New evidence on the institutional causes of economic growth:

Using peer pressure to unbundle institutions across countries

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Abstract

This study presents a new framework of assessing the causal effect of institutions on economic growth via exploiting the exogenous variation in institutions triggered by "peer pressure" exerted on governments by other states, affecting policy decisions and the environment national and international businesses operate in. The applied method reinforces the importance of institutional factors for economic outcomes and allows to effectively address the data quality and instrument validity concerns surrounding earlier studies. Most importantly, the "peer pressure" method allows to distinguish between influences of various institutions, answering not only the question whether, but also which institutions do matter for growth. The study shows that the proposed method has significant power to "unbundle institutions" and finds that property rights protection and financial freedom are more important for growth than democracy, constraint on the executive, legal origins, or business freedom, even when controlled for human capital. The developed method has significant applicability in future research of institutional and cultural impact on international business and economic outcomes.

Keywords: economic growth; instrumental variables; institutions; international trade; political pressure; property rights

JEL codes: C26, E02, O43, O47

Twenty years of acknowledging institutions: international business and economics perspectives

The importance of the institutional factors for international business and the macroeconomic performance in general has been acknowledge in the literature for almost two decades now (Mudambi and Navarra, 2002; Henisz and Swaminathan, 2008; Aguilera and Groggard, 2019). The paradigm of institutional business and economics research has been developing rapidly, with the notion that "institutions matter" being obvious and well-proven from multiple studies and lines of inquiry, both macroeconomic and firm-level (Acemoglu et al., 2001; Aguilera and Groggard, 2019b). However, there is still no consensus in the literature on which and how these institutional factor shape economic and international business outcomes, with "unbundling institutions" becoming a very important challenge for contemporary researchers in this multidisciplinary field (Rodrik et al., 2004; Bhattacharyya, 2009; Cuervo-Cazurra et al., 2019b).

This study fills the gap in the existing literature by conceptualising the methodology to address some of the recent debates and issues in the area, such as disentangling the effects of various institutions (Mudambi and Navarra, 2002; Henisz and Swaminathan, 2008), acknowledging institutional spillovers (Cuervo-Cazurra et al., 2019b), institutional drift (Aguilera and Groggard, 2019), the determinants of institutional change (Cuervo-Cazurra et al., 2019a), and the causality issue (Acemoglu et al., 2001; Henisz and Swaminathan, 2008). To successfully tackle this research question, the study turns to a multidisciplinary approach, utilising and improving upon the techniques used in new institutional economics, while acknowledging the concepts elaborated in the field of international business.

Twenty years of instrumenting for institutions: advances and challenges

The identification of adequate instrumental variables for measures of institutions has been a major staple in empirical research in development economics at least since Hall and Jones

(1999) and Acemoglu et al. (2001). As institutions are endogenous regressors, the consensus in the literature agrees that OLS estimates of institutional impact on economic growth are proven to be inconsistent, constituting a textbook case where experimental evidence or IV estimators might resolve the issue (Angrist and Krueger, 2001). Since laboratory experiments in macroeconomics are obviously impossible, researchers utilise quasi-natural experiments and instrumental variable methods to isolate exogenous variation in institutions and therefore to obtain unbiased and consistent estimates of the causal impact of institutions on growth. However, the definition of instrumental variables of sufficient quality has proven to be quite challenging. Acemoglu et al. (2001) propose the famous "log settler mortality" variable as an instrument for institutions, namely protection against expropriation, constraint on the executive, law and order, and judiciary efficiency, to study the institutional causes of economic development in 69 ex-colonies. They argue that territories where early colonists experienced higher mortality prohibited active European settlement and incentivised the establishment of tyrannical colonial administration aimed at extracting rents from local populations that have been preserved through centuries in various forms. Conversely, low mortality led to a different form of colonisation, where settlers could populate the area and import more inclusive European institutions such as rule of law, protection of property rights, and the system of "checks and balances" to facilitate their own economic activity. Log settler mortality has been found to be a sufficient instrument in explaining the cross-country variation in institutions, simple first-stage regressions yielding adjusted R-squared in range from 0.17 to 0.51 for different institutional variables. TSLS estimates of the causal effects of institutions on economic development were two to three times higher than respective OLS estimates (Acemoglu et al., 2001). This study has been extremely influential and spurred continuous academic discussions. Acemoglu et al.'s (2001) has been criticised across multiple well-identifiable lines of argument.

First, as shown by Glaeser et al. (2004), log settler mortality might not be a truly exogeneous instrument. Apart from institutional quality, initial mortality of colonists might have influenced another important determinant of economic growth, namely, human capital stock. Early settlers that chose to stay could bring with them not only the tradition of property rights protection, but also their superior knowledge, skills, and practices. Moreover, it can be puzzling that log settler mortality adequately predicts various economic institutions, but is markedly worse at explaining the variations in political institutions, such as proportional representation or popular democracy. It is therefore unclear why colonists successfully imported one strain of institutions but failed to transfer the other. Moreover, settler mortality (similarly to latitude, a variable sometimes also used to instrument for institutions) is associated with prominent geographical and climatic factors that can also be crucial in explaining cross-country differences in per capita income (McArthur and Sachs, 2001). Primarily, it has been evidenced that the presence of the tsetse fly has continuously crippled African agriculture and economic development (Alsan, 2015). Furthermore, as Europeans brought their "guns, germs, and steel" with them in addition to their institutional practices, it is decidedly unclear through which channel settler mortality did shape long-term economic growth (Glaeser et al., 2004). For example, some recent evidence from a regional study in the island of Java, Indonesia shows that the areas where Dutch colonists have established sugar factories in the XIX century remain wealthier and more industrialised today, more than 70 years since Indonesia has gained independence (Dell and Olken, 2017). Dell and Olken (2017) show that this result is robust to geographic and climatic factors via respective placebo test, and attribute this effect to human capital accumulation and infrastructural development. Therefore, log settler mortality becomes a questionable instrument, at best, given its potential correlation with climatic and geographical factors and initial human capital accumulation. Similar discussions emerged surrounding nearly all variables used to instrument for institutions. As such, the linguistic measures of Hall and Jones

(1999) and Dollar and Kraay (2003) (fraction of populations speaking English or other European language as a first or a second language) can be subject to a similar criticism, as language skills are correlated with human capital (a more educated population is more likely to learn foreign languages) as well as be an endogenous variable in the first place (modern European migrants having a higher propensity to move into wealthier countries with better institutions). In the same fashion, using resource abundance as an instrument, motivated by a well-known "resource curse" phenomenon, with high rents tempting governments to adopt extractive institutions (Kolstad, 2009; Guriev et al., 2012), can be debatable as natural resources might also factor into the production function directly as an input (Rodrik et al., 2004). Therefore, there is a significant gap in the literature regarding verifiability and replicability of the instrumental variable construction. Reflecting the necessary requirements for an adequate instrumental variable identified in Angrist and Krueger (2001), a would be a process that can produce an instrumental variable that cannot plausibly be correlated with any other determinant of economic growth (such as geography, climate, human capital, infrastructure, etc.) and is clearly corresponding to one particular institutional variable.

Second, there are significant criticisms directed at the Acemoglu et al.'s (2001) dataset and variable definition. Albouy (2012) shows that the settler mortality data is largely inadequate, as it treats mortality rates of bishops, soldiers in barracks, and soldiers on campaign as comparable. Furthermore, sometimes that study fails to annualise mortality rates in some cases and extrapolating rates from different territories onto others, the most prominent example perhaps being assigning a mortality rate from mainland China to Hong Kong (Albouy, 2012). The estimates are also very sensitive to log settler mortality variable definition, with results ceasing to be statistically significant when some of the highest and most questionable observations are excluded from the sample (Albouy, 2012). Fails and Kriekhaus (2010) criticise the sample choice of Acemoglu et al. (2010) from a different perspective: they show that if city-states such

as Hong Kong and Singapore as well as unconventional colonies such as Australia, Canada, New Zealand, and United States are removed from the sample, the first-stage regression between institutional quality and settler mortality ceases to be significant. The notion that economic institutions in high-mortality colonies were particularly extractive is also highly debatable: Frankema (2010) examines taxation levels in colonies and metropoles and argues the tax burden has been lower in all of the colonies, and therefore the "extractive institutions" claim is invalid. This corresponds to another strain of institutional economics literature, relating "state capacity", i.e. the ability of the government to enforce order and collect taxes, to current economic outcomes (Bessley and Persson, 2009; Acemoglu et al., 2015; Johnson and Koyama, 2017). This approach emphasises the foundational role of the state as the engine of economic development rather than the traditional new institutional approach that focuses on government constraints as facilitators to long-term growth. However, these studies can be also subject to same criticisms of instrument endogeneity outlined above. Therefore, there is a significant gap in the literature regarding verifiability and replicability of the instrumental variable construction. An ideal case would be a process that computes instrumental variables for all countries based on the same data source so the results are directly comparable across countries. Acemoglu et al. (2014) addressed this concern by revising their data and approach to include human capital variables and capping mortality estimates at 250 per 1,000. Their results reinforced the relative importance of institutions and showed that human capital impact is three to four times lower in TSLS than in OLS regressions, while the estimate of the institutional effect remains virtually unchanged with the inclusion of human capital and a variety of controls. Furthermore, Acemoglu et al. (2014) suggested another instrument for institutions – initial population density – that is arguably better suited for their theory. In regions with high population density, Europeans could rely on exploiting the labour of ingenious populations, thus establishing extractive institutions. If population density was low, however, colonists had

to subsist on their own work and therefore had to imitate more inclusive institutions of the metropoles. Nevertheless, Albouy (2012) argues that the new dataset does not correct the most impactful mistakes of the initial paper and therefore the results are still not reliable enough. As for the educational instruments, Acemoglu et al.'s (2014) choice of using data on Christian missions is also questionable. While missionaries, especially Protestants, undoubtedly facilitated education in target regions, it is not the only channel though which missionary activity could contribute to economic growth. For example, Woodberry (2012) and Woodberry and Shah (2004) show that countries with more intensive Protestant missionary activity are more likely to become democracies, and therefore Protestantism might be not an exogenous instrument for human capital. Nevertheless, many subsequent studies replicated the approach of two separate instruments for instruments and human capital, using log settler mortality or population density for the former and various measures of missionary activity for the latter (Rodrik et al., 2004).

Third, Acemoglu et al.'s (2001, 2005, 2014) methodology is only applicable to ex-colonies. Acemoglu et al. (2001) study a 69-country sample, while Rodrik et al. (2004) show that their analysis can be at best extended to a sample with 80 observations. While such a sample arguably allows to generate consistent and significant estimates, its representativeness remains questionable. Log population density, suggested by Acemoglu et al. (2014) in their later study, does not resolve the issue, as estimates of initial ingenious populations are also limited, capping the sample at 62 observations. Rodrik et al. (2004) suggest expanding the sample to 140 countries by abandoning log settler mortality and utilising Hall and Jones's (1999) linguistic variable instead, however there is a clear trade-off, as this instrument's exogeneity is even more debatable. Another gap in the literature, therefore, revolves around small sample sizes: as exogenous instruments are computed from historical data, they are by definition heavily restricted by data availability (such as limiting the sample with ex-colonies alone). On the other

hand, an instrumental variable that could be derived from current data can resolve this notable issue.

Finally, another prominent limitation of existing instrumental variable strategies is their ambiguity related to particular types of institutions. Currently, there exist dozens of various institutional indicators, measuring various facets of democracy (Polity project), economic institutions (Heritage economic freedom index), rule of law (World Justice Project), and ease of doing business (World Economic Forum). All of them are potentially relevant for economic growth, and most of them are perhaps being influenced by distant historical factors that manifest themselves in classical instrumental variables. However, there are few instruments that are unique to only one institutional measure, therefore disentangling the impact of various institutions becomes problematic and ambiguous. For econometric estimation purposes, if there is a variety of institutional indices and one instrument related to all of them, it is impossible to estimate a model with all institutional regressors as the system of equations is under-identified (Angrist and Krueger, 2001). One of the notable exceptions is the "legal origins" dummy variable, distinguishing between common and civil law countries and closely related to British or French colonial institutions, respectively (La-Porta et al., 2008). In the mid-2000s, a body of literature has become emerging that seeks to "unbundle" the effect of different institutions. Acemoglu and Johnson (2005) distinguish between "political institutions" – constraint on the executive, which, as they argue, is a proxy for property rights, instrumented with log settler mortality or initial population density – and "contracting institutions" – legal framework facilitating private contracts between agents, instrumented with the legal origins dummy. It is shown that among these two institutional variables, "political institutions" are relevant for growth, while "contracting institutions" are not. Rodrik (2005) classifies the institutional mix policymakers might implement into appropriate incentives, market-based competition, property rights protection, and sound money, arguing that in theory these objectives can be pursued and

fulfilled independently. Bhattacharyya (2009) modifies Rodrik's (2005) approach and uses different measures for market-creating, market-legitimising, market-regulating, and market-stabilising institutions while also controlling for human capital. Bhattacharyya (2009) uses classical linguistic instruments (Hall and Jones, 1999; Dollar and Kraay, 2003), log settler mortality (Acemoglu et al., 2001), and latitude to instrument for the set of endogenous variables, finding that market-creating and market-stabilising institutions are important for growth, while the effect of market-legitimising and market-regulating institutions is limited. However, Bhattacharyya (2009) reports severe multicollinearity in some estimations, citing near linear dependence in institutions or instrumental variables as a reason. Therefore, multicollinearity tests, such as variance inflation factor reporting, can be considered crucial for future studies on "unbundling institutions". Some simpler studies feature OLS regressions to determine which institutions alleviate the "resource curse" issue. Kolstad (2009) shows that rule of law does mitigate the adverse economic effects of resource abundance while democratic institutions do not. Another notable issue in the literature is lack of good-quality instruments for democracy. As such, Acemoglu et al. (2019) had to resort to GMM models (that notably create synthetic instruments from lagged dependent and independent variables) to measure the economic effects of democratisation and illustrated that it does increase GDP per capita by 20% in the long run. Therefore, the inconclusive and fragmented literature on "unbundling institutions" proves that the field would certainly benefit from the establishment of a theoretically plausible and empirically sound one-to-one correspondence (bijection) between a set of institutional variables and their instruments.

Using external pressure as a source of exogenous variation: international trade

The main reason why a study would like to utilise IV estimations is regressor endogeneity (Angrist and Krueger, 2001). Indeed, even Acemoglu et al. (2014) themselves cite the

competing institutional theories of North (North and Thomas, 1973; North, 1991; North et al., 2009), stating that institutions lead, and economic development follows, and Lipset (1959), arguing essentially the opposite in his famous "prerequisites to democracy" concept. In international business studies, the endogeneity issue is raised by Henisz and Swaminathan (2008), when it is identified that institutional change has accelerated since 1980s, and correctly estimating the relationship between institutional, international business, and economic outcomes has become increasingly challenging. Therefore, IV approach has firmly taken its place as a go-to technique in such empirical studies.

In any instrumental variable estimation, a researcher tries to isolate exogeneous source of variation in the regressor (here, various institutional indices), and then compute unbiased estimators of its causal effect on the dependent variable (here, economic growth). The main concern is that institutions can be influenced by past realisations of economic growth, and therefore the OLS estimate of institutional effect will be inconsistent. Apart from Lipset's (1959) idea, another prominent model of institutional endogeneity is suggested by Caplan (2003). His "idea trap" model involves the population that demands interventionist and exploitative economic policies following periods of poor growth and free-market policies after prolonged economic expansions. With opportunistic politicians that seek to be re-elected, Caplan (2003) shows that the equilibrium can be self-sustaining, with poor countries stuck with poor institutions, rich countries enjoying good institutions, and mid-income countries having average-quality institutions. Similarly, more recent studies showed that both Democratic-Republican political cycles in the United States (and left-right wing party cycles in all other two-party democracies) as well as populist policies, and corresponding institutional change can be explained by rational electoral demands shifted by time-varying risk aversion and inequality aversion (Pastor and Veronesi, 2017, 2018). Therefore, it is obvious that any study that seeks to determine the causal effect of institutions on growth (and, all the more so, to assess

differential impact of a *set* of institutions) should isolate a source of exogenous variation in institutions.

This study utilises a novel approach that, to the authors' best knowledge, has not been yet applied to the empirical economic growth studies. It seeks to measure the institutions across the representative group of each country's peers and treat this variable as "external pressure" that incentivises the government to bring its policies more in line with its counterparts and induce institutional change. As such change would be necessarily external, the TSLS estimate obtained from such an instrument will be consistent. This resonates with theoretical insights and suggestions in Cuervo-Cazurra et al. (2019ab) and Aguilera and Groggard (2019), who propose that potential drivers of institutional change should be accounted for in empirical studies.

The logic of "external pressure" as a source of exogenous variation also makes it applicable to a broad set of institutional parameters. If a particular country is not a democracy but borders or trades primarily with democracies, it can be pressurised, formally or informally, to become more democratic itself. Perhaps the clearest and most generalisable example is trade policy, taxation, and labour market regulations. If a particular country implements high tariffs, high corporate tax rates, or numerous labour protection laws, it might urge its trade partners that are more free-trade, have lower taxes, and have less rigid labour markets, to follow suit, or vice versa. Such reforms might also be a result of lobbying or informal influence by international business actors, as suggested in the studies of pro-market reforms (Cuervo-Cazurra et al., 2019a).

A textbook case of such external political pressure being successful is the end of apartheid in South Africa, when international sanctions imposed by major trade partners induced political change (Levy, 1999). A recent study shows that external influence has been impactful in determining the outcome of the Arab Spring in Morocco (Abdel-Samad, 2014). However, external pressure might not always encourage productive political change. An equally vivid

anecdotal example might be Soviet tanks in Prague in 1968 that prematurely ended democratic-socialist experiments of Alexander Dubcek and brought the Czechoslovakian policies of the time more in line with the authoritarian socialist regime of the Soviet Union (Williams, 1997). A formal model that offers an explanation why international political pressure can be effective in shaping political change is developed by Kaempfer and Lowenberg (1988). Overall, the consensus view is that external influence can be effective to change domestic policy but not enough to affect foreign policy, as argued by Veebel and Markus (2015) for the case of Russia-Ukraine tensions over the Crimea.

The literature therefore allows the study to formalise two distinct sources of external pressure that might affect domestic institutions: *economic* and *geographical*. Imagine that there are n countries and m distinct institutional measures. Then, EP_{ij} and GP_{ij} is the economic and geographical pressure country i receives with regards to institution j.

For economic pressure, the study considers the export-weighted institutional measure across all target countries:

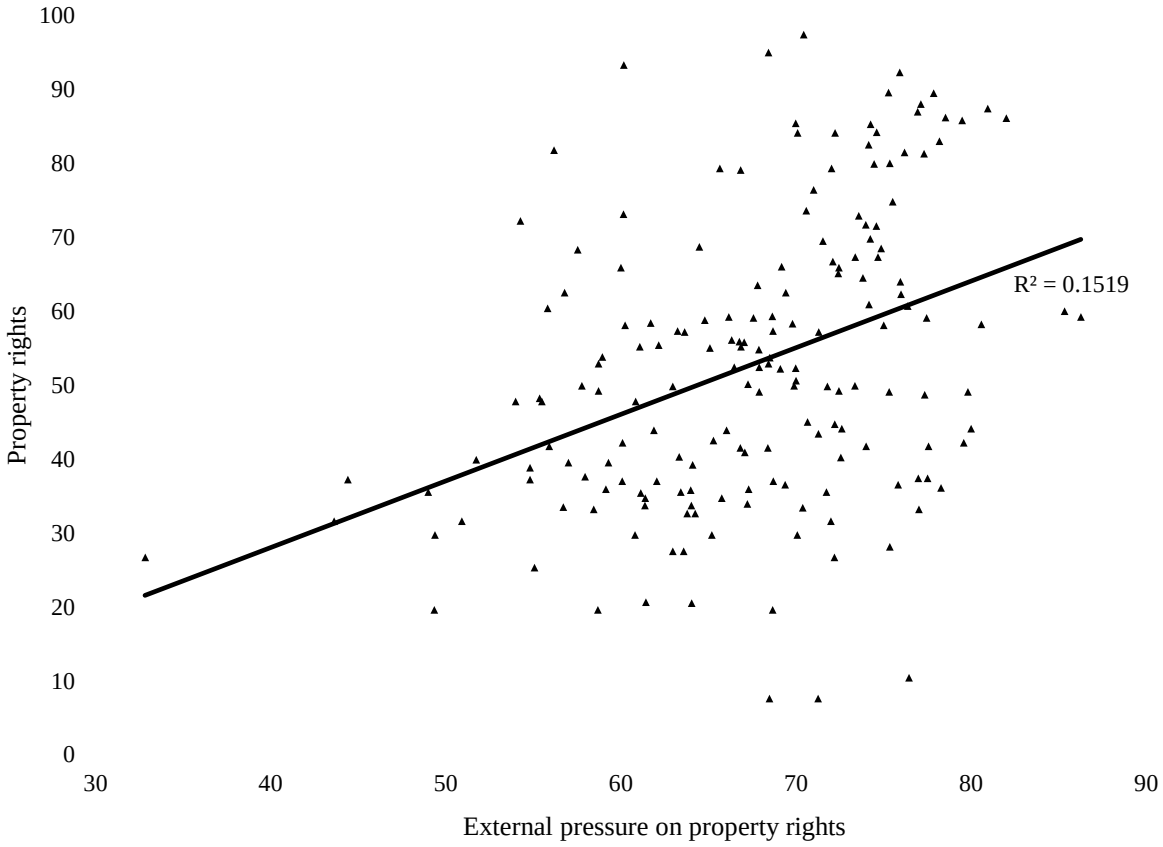
$$EP_{ij} = \frac{\sum_{\substack{k=1 \\ k \neq i}}^{n} X_{ik} I_{jk}}{\sum_{\substack{k=1 \\ k \neq i}}^{n} X_{ik}}$$

Where X_{ik} constitutes the volume of exports from country i to country k in the most recent year available provided by the International Trade Centre, and I_k is the institutional measure j in country k. Note that the summation operator sums over all countries except i, therefore the country does not influence itself. Economic pressure, therefore, considers economic importance of various trade partners to country i and its relative bargaining power or, alternatively, the need to synchronise policies (legal systems, tax codes, other policies, etc.) with major partner countries.

Trade-related instrumental variables have a long and rich history of use in empirical economic development literature. In the 1990s, a wide variety of studies has emerged, trying to underpin

the causal effects of trade on growth. Hall and Jones (1999) use predicted trade volumes from a simple gravity model to instrument for actual trade flows and find that trade openness does positively affect growth. This study, in contrast, exploits variation in trade to isolate exogeneous differences in institutions that are relatable to economic pressure from trade partners. As EP_{ij} is a relative measure that does not depend on total trade volumes, it does not account for trade-related growth factors and can be interpreted as an instrument for institutions. Figure 1 below graphically represents the first stage for the example of property rights.

Figure 1. First stage for property rights – economic (trade-weighted) external pressure index.



It is reasonable, however, to doubt the exogeneity of such an instrument. Certainly, countries can to some extent control which countries do they trade with, and favour potential partners with closer political regimes. Therefore, EP_{ij} can represent a conscious policy choice rather than exogeneous pressure, causing a reverse causation issue. To reconcile this problem, one can observe if historical peer pressure has influenced institutional change on a broad sample of

countries over a reasonable period of time. Undertaking such an analysis might be worthy of a study of its own, nevertheless, as many institutional measures in question are discontinuous or started to have been computed recently, checking the pressure effectiveness hypothesis is unfeasible for the whole set of institutions investigated here. Notwithstanding, some indicators, predominantly Heritage measures of various economic freedom dimensions (though not all of them), have been available since 1995. Among twelve institutional measures Heritage currently considers, three (fiscal health, government integrity, and labour freedom) are unavailable for 1995 due to methodology change. The overall index is also therefore incomparable, as it now includes twelve components instead of nine. However, nine measures, namely, property rights, judicial effectiveness, tax burden, government spending, business freedom, monetary freedom, trade freedom, investment freedom, and financial freedom, are available both for 1995 and 2019. Therefore, one can calculate exogeneous pressure (difference between trade-weighted institutional variable for partner countries and the country's score) in 1995 and see if this measure is correlated with the change in respective indices of countries between 1995-2019. The data allows to test if external pressure has indeed caused countries to change their institutional frameworks on a 24-year time horizon. The sample is however markedly smaller for 1995, as data only on 98 countries is available. Nevertheless, it is sufficient to test this "institutional convergence" hypothesis. One can estimate the following regression equation:

$$I_{ij}^{2019} - I_{ij}^{1995} = \alpha_j + \beta_j\left(EP_{ij}^{1995} - I_{ij}^{1995}\right) + \varepsilon_{ij}$$

Where I_{ij}^{t} and EP_{ij}^{t} are the country's i score and external economic pressure that trade partners exercise over it in year t with regard to institution j, α_j is the intercept, β_j is the pressure effectiveness measure, and ε_{ij} is the error term. The study is interested in the sign, magnitude, and significance of β_j. If it is positive and significant, it implies that countries indeed do change their institutions in response to external pressure. If it is statistically indistinguishable from zero,

then it would mean that the external economic pressure instrument is endogenous and cannot be used.

Figures 2 and 3 below shows this relationship for property rights and trade freedom on the scatterplot, while Table 1 below presents the results for all nine institutions for which data is available. It can be clearly seen that external economic pressure is indeed effective with regards to all nine variables, all β_j coefficients being positive and statistically significant. For monetary and trade freedom, initial pressure in 1995 explains over 50% of the institutional change.

Figure 2. Evidence on the effectiveness of external economic pressure – property rights.

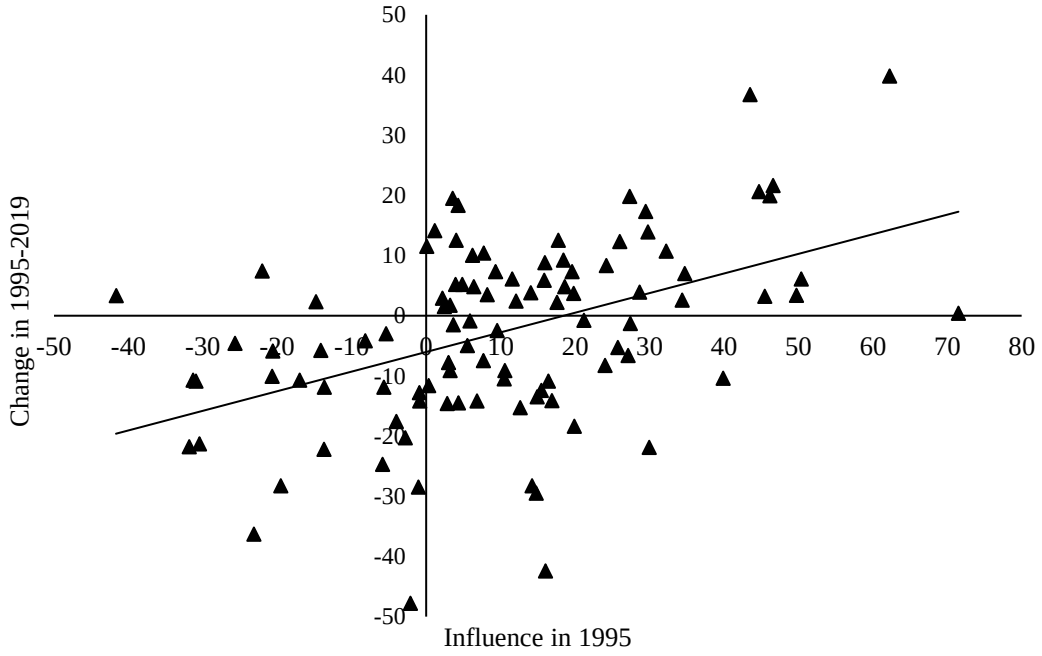


Figure 3. Evidence on the effectiveness of external economic pressure – trade freedom.

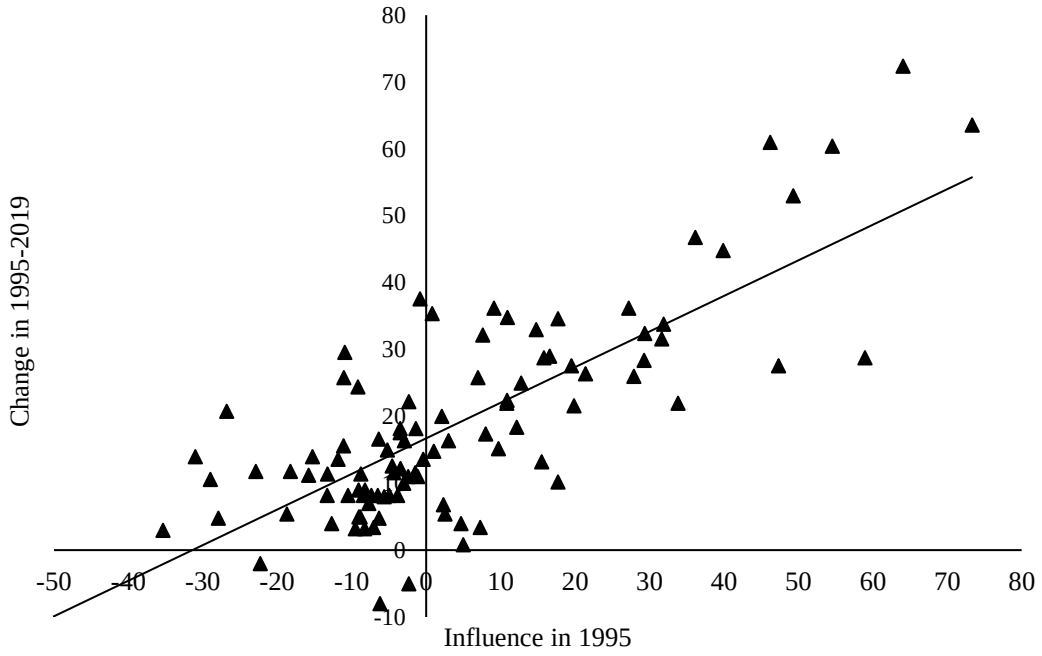


Table 1. Evidence on the effectiveness of external economic pressure (1995-2019).

Indicator	Convergence	R^2
Property rights	0.3380 (5.1873) *0.0000*	0.2189
Judicial effectiveness	0.5013 (9.7646) *0.0000*	0.4983
Tax burden	0.7683 (8.9804) *0.0000*	0.4565
Government spending	0.4898 (7.5115) *0.0000*	0.3702
Business freedom	0.3935 (5.2583) *0.0000*	0.2236
Monetary freedom	0.9204 (17.0946) *0.0000*	0.7527
Trade freedom	0.5329 (11.8427) *0.0000*	0.5936
Investment freedom	0.3258 (3.2467) *0.0016*	0.0989

	0.4080	
Financial freedom	(4.6913)	0.1865
	0.0000	

Therefore, trade-weighted external pressure can be considered an exogenous instrument for institutions and used on par with the geographical measure that is defined and discussed below.

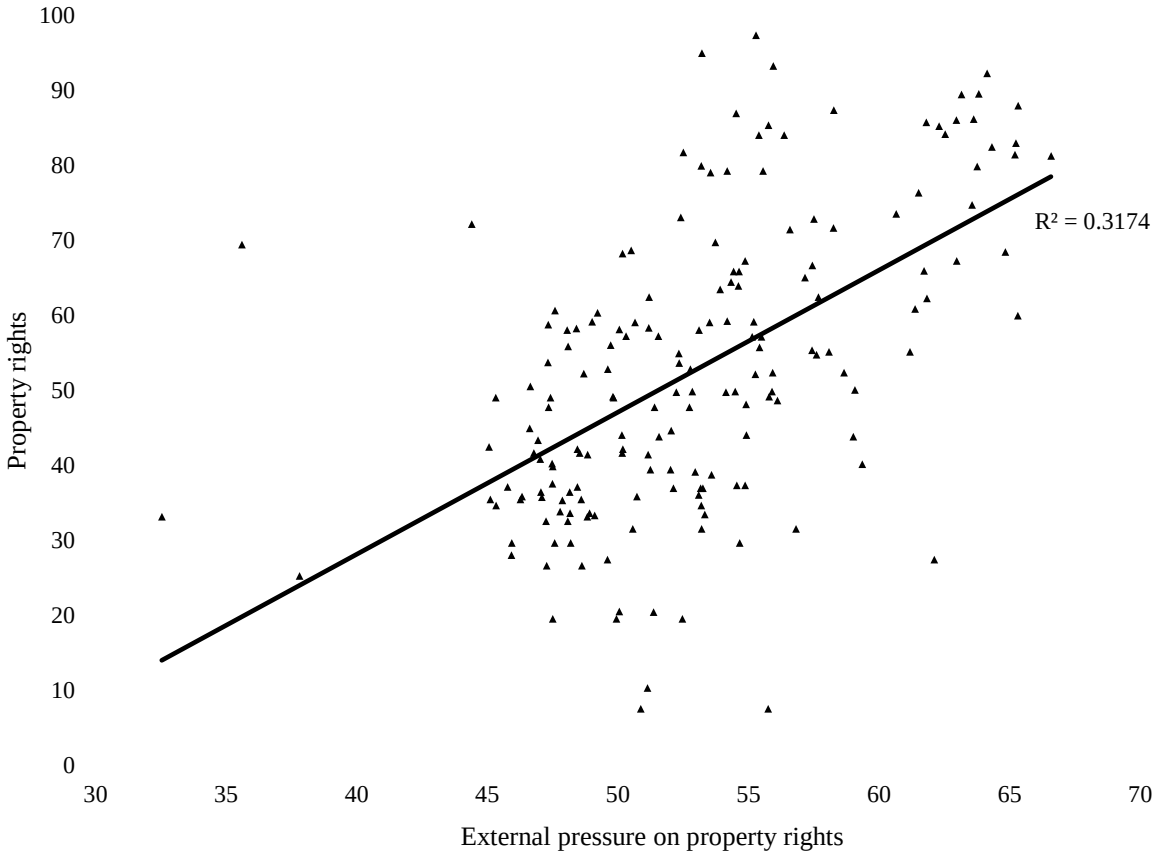
Using external pressure as a source of exogenous variation: geographical distance

For geographical pressure, the study considers inverse distance-weighted institutional measure across all target countries:

$$GP_{ij} = \frac{\sum_{\substack{k=1 \\ k \neq i}}^{n} D_{ik}^{-1} I_{jk}}{\sum_{\substack{k=1 \\ k \neq i}}^{n} D_{ik}^{-1}}$$

Where D_{ik}^{-1} is the inverse distance between capitals of i and k, computed as the length of a geodesic curve from latitudes and latitudes of cities. Geographic pressure instrument assigns a higher weight to closer countries, potentially incorporating threats of military influence (consider the tanks in Prague case). Unlike the trade-weighted instrument, this constructed variable is clearly exogenous by design: while governments can feasibly choose not to trade with countries that are not politically similar to them, they obviously cannot change their physical location and move away from them. The first-stage for the example of property rights index is graphically represented in Figure 4 below.

Figure 4. First stage for property rights – geographical (inverse distance-weighted) external pressure index.



The availability of trade data and geographic locations of country capitals allows the study to estimate econometric equations on much larger samples. Table 2 below shows the descriptive statistics on 26 institutional variables this study utilises. As can be seen, World Justice Project variables allows to include 120 companies into the sample, while Polity project, World Economic Forum, and Heritage data allow for more than 160 data points in case of each and every indicator, estimating the following equations:

$$I_{ij} = \alpha_j + \beta_j EP_{ij} + \varepsilon_{ij}$$

$$I_{ij} = \alpha_j + \beta_j GP_{ij} + \varepsilon_{ij}$$

Table 2. Descriptive statistics.

Institutional variable	Source	Number of observations	Mean	Median	Standard deviation	Minimum	Maximum
Economic freedom	Heritage	173	61.06	60.70	10.62	25.90	90.20
Property rights	Heritage	178	52.76	50.35	19.54	7.60	97.40
Judicial effectiveness	Heritage	178	45.15	42.90	17.95	10.00	92.40
Government integrity	Heritage	178	41.53	35.50	20.11	7.90	96.70
Tax burden	Heritage	173	77.45	78.00	11.97	42.00	99.80
Government spending	Heritage	176	64.71	68.90	22.52	0.00	96.60
Fiscal health	Heritage	176	65.68	80.00	31.70	0.00	100.00
Business freedom	Heritage	178	63.59	63.40	15.38	17.70	96.40
Labour freedom	Heritage	177	59.50	59.50	13.83	20.00	91.00
Monetary freedom	Heritage	177	75.31	77.70	9.85	0.00	88.00
Trade freedom	Heritage	175	74.84	76.60	11.07	45.00	95.00
Investment freedom	Heritage	176	57.39	60.00	22.48	0.00	95.00
Financial freedom	Heritage	173	49.42	50.00	19.19	10.00	90.00
Polity index	Polity IV	161	5.96	7.00	3.72	0.00	10.00
Constraint on the executive	Polity IV	161	5.22	6.00	1.96	0.00	7.00
Legal origins	La-Porta et al. (2008)	178	0.13	0.00	0.34	0.00	1.00
Ease of doing business	World Economic Forum	174	62.07	62.75	13.76	19.98	86.59
Rule of law	World Justice Project	120	0.56	0.52	0.14	0.28	0.90
Constraint on government	World Justice Project	120	0.55	0.53	0.16	0.18	0.95
Absence of corruption	World Justice Project	120	0.51	0.46	0.19	0.18	0.95
Government openness	World Justice Project	120	0.53	0.50	0.15	0.22	0.88
Fundamental rights	World Justice Project	120	0.57	0.56	0.16	0.25	0.92
Order and security	World Justice Project	120	0.72	0.72	0.13	0.30	0.93
Regulation enforcement	World Justice Project	120	0.54	0.50	0.15	0.20	0.90
Civil justice	World Justice Project	120	0.55	0.53	0.14	0.23	0.87
Criminal justice	World Justice Project	120	0.47	0.44	0.16	0.14	0.84

Table 3 below presents first-stage regressions for all 26 institutional variables for two candidate instruments (economic and geographical pressure).

Table 3. First-stage regressions.

Indicator	Economic pressure			Geographical pressure		
	β_j	R^2	Number of observations	β_j	R^2	Number of observations
Economic freedom	0.8401 (5.2460) *0.0000*	0.1386	173	1.6929 (6.9456) *0.0000*	0.2200	173
Property rights	0.9013 (5.6155) *0.0000*	0.1519	178	1.8937 (9.0467) *0.0000*	0.3174	178
Judicial effectiveness	0.4958 (2.8821) *0.0044*	0.0451	178	1.9949 (7.0067) *0.0000*	0.2181	178
Government integrity	0.6813 (4.4610) *0.0000*	0.1016	178	2.3373 (9.0557) *0.0000*	0.3178	178
Tax burden	0.9676 (6.8119) *0.0000*	0.2134	173	2.1551 (8.2332) *0.0000*	0.2839	173
Government spending	0.7774 (5.4118) *0.0000*	0.1441	176	1.6556 (7.4354) *0.0000*	0.2411	176
Fiscal health	0.4845 (2.2606) *0.0250*	0.0285	176	0.8365 (2.5851) *0.0106*	0.0370	176
Business freedom	0.8642 (4.9255) *0.0000*	0.1211	178	1.8825 (7.5302) *0.0000*	0.2437	178
Labour freedom	0.3910 (2.7270) *0.0070*	0.0408	177	1.2149 (3.2228) *0.0015*	0.0560	177
Monetary freedom	0.5015 (2.2987) *0.0227*	0.0293	177	0.2686 (0.7370) *0.4621*	0.0031	177
Trade freedom	1.0075 (5.2861) *0.0000*	0.1391	175	1.8470 (9.1257) *0.0000*	0.3250	175
Investment freedom	0.8735 (6.4498) *0.0000*	0.1930	176	1.9034 (6.7081) *0.0000*	0.2055	176

Financial freedom	0.6439 (4.6185) *0.0000*	0.1109	173	1.8324 (6.8285) *0.0000*	0.2143	173
Polity index	0.8428 (6.2562) *0.0000*	0.1975	161	1.6008 (7.1624) *0.0000*	0.2439	161
Constraint on the executive	0.9494 (6.3158) *0.0000*	0.2006	161	1.5564 (6.6118) *0.0000*	0.2156	161
Legal origins	0.3571 (3.2300) *0.0015*	0.0560	178	1.2633 (2.6904) *0.0078*	0.0395	178
Ease of doing business	0.9827 (5.4713) *0.0000*	0.1482	174	1.8805 (9.9591) *0.0000*	0.3657	174
Rule of law	0.8799 (5.0058) *0.0000*	0.1752	120	1.9134 (7.4232) *0.0000*	0.3183	120
Constraint on government	0.6713 (3.8494) *0.0002*	0.1116	120	1.8477 (5.5434) *0.0000*	0.2066	120
Absence of corruption	0.9610 (5.0687) *0.0000*	0.1788	120	2.0849 (7.585) *0.0000*	0.3278	120
Government openness	0.8583 (5.1382) *0.0000*	0.1828	120	1.9555 (7.4454) *0.0000*	0.3196	120
Fundamental rights	0.7302 (4.8466) *0.0000*	0.1660	120	1.9577 (7.4135) *0.0000*	0.3178	120
Order and security	1.0825 (5.7686) *0.0000*	0.2200	120	1.8901 (7.3229) *0.0000*	0.3125	120
Regulation enforcement	0.7730 (4.1397) *0.0001*	0.1268	120	1.6984 (6.0694) *0.0000*	0.2379	120
Civil justice	0.9025 (4.6153) *0.0000*	0.1529	120	1.6962 (6.0634) *0.0000*	0.2376	120
Criminal justice	1.0246 (5.4098) *0.0000*	0.1987	120	1.8938 (7.4801) *0.0000*	0.3217	120

In can be clearly seen that both candidate instruments have sufficiently strong first stages. The only statistically insignificant regression comes from monetary freedom in case of geographical pressure. However, the relationship between monetary freedom and economic pressure remains sufficiently strong. Overall, 45 out of 52 first-stage regressions comfortably exceed the t-stat threshold of three, suggesting the applicability of IV estimations with economic and geographical external pressure as instruments.

Another concern regarding the applicability of these pressure-related instrumental variables is the uniformity of pressure effectiveness across countries. It is not unreasonable to presume some countries might be more responsive to pressure than others. For economic pressure, trade-dependent countries (with higher trade-to-GDP ratios) can be more easily swayed, while for geographic pressure, surrounded countries (that have multiple bordering countries) might be more affected. To test for this, the study considers trade-to-GDP ratio (most recent figure available via World Development Indicators) and $\sum_{\substack{k=1 \\ k\neq i}}^{n} D_{ik}^{-1}$ as absolute pressure measures and estimates the following equations with interaction terms:

$$I_{ij} = \alpha_j + \beta_j EP_{ij} + \gamma_j EP_{ij} TRADE_i + \varepsilon_{ij}$$

$$I_{ij} = \alpha_j + \beta_j GP_{ij} + \gamma_j GP_{ij} \sum_{\substack{k=1 \\ k\neq i}}^{n} D_{ik}^{-1} + \varepsilon_{ij}$$

If γ_j are positive and significant, then there are heterogeneities in terms of pressure effectiveness, and it must be reflected in the design of instrumental variable sets.

Table 4 below reports the results of regressions with interaction terms:

Table 4. First-stage regressions with interaction terms.

Indicator	Economic pressure			Geographical pressure		
	β_j	γ_j	R^2	β_j	γ_j	R^2
Economic freedom	0.9390 (4.3175) *0.0000*	0.1004 (3.5542) *0.0006*	0.2793	1.3794 (4.3801) *0.0000*	-0.2766 (-0.3391) *0.7352*	0.1638

Property rights	0.8895 (4.1736) *0.0001*	0.1593 (2.9814) *0.0035*	0.2776	1.9738 (6.2622) *0.0000*	-1.9950 (-1.2125) *0.2279*	0.3146
Judicial effectiveness	0.5571 (2.2551) *0.0261*	0.1343 (2.1928) *0.0304*	0.1112	2.1060 (5.0567) *0.0000*	-1.8214 (-0.9258) *0.3566*	0.2149
Government integrity	0.6928 (3.0596) *0.0028*	0.1748 (2.4257) *0.0169*	0.1861	2.8000 (7.2502) *0.0000*	-3.2713 (-1.4991) *0.1367*	0.3690
Tax burden	0.9754 (5.0419) *0.0000*	0.0360 (1.15) *0.2527*	0.2047	2.2132 (6.8604) *0.0000*	0.7368 (1.0526) *0.2948*	0.3022
Government spending	1.0946 (6.885) *0.0000*	-0.0332 (-0.5178) *0.6056*	0.3047	2.3666 (10.4899) *0.0000*	3.3190 (2.0097) *0.0469*	0.5130
Fiscal health	0.5938 (1.7971) *0.0751*	0.1535 (1.776) *0.0785*	0.1079	0.6253 (1.5252) *0.1300*	3.7138 (1.6211) *0.1078*	0.0902
Business freedom	0.8500 (3.8465) *0.0002*	0.0936 (2.529) *0.0129*	0.2274	1.7636 (6.1443) *0.0000*	-0.4215 (-0.4334) *0.6656*	0.2804
Labour freedom	0.3599 (1.708) *0.0905*	0.1127 (2.4402) *0.0163*	0.0829	1.3027 (2.6688) *0.0088*	-1.5099 (-1.2517) *0.2133*	0.0755
Monetary freedom	0.9853 (2.4137) *0.0175*	0.0569 (1.9927) *0.0488*	0.1003	0.7003 (0.8359) *0.4050*	0.0897 (0.0982) *0.9219*	0.0136
Trade freedom	0.9392 (4.9944) *0.0000*	0.0806 (3.8335) *0.0002*	0.3374	1.3683 (5.8477) *0.0000*	-0.0161 (-0.0284) *0.9774*	0.2750
Investment freedom	0.8163 (4.3188) *0.0000*	0.1398 (2.2404) *0.0271*	0.2622	1.9771 (4.4606) *0.0000*	-0.9916 (-0.5309) *0.5965*	0.2172
Financial freedom	0.5868 (3.0345) *0.0030*	0.1787 (2.8263) *0.0056*	0.2016	2.0251 (4.5128) *0.0000*	-3.0251 (-1.5095) *0.1340*	0.1815
Polity index	0.8697 (3.8161) *0.0002*	-0.0860 (-0.8154) *0.4166*	0.1341	1.3801 (3.8697) *0.0002*	-1.8113 (-0.6553) *0.5137*	0.1624
Constraint on the executive	0.8693 (4.0855) *0.0001*	-0.0491 (-0.7904) *0.4310*	0.1377	1.0994 (3.6658) *0.0004*	-0.2893 (-0.1821) *0.8558*	0.1346
Legal origins	0.4210 (1.7631)	-0.0054 (-0.0238)	0.0514	2.6671 (3.3783)	-56.7865 (-1.9099)	0.0932

	0.0807	*0.9810*		*0.0010*	*0.0587*	
Ease of doing business	0.9649	0.1032		1.8257	-0.8451	
	(4.8653)	(3.1648)	0.3157	(7.407)	(-0.9658)	0.3773
	0.0000	*0.0020*		*0.0000*	*0.3362*	
Rule of law	0.8103	0.0902		2.3049	-2.1070	
	(4.1697)	(2.0155)	0.2358	(7.2923)	(-1.8251)	0.3594
	0.0001	*0.0463*		*0.0000*	*0.0707*	
Constraint on government	0.6984	0.0384		2.1729	-2.2314	
	(3.4787)	(0.684)	0.1356	(5.6032)	(-1.5681)	0.2304
	0.0007	*0.4954*		*0.0000*	*0.1197*	
Absence of corruption	0.8270	0.1314		2.4757	-2.7902	
	(3.9624)	(2.2577)	0.2358	(7.4512)	(-1.7811)	0.3710
	0.0001	*0.0260*		*0.0000*	*0.0776*	
Government openness	0.9556	0.0201		2.3133	-2.7204	
	(5.1508)	(0.4032)	0.2298	(7.6862)	(-2.1525)	0.3619
	0.0000	*0.6876*		*0.0000*	*0.0335*	
Fundamental rights	0.6618	0.0861		2.3191	-1.9886	
	(3.7741)	(1.6602)	0.2051	(6.8728)	(-1.5694)	0.3451
	0.0003	*0.0998*		*0.0000*	*0.1194*	
Order and security	0.7997	0.1337		2.3849	-1.3829	
	(4.0713)	(4.3571)	0.3513	(6.7605)	(-1.5657)	0.3591
	0.0001	*0.0000*		*0.0000*	*0.1203*	
Regulation enforcement	0.7405	0.0953		1.9724	-1.6904	
	(3.631)	(1.9901)	0.1975	(5.9028)	(-1.2979)	0.2658
	0.0004	*0.0491*		*0.0000*	*0.1970*	
Civil justice	0.7959	0.0876		1.9971	-1.4454	
	(3.6732)	(1.9299)	0.2046	(5.8429)	(-1.1984)	0.2690
	0.0004	*0.0562*		*0.0000*	*0.2333*	
Criminal justice	0.8780	0.1283		2.3268	-2.3970	
	(4.2047)	(2.3532)	0.2630	(6.8972)	(-1.5512)	0.3656
	0.0001	*0.0204*		*0.0000*	*0.1237*	
Sample	0.8451	0.0995		1.0140	0.1280	
	(76.1285)	(9.4378)	0.8684	(74.1640)	(0.2522)	0.8823
	0.0000	*0.0000*		*0.0000*	*0.6119*	

Only two out of 26 interaction terms have significant estimators in case of geographical pressure instruments (government spending and government openness). Moreover, the estimator for government openness is of the sign opposite to what the initial theoretical presumption is. The coefficient for the overall sample is also insignificant. Therefore, geographic pressure effectiveness can be considered independent of pressure intensity. Economic pressure, on the

other hand, is largely positively affected by countries' trade-to-GDP ratio: the interaction term is positive and statistically significant (at 5%) for 15 out of 26 institutional variables and positive and statistically significant for the whole sample. Therefore, the institutional variable set must be corrected for this heterogeneous effect.

In the next section, the method derived and instrumental variables defined are applied to "unbundle" the effect of 26 institutions on economic growth of sample countries.

Findings and discussion

In this section, the model estimation results are presented and the effect of 26 institutional measures on growth is considered.

Table 5 below presents the results of IV model estimations, including the TSLS equations with interaction terms for economic pressure, reflecting the evidence of pressure heterogeneity based on trade openness derived in Table 4 in the previous section. As advised by Young (2017), IV estimators are compared with corresponding OLS coefficients, while endogeneity and weak instruments are explicitly tested using Durbin-Wu-Hausman statistic (Nakamura and Nakamura, 1981) and Cragg-Donald F-stat (Cragg and Donald, 1993), respectively. The standard errors are adjusted for arbitrary heteroskedasticity using White (1980) covariance matrix, following Acemoglu et al. (2014).

Table 5. Model estimation results.

Indicator	OLS	Economic pressure			Economic pressure				Geographical pressure		
		TSLS	Endogeneity	Cragg-Donald	TSLS	Interaction term	Endogeneity	Cragg-Donald	TSLS	Endogeneity	Cragg-Donald
Economic freedom	0.0930	0.1519	8.4931	27.5203	0.1491	-0.0017	8.0644	14.5066	0.1716	26.5700	48.2413
	(11.4834)	(6.1012)			(5.2423)	(-0.4917)			(7.9812)		
	0.0000	*0.0000*	*0.0036*		*0.0000*	*0.6236*	*0.0177*		*0.0000*	*0.0000*	
Property rights	0.0580	0.0831	7.8516	31.5341	0.0870	-0.0021	8.3513	12.6462	0.0791	14.4469	81.8432
	(15.3397)	(7.6593)			(5.7538)	(-0.6668)			(10.8627)		
	0.0000	*0.0000*	*0.0051*		*0.0000*	*0.5058*	*0.0154*		*0.0000*	*0.0001*	
Judicial effectiveness	0.0532	0.1257	10.5252	8.3068	0.1324	-0.0024	11.9796	3.0607	0.0941	19.7597	49.0934
	(10.9613)	(3.6511)			(2.7566)	(-0.3468)			(7.6416)		
	0.0000	*0.0003*	*0.0012*		*0.0065*	*0.7292*	*0.0025*		*0.0000*	*0.0000*	
Government integrity	0.0531	0.0921	11.0249	19.9006	0.0931	-0.0019	14.9001	8.4951	0.0822	25.3035	82.0061
	(13.4463)	(5.9625)			(4.5697)	(-0.4652)			(10.2589)		
	0.0000	*0.0000*	*0.0009*		*0.0000*	*0.6424*	*0.0006*		*0.0000*	*0.0000*	

Tax burden	-0.0199 (-2.1105) *0.0363*	-0.0375 (-1.8148) *0.0713*	0.9359 *0.3333*	46.4015	-0.0409 (-2.1383) *0.0340*	0.0126 (5.0868) *0.0000*	0.2094 *0.9006*	24.2421	-0.0319 (-1.7897) *0.0753*	0.6335 *0.4261*	67.7856
Government spending	-0.0280 (-6.1477) *0.0000*	-0.0566 (-4.2588) *0.0000*	6.6361 *0.0100*	29.2875	-0.0608 (-4.7627) *0.0000*	0.0082 (2.6327) *0.0093*	11.3547 *0.0034*	12.5446	-0.0655 (-5.9878) *0.0000*	21.4720 *0.0000*	55.2848
Fiscal health	0.0124 (3.5999) *0.0004*	0.0864 (2.2171) *0.0279*	13.5769 *0.0002*	5.1101	0.1067 (1.1759) *0.2413*	-0.0021 (-0.1786) *0.8585*	7.9065 *0.0192*	0.8486	0.1215 (2.6081) *0.0099*	38.5520 *0.0000*	6.6828
Business freedom	0.0666 (12.3891) *0.0000*	0.1260 (6.2692) *0.0000*	16.8900 *0.0000*	24.2605	0.1238 (5.2705) *0.0000*	-0.0002 (-0.0603) *0.9520*	16.0013 *0.0003*	11.4560	0.1173 (8.7792) *0.0000*	28.6745 *0.0000*	56.7038
Labour freedom	0.0311 (3.9801) *0.0001*	0.1101 (2.2594) *0.0251*	4.3344 *0.0373*	7.4366	0.1168 (1.8080) *0.0724*	0.0047 (0.7739) *0.4401*	5.7970 *0.0551*	2.6859	0.1302 (2.8466) *0.0049*	9.5360 *0.0020*	10.3866
Monetary freedom	0.0551 (5.1587) *0.0000*	0.2234 (2.3024) *0.0225*	7.4965 *0.0062*	5.2839	0.1932 (2.0500) *0.0419*	0.0006 (0.0984) *0.9217*	6.4947 *0.0389*	2.7883	0.8891 (0.7735) *0.4403*	18.9262 *0.0000*	0.5432
Trade freedom	0.0808 (9.7433) *0.0000*	0.1706 (5.9237) *0.0000*	19.0043 *0.0000*	27.9431	0.1776 (4.9525) *0.0000*	-0.0034 (-0.9028) *0.3680*	17.8207 *0.0001*	11.8629	0.1287 (8.1043) *0.0000*	16.1389 *0.0001*	83.2776
Investment freedom	0.0337 (7.7246) *0.0000*	0.0464 (4.5616) *0.0000*	2.0243 *0.1548*	41.6001	0.0368 (3.1872) *0.0017*	0.0056 (1.9431) *0.0537*	2.0066 *0.3667*	24.5200	0.0700 (6.1547) *0.0000*	17.9802 *0.0000*	44.9991
Financial freedom	0.0481 (10.2256) *0.0000*	0.0773 (4.9455) *0.0000*	4.8225 *0.0281*	21.3308	0.0703 (4.0634) *0.0001*	0.0024 (0.6492) *0.5171*	6.2744 *0.0434*	12.3068	0.1002 (7.5248) *0.0000*	33.5294 *0.0000*	46.6289
Polity index	0.1402 (4.6342)	0.1697 (2.4848)	0.2331	39.1398	-0.0011 (-0.0140)	0.1293 (3.9015)	6.6384	19.5442	0.3263 (4.7875)	12.2068	51.3000

	0.0000	*0.0140*	*0.6292*		*0.9889*	*0.0001*	*0.0362*		*0.0000*	*0.0005*		
Constraint on the executive	0.2038	0.4109	3.0696	39.8893	0.1646	0.1633	4.2114	20.2923	0.6502	15.7483	43.7153	
	(3.4551)	(3.0005)			(1.1544)	(3.8851)			(4.3892)			
	0.0007	*0.0031*	*0.0798*		*0.2502*	*0.0002*	*0.1218*		*0.0000*	*0.0001*		
Legal origins	0.7917	-1.6070	3.2368	10.4327	-3.7480	2.1380	11.8950	4.9050	-0.2877	0.4547	7.2384	
	(2.4389)	(-1.0231)			(-1.9367)	(3.2606)			(-0.1709)			
	0.0157	*0.3077*	*0.0720*		*0.0545*	*0.0013*	*0.0026*		*0.8645*	*0.5001*		
Ease of doing business	0.0767	0.1323	15.4050	29.9346	0.1355	-0.0001	13.7327	11.5696	0.0998	8.7946	99.1828	
	(12.9702)	(7.0010)			(5.4251)	(-0.0346)			(9.7812)			
	0.0000	*0.0000*	*0.0001*		*0.0000*	*0.9724*	*0.0010*		*0.0000*	*0.0030*		
Rule of law	7.9802	11.9386	8.9671	25.0578	12.2911	-0.3791	9.4816	11.8584	10.1573	5.9648	55.1034	
	(13.1006)	(7.0391)			(6.2053)	(-1.0228)			(8.9366)			
	0.0000	*0.0000*	*0.0027*		*0.0000*	*0.3085*	*0.0087*		*0.0000*	*0.0146*		
Constraint on government	5.7834	9.0682	3.4055	14.8176	8.2272	0.6653	3.2145	7.1530	10.0036	11.6576	30.7288	
	(9.1690)	(4.3301)			(3.7458)	(1.6758)			(6.1380)			
	0.0000	*0.0000*	*0.0650*		*0.0003*	*0.0965*	*0.2004*		*0.0000*	*0.0006*		
Absence of corruption	6.1633	9.8824	14.8802	25.6917	10.5386	-0.6045	14.7160	12.3276	8.2755	10.7478	57.5318	
	(13.7001)	(7.3916)			(6.4811)	(-1.5938)			(9.6667)			
	0.0000	*0.0000*	*0.0001*		*0.0000*	*0.1137*	*0.0006*		*0.0000*	*0.0010*		
Government openness	6.7311	9.8566	5.7654	26.4011	9.2716	0.4761	6.3581	13.1841	9.8400	11.9778	55.4338	
	(10.9326)	(6.2015)			(5.5237)	(1.3396)			(8.1936)			
	0.0000	*0.0000*	*0.0163*		*0.0000*	*0.1830*	*0.0416*		*0.0000*	*0.0005*		
Fundamental rights	6.3443	8.6877	2.6769	23.4896	8.3245	0.2271	4.1443	10.9183	9.0163	8.1430	54.9594	
	(9.9277)	(5.2482)			(4.4273)	(0.5824)			(7.4224)			
	0.0000	*0.0000*	*0.1018*		*0.0000*	*0.5615*	*0.1259*		*0.0000*	*0.0043*		
Order and security	6.9021	12.7408	12.7468	33.2770	15.5272	-1.1765	22.7550	12.0979	10.1114	6.2062	53.6243	
	(7.9476)	(5.8508)			(4.8579)	(-2.2806)			(6.1614)			
	0.0000	*0.0000*	*0.0004*		*0.0000*	*0.0244*	*0.0000*		*0.0000*	*0.0127*		
	7.3989	12.8362	12.3130	17.1375	13.2408	-0.5517	11.4425	8.6482	10.0065	6.0869	36.8377	

Regulation enforcement	(12.5296)	(5.9050)			(5.4066)	(-1.2913)			(7.6568)		
	0.0000	*0.0000*	*0.0004*		*0.0000*	*0.1992*	*0.0033*		*0.0000*	*0.0136*	
Civil justice	7.9257	13.3915	12.4064		14.2848	-0.6071	12.2147		11.0996	7.2204	
	(12.0213)	(6.3140)		21.3010	(5.4537)	(-1.3501)		9.4571	(7.5018)		36.7652
	0.0000	*0.0000*	*0.0004*		*0.0000*	*0.1796*	*0.0022*		*0.0000*	*0.0072*	
Criminal justice	6.7200	9.6629	6.3285		10.2137	-0.4042	7.1494		8.0102	2.3256	
	(11.5342)	(6.7043)		29.2662	(5.7433)	(-0.9727)		13.5070	(7.6404)		55.9518
	0.0000	*0.0000*	*0.0119*		*0.0000*	*0.3328*	*0.0280*		*0.0000*	*0.1273*	

Notes: OLS and TSLS estimations of institutional impact on economic growth with economic (trade-weighted) and geographic (inverse distance-weighted) external pressure as instruments. T-stats are reported (in parentheses) and p-values are presented *in italics*. Durbin-Wu-Hausman test (Nakamura and Nakamura, 1981) and Cragg-Donald F-stat (Cragg and Donald, 1993) are used to determine regressor endogeneity and weak instruments, respectively. Standard errors are corrected for arbitrary heteroskedasticity using White heteroskedasticity-consistent covariance matrix (White, 1980).

The results show that institutional variables are highly endogenous, and therefore OLS estimators of their effect on growth are biased. For economic pressure, economic pressure with an interaction term, and geographic pressure 22, 23, and 24 institutional indicators out of 26 are proven to be endogenous as per Durbin-Wu-Hausman test (Nakamura and Nakamura, 1981). Only tax burden, investment freedom, constraint on the executive, legal origins, constraint on government, fundamental rights, and criminal justice (7 out of 26 variables) can be considered exogenous for at least one of the models, with tax burden alone reliably classified as exogenous for all three. The instrumental variables developed earlier in the study can be considered sufficiently strong, with 22, 17, and 23 of them comfortably exceeding the conventional Cragg-Donald F-statistic threshold of 10 (Cragg and Donald, 1993). Fiscal health, monetary freedom, and legal origins demonstrate some signs of weak instruments, while all other estimations can be reliably used for inference. Hence, both major issues with IV techniques identified in Young (2017) are successfully avoided in

the design of this study, with instruments both having sufficiently strong first stages and effectively demonstrating the endogeneity of the institutional variables considered.

Another potential concern with TSLS models might be that respective IV estimators are not statistically and economically different from corresponding OLS estimators, therefore rendering the benefits of TSLS approach questionable at most (Young, 2017). To address that, the study calculates average differences between TSLS and OLS estimators both across all models and across models with sufficiently strong instruments only. Table 6 presents the respective results. In all cases, TSLS estimators are at least 50% higher than respective OLS coefficients, with all differences being statistically and economically significant. For purposes of further inference, the geographical pressure instruments are used, as they are the strongest (evidenced by higher Cragg-Donald F-statistics and R^2 values in first-stage regressions) and the most efficient in identifying endogeneity.

Table 6. OLS and TSLS estimator comparison

		Number of observations	Mean difference	Standard error	p-value
All estimators	Economic pressure	26	91.35%	27.11%	*0.0025*
	Economic pressure + interaction term	26	79.25%	40.14%	*0.0599*
	Geographic pressure	26	158.45%	80.82%	*0.0616*
Only sufficiently strong instruments	Economic pressure	22	49.20%	23.01%	*0.0450*
	Economic pressure + interaction term	17	54.21%	17.15%	*0.0065*
	Geographic pressure	23	80.98%	19.70%	*0.0005*

One of the most common and valid criticisms of the institutional theory of economic growth and development is the human capital theory. As such, Glaeser et al. (2004) first criticised Acemoglu et al. (2001) log settler mortality approach as it affects not only the initial institutional mix, but also the initial stock of human capital in the ex-colonies. Despite the instruments proposed by this study cannot be theoretically correlated with human capital, it still opts to use human capital controls as an additional robustness check to test the validity of the results and present evidence of the relevance of "unbundled institutions" effect against the most common competing theory.

The existing literature reports mixed findings regarding the effect of human capital on growth. It is a well-stated empirical fact that private returns to a year of schooling (the increase in individuals' earnings when they attain school or a higher education institution for an additional year, the so-called "micro-Mincer" relationship) is somewhere between 7% and 10% (Mincer, 1974; Acemoglu and Angrist, 2001; Hsieh and Klenow, 2010). There is a significant debate that is still unresolved on to which extent education is contributing to national income, however, i.e., on the "macro-Mincer" relationship. If the "macro-Mincer" elasticity of national income on the average years of schooling is significantly greater than the "micro-Mincer" elasticity for private income, then education can be considered generating substantial positive externalities, and there is scope of subsidising educational attainment and funding educational institutions from state tax revenue (Hsieh and Klenlow, 2010; Caplan, 2018). Various studies report contradictory evidence. As such, Hsieh and Klenlow (2010) estimate the macro-Mincer coefficient at 20-30%. Acemoglu and Angrist (2014) use compulsory schooling laws to derive instruments for educational attainment and compute macro-Mincer at 9%, only marginally higher than their micro-Mincer coefficient of 7%. Caplan (2018) argues that social returns of education are smaller than private returns, interpreting it as evidence in favour of the signaling model of education. Acemoglu et al. (2014) report median OLS estimates at 26.3%, median TSLS estimates at

20.4% when controlled for rule of law as their institutional measure, using protestant missionaries, capped log settler mortality, and log initial population density as instruments. Therefore, the study opts to control for human capital to provide additional comparability with existing studies. There exist multiple potentially valid measures of human capital, including years of schooling, the percentage of educated labour force, and education investment as % of GDP (Barro, 2001; Barro and Lee, 2013). They are readily available from World Bank Development indicators database or from Barro and Lee (2013) for 161, 142, and 174 countries of the sample, respectively, therefore enabling the robustness check. To determine which of the human capital measures to utilise as a control, IV regressions with all three are run. Table 7 below presents the results.

Table 7. Human capital and growth

	Years of schooling		Educated, % labour force		Education investment, % of GDP	
	OLS	TSLS	OLS	TSLS	OLS	TSLS
Coefficient	0.4151	0.4593	0.0248	0.0323	0.1909	0.8705
	(19.5164)	(14.4345)	(1.4229)	(0.5161)	(3.0605)	(4.1304)
	0.0000	*0.0000*	*0.1570*	*0.6066*	*0.0026*	*0.0001*
Endogeneity	3.0436		0.0131		16.1893	
	0.0811		*0.9088*		*0.0001*	
Cragg-Donald F-stat	166.5054		9.4536		16.1893	

Among the three measures, two – years of schooling and education investment – have both significant effects in OLS and TSLS estimations, show signs of endogeneity and strong first-stage. As the relationship for years of schooling is much more pronounced and this measure is more frequently

used in human capital studies (Hsieh and Klenlow, 2010; Barro and Lee, 2013; Acemoglu et al., 2014), the study opts to use it as a control in further estimations to obtain more conservative estimates for institutional effects[1]. The OLS and TSLS estimator obtained (41.5% and 45.9%) are much greater than 26.4% or 30% reported by Acemoglu et al. (2014) and Hsieh and Kudlow (2010), respectively. It potentially identifies the aforementioned theoretical issues with protestant missions as an instrument for human capital, as protestant missionary activity might be causing democracy as well (Woodberry and Shah, 2004; Woodberry, 2012). The instrument used by this study (geographical pressure) is directly education-related and therefore is free from this endogeneity bias, while respective Durbin-Wu-Hausman and Cragg-Donald test evidence its general validity.

Table 8a. Robustness check – institutional effects with human capital controls.

	Economic freedom		Property rights		Judicial effectiveness		Government integrity		Tax burden		Government spending		Fiscal health	
	OLS	TSLS	OLS	TSLS	OLS	TSLS	OLS	TSLS	OLS	TSLS	OLS	TSLS	OLS	TSLS
Institutions	0.0455	0.0535	0.0319	0.0242	0.0259	0.0200	0.0301	0.0260	-0.0106	-0.0071	-0.0100	-0.0067	0.0028	0.0204
	(5.6992)	(2.2275)	(6.1530)	(2.0741)	(6.3578)	(1.8939)	(9.0912)	(3.2990)	(-1.4748)	(-0.4974)	(-2.7057)	(-0.1706)	(1.1881)	(0.6535)
	0.0000	*0.0274*	*0.0000*	*0.0397*	*0.0000*	*0.0601*	*0.0000*	*0.0012*	*0.1450*	*0.6196*	*0.0076*	*0.8647*	*0.2366*	*0.5144*
Human capital	0.3188	0.3435	0.2592	0.3371	0.3286	0.3969	0.2910	0.3455	0.4087	0.4521	0.3774	0.4183	0.4051	0.3563
	(12.7153)	(6.3470)	(8.3645)	(5.2785)	(13.3580)	(9.4754)	(12.0470)	(7.7786)	(19.3681)	(13.1373)	(15.3291)	(1.6697)	(17.9017)	(2.2193)
	0.0000	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.0970*	*0.0000*	*0.0279*
Endogeneity	3.4011		3.5813		5.3107		3.0359		2.6030		0.8255		2.8763	
	0.1826		*0.1669*		*0.0703*		*0.2192*		*0.2721*		*0.6618*		*0.2374*	
Cragg-Donald	5.9473		8.3261		11.8226		18.7096		29.8180		0.4500		0.4618	

[1] The study also estimated the equations with educated labour force and educational investment as human capital measures, and the results for institutional variables were even more statistically and economically significant than in the case of years of schooling.

Table 8b. Robustness check – institutional effects with human capital controls.

	Business freedom		Labour freedom		Monetary freedom		Trade freedom		Investment freedom		Financial freedom	
	OLS	TSLS	OLS	TSLS	OLS	TSLS	OLS	TSLS	OLS	TSLS	OLS	TSLS
Institutions	0.0340	0.0267	0.0101	-0.0151	0.0245	-0.3496	0.0301	0.0295	0.0141	0.0171	0.0211	0.0496
	(6.2201)	(1.4476)	(1.8770)	(-0.7118)	(2.5569)	(-0.1625)	(3.2789)	(1.8966)	(3.9118)	(1.7306)	(5.3976)	(3.1994)
	0.0000	*0.1497*	*0.0624*	*0.4776*	*0.0115*	*0.8711*	*0.0013*	*0.0597*	*0.0001*	*0.0855*	*0.0000*	*0.0017*
Human capital	0.2993	0.3642	0.3997	0.4739	0.3931	0.7746	0.3502	0.3817	0.3705	0.4027	0.3386	0.2769
	(11.5563)	(5.2569)	(16.8573)	(11.6349)	(18.6359)	(0.4002)	(12.8973)	(8.6267)	(17.0557)	(9.0583)	(14.8193)	(4.5230)
	0.0000	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.6897*	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.0000*
Endogeneity	2.8293		5.3452		3.4942		1.5757		3.0736		5.7358	
	0.2430		*0.0691*		*0.1743*		*0.4548*		*0.2151*		*0.0568*	
Cragg-Donald	4.9026		4.5941		0.0106		15.0841		11.2914		15.0841	

Table 8c. Robustness check – institutional effects with human capital controls.

	Polity index		Constraint on the executive		Legal origins		Ease of doing business		Rule of law		Constraint on government		Absence of corruption	
	OLS	TSLS	OLS	TSLS	OLS	TSLS	OLS	TSLS	OLS	TSLS	OLS	TSLS	OLS	TSLS
Institutions	0.0295	0.0119	0.0378	-0.0211	0.2321	1.1600	0.0299	-0.0017	4.3352	1.9398	2.8578	2.3083	3.5373	2.6723
	(1.1100)	(0.1926)	(0.7265)	(-0.1557)	(1.3049)	(1.2990)	(3.6179)	(-0.1141)	(6.6674)	(1.1503)	(5.8149)	(1.6456)	(7.6797)	(2.1456)
	0.2688	*0.8475*	*0.4687*	*0.8765*	*0.1938*	*0.1958*	*0.0004*	*0.9093*	*0.0000*	*0.2524*	*0.0000*	*0.1026*	*0.0000*	*0.0340*
Human capital	0.3993	0.4544	0.4056	0.4656	0.4105	0.4809	0.3141	0.4653	0.2736	0.3931	0.3352	0.3950	0.2628	0.3361
	(16.0919)	(10.1354)	(16.4653)	(9.8211)	(18.7473)	(12.6435)	(9.7607)	(7.7596)	(9.2537)	(5.5761)	(13.4470)	(7.4039)	(9.7555)	(5.0043)
	0.0000	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.0000*
Endogeneity	3.8044		3.8076		6.1462		4.3082		4.5293		2.9249		1.5292	
	0.1492		*0.1490*		*0.0463*		*0.1160*		*0.1039*		*0.2317*		*0.4655*	
Cragg-Donald	16.3815		12.4911		3.5136		8.9804		10.6963		8.0138		7.6316	

Table 8d. Robustness check – institutional effects with human capital controls.

	Government openness		Fundamental rights		Order and security		Regulation enforcement		Civil justice		Criminal justice	
	OLS	TSLS	OLS	TSLS	OLS	TSLS	OLS	TSLS	OLS	TSLS	OLS	TSLS
Institutions	3.1924	3.4169	2.8037	2.0011	2.0009	-2.2537	4.0383	1.9382	3.8909	1.1459	3.3346	-0.4217
	(5.1875)	(2.4902)	(4.1097)	(1.3724)	(3.1257)	(-1.0789)	(7.2943)	(1.3359)	(5.8119)	(0.5736)	(5.6527)	(-0.2692)
	0.0000	*0.0142*	*0.0000*	*0.1726*	*0.0023*	*0.2829*	*0.0000*	*0.1842*	*0.0000*	*0.5674*	*0.0000*	*0.7883*
Human capital	0.3081	0.3342	0.3249	0.3905	0.3660	0.5317	0.2832	0.3979	0.2909	0.4258	0.2984	0.4790
	(10.4411)	(5.3093)	(10.7631)	(6.0502)	(13.0838)	(6.9446)	(10.5271)	(6.7751)	(9.3280)	(5.9518)	(10.3409)	(6.4167)
	0.0000	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.0000*
Endogeneity	1.6028		2.2407		5.7119		6.0707		5.9188		10.0541	
	0.4487		*0.3262*		*0.0575*		*0.0481*		*0.0518*		*0.0066*	
Cragg-Donald	11.1216		13.5733		8.2905		8.2001		6.6910		11.5836	

Tables 8a-d above demonstrate the causal effect of institutions on growth when controlled for human capital. Human capital is shown to be an important determinant of economic growth. Only eight institutional variables – economic freedom, property rights, judicial effectiveness, government integrity, investment freedom, financial freedom, absence of corruption, and government openness – from the initial 26 retain statistical significance when human capital is accounted for. Notably, rule of law – the variable used to proxy for institutions in Acemoglu et al. (2014) – is of smaller magnitude in comparison to this study (1.94 against a median estimate of 1.12), yet statistically insignificant. This might be a result of a smaller sample and a different instrumental variable derivation strategy in Acemoglu et al. (2014). For eight significant indicators, TSLS estimators are on average 68% lower than the respective coefficients reported in Table 5, implying that a lot of variation, albeit not all, that could be initially explained by institutions is in fact attributable to human capital. The findings of this study are more favourable to

human capital than Acemoglu et al. (2014) – in their estimations, the coefficient on rule of law decreases from 1.35 to 1.12 (a 17% reduction, compared to 68% reduction reported above). The human capital estimates in this study are 24.5% lower than the TSLS estimate not accounting for institutions, very similar to 22.4% reduction reported by Acemoglu et al. (2014).

Overall, the findings of the study reinforce the consensus in the literature that institutions do matter for business and economic outcomes, while human capital, being a significant determinant as well, cannot explain all variations initially attributed to institutions. Furthermore, this study sheds some light on the long-standing problem of "unbundling institutions", evidencing the primary impact of property rights protection, absence from corruption, financial liberalisation, and government integrity and openness. That is consistent with the findings of Bhattacharyya (2009), verifying the claim that market-creating and market-stabilising institutions matter the most for growth, as well as with Acemoglu and Johnson (2005), arguing that property rights institutions (or, as they call them "political" institutions) are more important than contracting institutions. However, the study suggests that, unlike constraint on the executive and rule of law indicators predominantly used in the field, property rights, judicial effectiveness, government integrity, investment and financial freedom indices by Heritage, as well as absence of corruption and government openness by World Justice Project, yield considerably higher explanatory power over economic outcomes.

Conclusion

This study has successfully fulfilled its research objective and addressed the challenges in institutional research in international business and economics identified by scholars in the field (Mudambi and Navarra, 2002; Henisz and Swaminathan, 2008; Aguilera and Groggard, 2019; Cuervo-Cazurra et al., 2019ab).

It has developed a novel technique to derive institution-specific instrumental variables, using trade and geographic data to formalise and quantify the concept of external pressure exerted on governments by other states that might lead to reforms and institutional change that are relevant for the economic environment international businesses operate in (Cuervo-Cazurra et al., 2019a). It has found that among 26 different institutional variables initially considered, economic freedom, property rights, judicial effectiveness, government integrity, investment freedom, financial freedom, absence of corruption, and government openness are robust and significant causal determinants of economic growth, as evidenced by TSLS estimates with human capital controls. The application of this new method reinforces some findings of existing research on "unbundling institutions", such as the relative importance of market-creating and market-stabilising institutions (Bhattacharyya, 2009) and the primacy of property rights institutions over contracting institutions (Acemoglu and Johnson, 2005).

The study has broad implications for academics, business practitioners, and policymakers alike. For academics, it shows the applicability of external pressure as an instrument for institutions, that might be used in future international business and economics research to evaluate the impact of other factors, such as cultural dimensions or informal institutional arrangements, on various outcomes, as well as to investigate the phenomenon of institutional change and drift. For international business managers, this study has provided additional insight into the business context of economic, political, and legal environment and can inform strategic decision-making

on host country-MNE bargaining. For policymakers, this study can serve as an initial guide for prioritisation in terms of growth-enhancing reforms and investment promotion policies.

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