistics for longer time period regressions.

	Obs	Mean	St.dev	Min	Max
<i>Dependent variables</i>					
Log real income per capita	2260	8.583	1.436	5.667	11.385
Long-term income level ($\hat{\mu}_i$)	2260	0.000	1.413	–2.736	2.643
<i>Institutional variables</i>					
Executive Constraints	2260	0.000	1.000	–2.260	0.905
Legal Enforcement of Contracts	2260	0.000	1.000	–2.908	2.178
<i>Control variables</i>					
Investment (% of GDP)	2260	23.973	7.444	1.525	79.401
Population (per sqkm)	2260	185.895	658.848	2.180	7953
Trade (% of GDP)	2260	82.310	45.502	1.295	437.327
Cultural fractionalization	2260	0.307	0.210	0	0.733
Years of schooling	2260	9.374	2.145	4	16

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