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Abstract

This paper examines the extent to which the quality of institutions, measured by the Economic Freedom of the World index, helps shape cross-country differences in economic complexity. To this end, I employ the intensity of ultraviolet radiation (UV-R) to isolate an exogenous source of variation in institutions, which helps circumvent endogeneity concerns. Empirical results indicate that the exogenous component of institutional quality exerts a strong and robust positive effect on economic complexity. The findings prevail after performing a battery of robustness tests. Furthermore, I find that institutions affect economic complexity by inducing human capital accumulation and strengthening incentives for innovative activities.

Key words: Economic Complexity, Economic Freedom, Institutions, Capabilities, Productive Structures.

JEL Classification: O43, O11, H11.

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1. Introduction

One of the most intriguing and difficult questions in economics is “why are some countries more affluent than others?” Recent contributions to this debate hold that economic complexity helps explain the pattern of economic growth and development in the world (Hidalgo & Hausmann, 2009; Hausmann & Hidalgo, 2011; Hausmann et al., 2014; Hartmann et al., 2017; Lee & Vu, 2019). Specifically, complexity reflects the level of sophistication of production of an economy. Thus, robust growth would be seen in countries whose productive structures are geared toward sophisticated products (Felipe et al., 2012). This line of inquiry has its roots in an earlier view asserting that economic development is the process of structural transformation by which resources are transferred from low-productivity (simple) industries toward high-productivity (complex) industries (e.g., Lewis, 1955; Rostow, 1959; Kuznets & Murphy, 1966; Kaldor, 1967; Chenery & Taylor, 1968). A key insight of this literature is that the mix of products an economy produces is strongly predictive of its economic performance.

A quantitative measure of the productive structure, however, has been equated with the contribution of agriculture, manufacturing and services to GDP for many decades. Hartmann et al. (2017) argue that this aggregate measure does not effectively capture the level of sophistication of a country’s production.[1] To address this concern, Hidalgo and Hausmann (2009) develop the Economic Complexity index (ECI), using highly disaggregated data at the product level. This indicator reflects the availability of productive capabilities that allow a country to produce more sophisticated products. Recent studies find that economic complexity is a strong and robust predictor of economic growth (Hidalgo & Hausmann, 2009; Felipe et al., 2012; Hausmann et al., 2014; Zhu & Li, 2017) and income inequality (Hartmann et al., 2017; Lee & Vu, 2019).

Moreover, Sweet and Maggio (2015) contend that economic complexity constitutes a good measure of a country’s innovative outputs. An increase in complexity implies an improvement in production capacity, obtained through acquiring new productive capabilities. It also captures the extent to which a country can utilize its knowledge and capabilities to create innovative outputs, which is relevant for economic prosperity. If economic complexity

[1] Hartmann et al. (2017) note that calculating the sophistication of the productive structure at broad categories (e.g., agriculture, manufacturing, and services) does not effectively reflect the complexity of industries that may vary across a diverse range of products. Hence, using highly disaggregated data at the product level is more informative for understanding economic sophistication.

determines the pattern of growth and development across countries, an interesting question emerges as "What determines economic complexity in the first place?"

There is a parallel literature arguing that the quality of institutions is a fundamental determinant of long-run economic performance. In particular, the institutional theory of comparative development can be traced back to the seminal contribution of North (1990, p. 3) who views institutions as "the rules of the game in a society, or, more formally, … the humanly devised constraints that shape human interaction." Good institutions, reflected in security of property rights or competitive markets, affect the relative returns to different productive and non-productive economic activities. As such, institutional quality fundamentally drives motivations for investment in human and physical capital and innovative activities. This ultimately helps explain wealth differences across the world.

On the empirical side, testing the effect of institutions on economic performance is challenging mainly because of the endogeneity of institutions. An influential study that endeavors to tackle this issue is the seminal article of Acemoglu et al. (2001). A novel contribution of this much-cited paper is that it employs the settler mortality rate as an exogenous source of variation in institutions to explain the global inequality of GDP per capita. The authors argue that Europeans adopted different colonization strategies depending on the disease environment of former colonies. In places where Europeans could healthily settle, they established inclusive institutions. In contrast, where the disease environment was unfavorable for Europeans to settle permanently, they set up extractive institutions. The early institutions persist until today, thus affecting economic development. Subsequent studies lend strong empirical support for the causal effect of institutions on economic progress (e.g., Easterly & Levine, 2003; Rodrik et al., 2004; Acemoglu & Johnson, 2005; Acemoglu et al., 2005; Knowles & Owen, 2010; Acemoglu et al., 2014). If the cross-country variation in income per capita is rooted in differences in institutions, we may well ask whether institutions fundamentally drive economic complexity in the first place.

The two lines of research discussed above offer two different views about the determinants of comparative prosperity across countries. Hence, they have been generally examined separately as competing alternatives. This paper goes beyond the current literature by bringing them together. In particular, this is the first study that empirically examines the effect of institutional quality on economic complexity. I hypothesize that institutions positively affect economic complexity through providing incentives for innovative activities and human capital accumulation. This notion is tested, using cross-sectional data for 108 countries to capture the long-run relationship between economic complexity and the quality of institutions. Empirical results lend strong credence to this supposition. I also perform a variety of sensitivity tests, none of which alters the main results.

This study contributes to a rapidly growing body of research examining economic complexity as a driver of economic development (Hausmann & Rodrik, 2003; Hausmann et al., 2007; Hidalgo & Hausmann, 2009; Hausmann & Hidalgo, 2011; Felipe et al., 2012; Hausmann et al., 2014; Hartmann et al., 2017; Zhu & Li, 2017; Lee & Vu, 2019). In particular, this paper attempts to uncover the institutional environment that fosters the ability to produce more value-added products (Zhu & Fu, 2013). By doing so, it contributes to a better understanding of the fundamental determinants of economic complexity. Additionally, I also provide some evidence of the mechanisms through which institutions transmit to complexity.

The current research is closely related to Zhu and Fu (2013) who explore the determinants of export sophistication, using a system GMM estimator. This paper, however, focuses on economic complexity, which resolves several issues of the export sophistication index of Hausmann et al. (2007). As noted by Felipe et al. (2012), complexity provides better information than the measure of export sophistication when it comes to reflecting the level of sophistication of production. I also use an exogenous instrument instead of relying on internal instruments as in their work. Importantly, this paper takes into account different theories of comparative development. Thus, I control for other fundamental determinants of economic performance. Failure to incorporate those potential confounders may yield biased and inconsistent estimates as discussed later.

Moreover, this paper advances the literature exploring institutions as the fundamental determinants of economic performance in different dimensions. First, it employs the Economic Freedom of the World index as the proxy for institutions. This indicator reflects a broad range of institutions and policies that co-evolve with economic development. As highlighted by Faria et al. (2016) and Bennett et al. (2017), this index is more informative for policymakers than unidimensional measures of institutions (e.g., constraints on the executive, rule of law, or risk of expropriation). Glaeser et al. (2004) note that defining institutional quality as security of property rights is too narrow, which necessitates using better measures of institutions. Nevertheless, other measures of institutional quality will be used for sensitivity analysis.

Second, I use the intensity of ultraviolet radiation, constructed by Andersen et al. (2016), as an instrumental variable for institutions. By doing that, it lends support to the new institutional theory of Ang et al. (2018). This instrument also has several advantages over the settler mortality rate of Acemoglu et al. (2001), which will be discussed later. However, the settler mortality rate will be used as an instrument in a robustness test. Finally, this paper contributes to an inconclusive debate on the relative significance of institutions and geography, with some studies finding that geographic endowments directly affect economic performance (Sachs, 2003; Carstensen & Gundlach, 2006), while other papers argue that geography affects prosperity only indirectly via institutions (Easterly & Levine, 2003; Rodrik et al., 2004). I find that the effect of institutions on complexity remains relatively robust to including a large set of geographic controls. By contrast, most geographic variables are individually insignificant at conventionally accepted levels when institutions are included in the regression. This adds evidence supporting the primacy of institutions.

The rest of the paper proceeds as follows. Section 2 discusses the theoretical framework for analysis. Section 3 describes the econometric methods used to estimate the causal effect of institutions on complexity. Section 4 provides the main empirical findings, followed by discussions of the results of robustness tests in Section 5. Next, Section 6 examines the channels of causality. The paper concludes by summarizing the results and discussing some implications for policymakers and future research.

2. Theoretical framework

An early line of inquiry emphasizes the role of formal and informal institutions in economic development (North, 1990; Williamson, 2000). Building upon this literature, subsequent studies attend to the quality of institutions as a key deep determinant of long-term economic performance (e.g., Hall & Jones, 1999; Acemoglu et al., 2001; Acemoglu et al., 2005; Knowles & Owen, 2010; Acemoglu et al., 2014). The institutional viewpoint, in particular, holds that institutions, often referred to as "rules of the game", stimulate investment in human capital and innovation, thus driving comparative prosperity. Consistent with this view, I argue that institutions exert a positive influence on economic complexity by enhancing the quality of human capital and providing incentives for innovative activities.

There is a vast literature linking economic institutions and human capital. Dias and Tebaldi (2012), for example, construct a micro-foundations model demonstrating that institutional quality plays an important role in affecting the pattern of human capital accumulation. Specifically, the incentives to improve education depend on returns to knowledge accumulation and the cost of obtaining such knowledge. Good institutions, reflected in security of property rights, enforcement of contracts, laws, and regulations, are the prerequisites of a well-functioning labor market. This affects the gains from obtaining knowledge, thus promoting human capital accumulation. Furthermore, human capital is essential for a country to obtain productive capabilities. This is because countries endowed with better human capital tend to learn and master complex production tasks faster (Zhu & Li, 2017). Hence, a more educated workforce fosters the ability to produce complex products. This proposition has gained empirical support in several studies. Zhu and Fu (2013), for instance, find that human capital positively affects the level of export sophistication. Hausmann et al. (2014) also document a positive relationship between human capital and economic complexity. Costinot (2009) develops a theoretical model illustrating that countries with better institutions and human capital are more likely to reveal comparative advantage in complex industries. Hence, good institutions help improve a country's human capital, thereby enhancing economic complexity.

Another argument for why institutions positively influence economic complexity rests upon a well-established literature linking institutions and innovation. There is a strong consensus in this area that good institutions provide incentives for innovative and entrepreneurial activities (e.g., Busenitz et al., 2000; Licht & Siegel, 2006). There are many empirical studies supporting the positive effect of institutions on innovation, mainly measured by R&D intensity and patents (Varsakelis, 2006; Tebaldi & Elmslie, 2013; Wang, 2013). Sweet and Maggio (2015) argue that economic complexity reflects improvements in a country's productive capacity. An increase in complexity, therefore, captures the extent to which an economy can create innovation through acquiring new productive capabilities to produce sophisticated commodities. Furthermore, complexity provides information about the ability to apply innovation in production, which is essential for economic prosperity (Hausmann et al., 2014). In this regard, the institutional environment plays an important role in providing incentives for acquiring new productive capabilities through which it improves economic structure. With respect to this view, Hausmann et al. (2007) demonstrate that the ability to export sophisticated goods critically depends on the institutional environment that stimulates entrepreneurs to engage in innovative activities. From this reasoning, I argue that institutional quality positively influences economic complexity via creating a conducive environment for innovative activities.

The existing literature provides strong support for the positive influence of institutions on economic complexity largely based on theoretical arguments. Figure 1 illustrates the positive association between institutions and economic complexity, which is in line with the above discussion. As far as I know, there is no empirical study linking these two variables. Given that there is a plausible theoretical connection between institutions and complexity, it is necessary to test the relationship between them. This paper, therefore, attends to institutional quality as a key factor that fundamentally drives cross-country differences in economic complexity. I suppose that two important mechanisms whereby institutions affect complexity include human capital accumulation and providing incentives for innovative activities.

3. Empirical approach

3.1. Model specification and data[2]

This paper aims to test the proposition that institutions positively affect economic complexity. To this end, I specify the following cross-country model:

$$ECI_i = \alpha + \beta INS_i + \gamma X_i + \varepsilon_i \quad [1]$$

where subscript i denotes country i. ECI is the Economic Complexity index, INS is the measure of institutional quality, X is the set of exogenous control variables, ε is the unobserved error term. β is the estimated coefficient of the effect of institutions on economic complexity, and is expected to have a positive sign.

Economic Complexity

The outcome variable is the ECI, obtained from the Observatory of Economic Complexity (https://atlas.media.mit.edu). The ECI reflects the sophistication of a country's economic structure. In particular, this indicator measures the availability of productive capabilities that allow countries to produce complex products. The number of productive capabilities in a country is dictated by information on the diversity of products it exports and the ubiquity of its products – the number of countries exporting a product (Hidalgo & Hausmann, 2009).[3] A country has more productive capabilities if it can export a diverse range of products. Sophisticated products, reflected in low ubiquity, are exported by only a few economies because they require many hard-to-find capabilities.[4] Combining this information by the so-called method of reflections, Hidalgo and Hausmann (2009) construct the ECI. The intuition is that complex economies are diverse and export products with low ubiquity. The average values

[2] See the online appendix for detailed information on the list of countries, variables' definitions and data sources.
[3] Hidalgo and Hausmann (2009) consider only products a country can export with revealed comparative advantage.
[4] According to Felipe et al. (2012), the most sophisticated products include machinery, chemicals, and metals while the simplest products are agricultural goods, raw materials, wood, textiles.

of the ECI, calculated over the period 2000-2010, are used in the benchmark model. I also check the sensitivity of results using alternative measures of economic complexity. Figure 2 represents the cross-country variation in the ECI. Accordingly, Japan, Germany, and Sweden are the most complex economies while the least complex economies include Nigeria, Sudan, and Cameroon.

Economic Institutions

I employ the Economic Freedom of the World (EFW) index, published by the Canadian Fraser Institute and the Heritage Foundation, as the baseline measure of economic institutions. This indicator is comprised of five sub-dimensions, including protection of property rights and legal system, size of government, freedom to trade internationally, access to sound money, and regulation of credit, labor and businesses. Hence, it captures the extent to which the institutional and policy environment within a country is consistent with freedom to enter markets and compete, personal choice, voluntary exchange, and protection of privately-owned property (Gwartney et al., 2004). The EFW index is scaled to take values between zero and one, with higher values denoting better institutions. An advantage of using this index is that it reflects a broad array of institutions and policies. For this reason, recent studies have employed the EFW index to estimate the impact of institutions on income per capita (e.g., Gwartney et al., 2004; Faria & Montesinos, 2009; Faria et al., 2016; Bennett et al., 2017). The EFW index is averaged across the period from 2000 to 2010. It can be seen from Figure 2 that there is significant variation in the quality of institutions across the globe. Good institutions, reflected in higher values of the EFW index, can be observed in Switzerland, United States, and United Kingdom. By contrast, Zimbabwe, Myanmar, Congo, and Angola suffer from much poorer institutions as reflected in lower values of the EFW index.

Control variables

Geography has been identified as a fundamental cause of comparative development in various studies. Some scholars argue that geography indirectly affects incomes through its effect on motivation to work, agricultural productivity and the quality of institutions (see, for instance, Bloom et al., 1998; Easterly & Levine, 2003; Rodrik et al., 2004). Another viewpoint asserts that geography directly drives comparative prosperity (Sachs, 2003; Carstensen & Gundlach, 2006). Following this line of research, I select geographic conditions as controls, including mean elevation, distance to the coast, and a landlocked dummy. Geographic conditions, such as being landlocked, may also capture the effect of barriers to the dissemination of knowledge and technologies that potentially affects economic complexity (Zhu & Fu, 2013). Furthermore, countries with land areas being suitable for agriculture may specialize in less complex products (Hausmann et al., 2007). Thus, land suitability for agriculture and the fraction of arable land, will be used as controls. It is also necessary to check whether the estimated effect of institutions on complexity is driven by unobserved continent-specific factors. Therefore, continent dummies, with Oceania being excluded as the base group, are included in Eq. [1] to control for regional heterogeneities. Additional control variables will be used for sensitivity analysis.

3.2. Estimation strategies

A major econometric issue when testing the institutional theory of comparative development stems from the endogeneity of institutions. In particular, there exists a reverse feedback from economic development to institutions because developed economies have better resources to improve their institutional environment. The endogeneity bias may also be caused by unobserved country-specific factors that we cannot rule out in a cross-country framework. Furthermore, institutions may be subject to measurement errors. These problems make OLS estimates biased and inconsistent.

For these reasons, obtaining consistent estimates of the effect of institutions on economic complexity requires finding an exogenous source of variation in institutions. In this regard, the "germs" theory of institutions, proposed by Acemoglu et al. (2001), is the most influential view. The authors employ the settler mortality rate as the instrumental variable for institutions. Their main argument is that Europeans adopted different colonization strategies depending on the disease environment of former colonies. Specifically, they established high-quality institutions in countries where the disease environment was favorable for settlement. By contrast, extractive institutions were set up in places where Europeans faced a higher risk of dying from disease. Although the settler mortality rate has been widely used as the instrument for institutions in subsequent studies, it is not free from criticism. First, Glaeser et al. (2004) find that the disease environment is more highly correlated with human capital than with institutions. They demonstrate that the historical event of European colonization matters for economic growth by affecting human capital and technology, and not by affecting institutions (Easterly & Levine, 2016). Second, Albouy (2012) shows that the settler mortality index suffers from a severe measurement issue, which raises some concerns about the validity of this instrument. Finally, data on settler mortality are only available for 64 former colonies (Albouy, 2012). Using this variable, therefore, imposes a huge constraint on the feasible sample size.

Given these limitations, this paper employs the intensity of ultraviolet radiation (UV-R) as an alternative instrument for institutions. This indicator is constructed by Andersen et al. (2016), using daily satellite-based data for ambient UV-R from NASA. A recent study by Ang et al. (2018) examines the role of the UV-R in explaining cross-country variations in institutional quality. The intuition is that countries with high UV-R intensity face a higher risk of eye diseases, for example, cataracts, which are the leading cause of blindness (Ang et al., 2018).[5] Based on this mechanism, Ang et al. (2018) contend that the long-lasting threat of eye diseases caused by the intensity of UV-R negatively affects a country's motivations to invest in cooperation by building institutions. Furthermore, the risk of becoming blind acts as a barrier to investments in skills and technologies, which impedes the ability to accumulate a food surplus. This, in turn, is detrimental to specialized activities such as institution building, which require a food surplus. In other words, the negative effect of blindness on investments in skills and technologies is associated with fewer people specialized in law-creation activities. The UV-R, ultimately, deters skills and experience with establishing and maintaining institutions (Ang et al., 2018). Using data for more than 120 countries, Ang et al. (2018) find that UV-R is a strong and robust predictor of institutional quality.[6]

Motivated by their findings, this paper employs UV-R as a potential instrument for institutions. However, I do recognize that the "germ" theory of institutions is an influential viewpoint in this literature. It is also important to note that the aim of this paper is not to compare the relative importance of the effect of the settler mortality rate and the UV-R on institutions. Nevertheless, a salient advantage of using the UV-R is that it does not impose further constraints on the sample size. Findings, therefore, may suggest a generalized pattern between institutions and economic complexity across a large number of countries. Later, I also perform a sensitivity test by using settler mortality as an instrument for institutions.

4. Results and discussions

4.1. IV-2SLS estimates

Baseline estimates are reported in Table 1. Specifically, Panel A shows the results of the first-stage regression while the second-stage estimation results are shown in Panel B.

[5] As noted by Ang et al. (2018), previous research has documented a positive association between UV-R and eye diseases (see, for instance, Gallagher & Lee, 2006; Linetsky et al., 2014; Löfgren, 2017).
[6] To be a valid instrument for institutions, the UV-R also needs to satisfy the exclusion restrictions. This will be discussed in Section 4.2.

Column (1) of Table 1 presents the unconditional estimates in which no control variables are included. The estimated coefficient of UV-R is statistically significant at the 1% level. This indicates that the UV-R is negatively correlated with the quality of institutions. This finding is consistent with the hypothesis of Ang et al. (2018) that countries with higher UV-R will suffer from poorer institutions. It also supports the relevance of the UV-R as the instrument for institutions. The second-stage estimates demonstrate that the estimated coefficient of the EFW index is positive and statistically significant at the 1% level. This shows that the exogenous component of institutions, generated by the UV-R, positively affects the ECI.

Geographic controls are added to the second column of Table 1. All geographic variables, except the fraction of arable land, are statistically insignificant at conventionally accepted levels. The p-value of the F-test of joint significance of those variables is 0.323. Hence, we fail to reject that the estimated coefficients of geographic endowments are jointly indistinguishable from zero. This indicates that geographic conditions, except arable land, are not shown to have a statistically significant effect on ECI. Furthermore, this result provides evidence that institutions are an important driver of cross-country differences in ECI. Controlling for geographic endowments yields a slight decrease in the size of the effect of institutions on ECI. However, the effect of institutions on ECI is still precisely estimated. Results remained largely unchanged when continent dummies are included in the regression, as shown in column (3) of Table 1. All variables are added in column (4) of Table 1, which does not alter the main findings. Overall, the positive significant effect of institutions on ECI is robust to controlling for geography and continent heterogeneities.

The size of the estimated coefficients suggests that institutions exert a substantial effect on the ECI. For example, the EFW index of Thailand and Japan is 6.71 and 7.72, respectively. The difference between these two countries equals to 1.01, which is approximately one standard deviation of the EFW index in the sample (see Table A1 in the online appendix). According to the estimated coefficients in column (4) of Table 1, a one-unit increase in the exogenous source of variation in institutions, generated by the UV-R, implies a 1.146-unit increase in the ECI. Thus, if Thailand instead experienced a level of the EFW index similar to Japan, the expected increase in the ECI of Thailand would be 1.157 units, which is approximately 2.5 times its initial ECI value (0.482), a substantial increase.

A number of diagnostic tests are also reported in Table 1. First, the endogeneity test of Hausman (1978) indicates that we can reject the null of exogeneity of institutions at the 1% level of significance. This is consistent with our discussion earlier that institutions are not exogenous in the ECI equation, which motivates the use of the instrumental variable. Second, the value of the Sanderson and Windmeijer (2016) F-test for the excluded instrument is bigger than the rule-of-thumb value of 10 in all cases, which implies that UV-R is not a weak instrument. Third, I perform the test of robust inference with weak instruments of Anderson and Rubin (1949). The low p-value indicates that we can reject the null hypothesis that the coefficient of the endogenous variable is zero at the 1% level. This implies that the effect of institutions on ECI is robust even in the presence of a weak instrument. Finally, I conduct the test of weak instruments of Cragg and Donald (1993). The high values obtained in all cases reveal that we can reject the null hypothesis that UV-R is weakly correlated with institutions.

4.2. Potential violation of the exclusion restriction

To my knowledge, there is no study linking UV-R and ECI. However, we cannot rule out the possibility that UV-R may exert a direct effect on ECI other than through its influence on institutions. If this channel of causation exists, the exclusion restriction is violated, invalidating the IV-2SLS estimates. In particular, the intensity of UV-R may affect productivity through shaping the disease environment and the motivation to work (Andersen et al., 2016). Thus, UV-R may exert a direct impact on income levels. Because economic complexity and GDP per capita are highly correlated, UV-R may also have some slight influence on complexity through disease ecology. To minimize the violation of the exclusion restriction, I control for additional geographic endowments, including latitude and malaria (Table 7). The baseline estimates are robust to this consideration.

Importantly, GDP per capita and complexity are not perfectly correlated. For instance, Chile and Malaysia have the same level of income but Malaysia's ECI is much higher than that of Chile (Hartmann et al., 2017). This ultimately helps explain why Malaysia enjoys a better distribution of income (low inequality) compared with Chile (Hartmann et al., 2017). Moreover, Australia has a relatively low ECI despite its high level of income (Hartmann et al., 2017). Although UV-R may directly affect income, its effect on complexity is much less clear-cut. Nevertheless, there may exist other channels through which UV-R affects complexity that I do not account for in the regression. For this reason, I perform the plausibly exogenous bounds test, proposed by Conley et al. (2012), to check the sensitivity of the baseline findings to partial deviations from the perfect exogeneity assumption.

As argued by Owen (2017), the validity of instruments used in the determinants of long-run development literature largely relies on "telling a good story" to justify the exogeneity assumption. Conley et al. (2012) contend that the exclusion restriction is often debatable because the disturbance term is unobserved. Motivated by this challenge, Conley et al. (2012) develop the method of union of confidence intervals (UCI) that allows us to calculate upper and lower bounds of the effect of institutions on ECI when the instrument (UV-R) partially deviates from the assumption of perfect exogeneity. This approach can be demonstrated in the following equation:

$$Y = \beta X + \gamma Z + \varepsilon$$

where Y is the outcome variable (ECI), X is the endogenous regressor (EFW), Z is the instrumental variable (UV-R), and ε is the disturbance term. The exclusion restriction conventionally requires $\gamma = 0$, which means that UV-R exerts no direct effect on ECI other than via its influence on institutions. Following the approach of Conley et al. (2012), this assumption is assumed to be partially violated. In particular, γ is assumed to be different from zero, taking some values in a given interval ($\gamma \in [-\delta; +\delta]$). Hence, we can estimate the confidence interval bounds of β, associated with each value of γ. δ is the estimated coefficient of the direct effect of UV-R on ECI in the above equation.

Estimation results are presented in Table 2. Accordingly, the estimated effect of institutions on ECI is well above zero when the perfect exogeneity assumption is partially relaxed. The 95% confidence intervals for β, associated with different values of γ, are illustrated in Figure 2. Accordingly, none of those confidence intervals includes zero. Hence, the causal effect of institutions on economic complexity is consistently significant when γ deviates from zero. This suggests that the baseline findings are insensitive to potentially violating the exclusion restriction, suggesting that the UV-R is "plausibly exogenous" (Conley et al., 2012).

To further check for the sensitivity of the baseline findings to violating the exclusion restriction, I conduct the fractionally resampled Anderson-Rubin test, following Berkowitz et al. (2012). In particular, Berkowitz et al. (2012) demonstrate that the Anderson-Rubin test over-rejects the null hypothesis of no effect of institutions on ECI when there is a minor deviation from the exogeneity assumption. To address this concern, the authors modify the AR test but allow for a slight violation of the exogeneity condition. More specifically, Berkowitz et al. (2012) construct the fractionally resampled Anderson-Rubin (FAR) test, based on the jackknife histogram estimator of Wu (1990). The aim is to obtain reliable but conservative inference when the instrument is not perfectly exogenous. According to results in Table 3, we can reject the null hypothesis of no effect of institutional quality on ECI at the 1% level of significance.

Overall, the UCI and FAR test results suggest that we can draw valid inference on the effect of institutions on ECI even when the orthogonality condition is slightly violated. This provides empirical support to the hypothesis that institutional quality positively affects ECI.

4.3. Using an alternative instrument for institutions

The discussion above supports the validity of the UV-R as the instrument for institutions. The results are also insensitive to some degree of deviation from the exclusion restriction. This section further explores whether the baseline findings are driven by the choice of instrument. To this end, I employ the log of settler mortality rate of Acemoglu et al. (2001) as an alternative instrument for institutions. Estimation results are represented in column (1) of Table 4. The first-stage estimates indicate that the settler mortality rate is negatively correlated with institutions. The estimated coefficient of the mortality rate is statistically significant at the 5% level. This is consistent with the "germ" theory of institutions of Acemoglu et al. (2001). Turning to the second-stage estimates, the effect of institutions is still positive and statistically significant at the 1% level. However, the value of the Sanderson-Windmeijer F-test of excluded instruments and the Cragg and Donald (1993) test of weak instruments is smaller than the rule-of-thumb value of 10. This suggests that the mortality rate is weakly correlated with institutions, measured by the EFW index. However, the quantitative aspect of the baseline estimates remains broadly unchanged even when the instrument is just weakly correlated with institutions.

The sample size, however, decreases significantly when the settler mortality rate is used. For this reason, I re-estimate the baseline regression for the sub-sample of countries whose data on the mortality rate are available. This allows comparing the baseline estimates with those in column (1) of Table 4 more precisely. As shown in column (2), the UV-R is strongly correlated with institutions. This justifies the relevance of the benchmark instrument even for a much smaller sample of former colonies. Furthermore, the impact of institutions on ECI remains precisely estimated. Hence, the baseline results are robust to the choice of instruments for institutions. Next, I use both instruments in column (3) to partially test for the validity of the exclusion restriction. Results indicate that we fail to reject the null hypothesis that the instruments are valid at conventionally accepted levels of significance. This partially provides some evidence for the exogeneity of the instruments. It is also important to note that these two variables are highly correlated. Thus, including them in one regression may mask the effect of each variable on the quality of institutions in the first-stage regression, thereby causing a weak instruments problem. Nevertheless, the second-stage results in column (3) are quantitatively similar to the baseline estimates.

5. Robustness tests

5.1. Using other measures of Economic Complexity and institutions

To check the sensitivity of the baseline estimates, I employ other measures of economic complexity. First, I have used the ECI values calculated over the period 2000-2010 in the benchmark model. The results, therefore, may reflect the short-run relationship between ECI and institutions. Hence, it is necessary to check the sensitivity of the estimates using an alternative period. To do this, I use the ECI values, averaged over the period from 1964 to 2010 for which data are available, as shown in column (1) of Table 5. Second, I employ the improved ECI index (ECI+), constructed by Albeaik et al. (2017), in column (2) of Table 5. Albeaik et al. (2017) apply the method of reflections of Hidalgo and Hausmann (2009) to construct the ECI+ measure, but they take into consideration how difficult it is to export each product. This indicator is also available from the Observatory of Economic Complexity. Finally, I adopt the Economic Fitness index, developed by Tacchella et al. (2012), in column (3) of Table 5. This indicator, obtained from the World Bank's World Development Indicators, also reflects a country's ability to export sophisticated products. As shown in Table 5, the estimated effect of institutions on economic complexity remains statistically significant at the 1% level, which is consistent with the baseline findings.

As argued earlier, the EFW index reflects a cluster of institutions and policies that provide a more comprehensive understanding of institutions. Specifically, the fact that institutions are often equated with security of property rights is too narrow (Glaeser et al., 2004). Furthermore, the EFW index may be more informative for policy-makers than the unidimensional measures of institutions (e.g., constraints on the executive, rule of law, or risk of expropriation) as highlighted by Faria et al. (2016) and Bennett et al. (2017). However, I re-estimate the benchmark model, using the commonly used measures of institutions to check the robustness of the core estimates. Results reported in Table 6 indicate that the positive effect of institutions on ECI is largely insensitive to this consideration.

5.2. Controlling for other effects

This section checks whether the results are robust to controlling for potential confounders.

First, I include additional covariates as shown in Table 7. In particular, additional geographic controls are added to the first column, including precipitation, latitude, longitude, and the fraction of the population at risk of contracting malaria. Those variables, except malaria, are individually insignificant at conventionally accepted levels. Furthermore, the inclusion of those controls does not quantitatively alter the baseline findings. Next, I control for the effect of ethno-linguistic fractionalization, obtained from La Porta et al. (1999). Ethnic and linguistic diversity has been shown to affect several development outcomes, including institutions (Alesina et al., 2003). Ethnolinguistically diversified countries may benefit from the diversity of ideas, thus strengthening the ECI. The empirical findings, therefore, can be biased if we fail to consider this effect. As shown in column (2), the estimated effect of institutions on the ECI is still highly precise. Next, the third column adds a country's world share of natural resource reserves (e.g., gold, iron, silver, zinc, and oil), obtained from Acemoglu et al. (2001). There is a line of research arguing that natural resources are essential inputs of industrial upgrading (Zhu & Fu, 2013). Other studies contend that they are harmful to institutions and economic growth, which is often referred to as the resource curse (e.g., Van der Ploeg, 2011). The estimated coefficient of institutions, however, is still significant at the 1% level after controlling for the effect of natural resources. The baseline estimates also prevail after all those variables are added to column (4).

Second, the long-term comparative development literature has identified several early development factors as drivers of economic performance. The core results may be biased if we do not properly control for those factors. For example, the historical experience with state-level institutions may affect both the quality of institutions and the outcome variable (e.g., Bockstette et al., 2002; Putterman & Weil, 2010; Ang, 2013; Borcan et al., 2018). I control for this effect using the measure of state antiquity of Borcan et al. (2018), calculated from 3500BCE to 2000CE. Additionally, the legal origins theory of finance, proposed by La Porta et al. (1998), argues that common-law countries with better security of property rights are more financially developed than civil-law counterparts. Legal origins are also found to affect the quality of institutions (La Porta et al., 1999), and they may exert some influence on the ECI. For this reason, the second column of Table 8 adds dummies representing legal origins from Klerman et al. (2011), with a civil-law dummy being excluded as the base category.[7] There is another

[7] Here, I follow the legal origins classification of Klerman et al. (2011), which is largely similar to that of La Porta et al. (1998) except for the group of mixed-law countries. According to Klerman et al. (2011), those countries initially adopted French civil law but it was only partly replaced by British common law later. Thus, their legal traditions contain some elements of both civil law and common law.

argument that the timing of agricultural transition, beginning 10,000 years ago, helps explain comparative prosperity (Hibbs & Olsson, 2004). I account for this effect, using Putterman's (2006) dataset of the length of time elapsed since the Neolithic revolution. As also highlighted by Spolaore and Wacziarg (2009), genetic distance from the technology frontier is harmful to economic performance because it acts as a barrier to knowledge and technology diffusion. Similarly, genetic diversity has a hump-shaped relationship with income levels, as suggested by Ashraf and Galor (2013). The results can be biased if genetic characteristics are correlated with both institutions and ECI. Thus, genetic variables are added to column (4) and (5) of Table 8. Those historical variables are highly correlated because they capture different aspects of early development. Hence, I do not include all of them into one regression. Estimation results in Table 8 indicate that the coefficients of institutions remain precisely estimated after accounting for those historical confounders.

Finally, Table 9 includes some "proximate" determinants of ECI.[8] Specifically, trade openness may facilitate the dissemination of knowledge across countries, thus enhancing the ability to produce sophisticated products (Zhu & Fu, 2013). The level of financial development, measured by domestic credit to the private sector as a percentage of GDP, may affect industrial upgrading via providing resources for innovation (Wang, 2013). In addition, government spending may positively affect ECI through providing public goods such as education, legal systems, and public order, as suggested by Sweet and Maggio (2015). I take the averaged data for those variables over the preceding decade (1990-1999) to mitigate bias caused by reverse causality. It is important to note that institutions are different from those "proximate" determinants of ECI in the sense that they provides a deeper understanding of cross-country differences in complexity. Hence, studies examining the fundamental determinants of comparative prosperity have generally excluded the "proximate" determinants because they may capture some of the effect of institutions on economic performance (see, for instance, Acemoglu et al., 2001; Acemoglu et al., 2005; Knowles & Owen, 2010; Acemoglu et al., 2014). However, the baseline estimates are largely insensitive to including those variables.[9]

[8] Acemoglu et al. (2005) argue that human capital, physical capital, and innovation are proximate determinants of economic growth because they require further explanation. By contrast, institutions, among others, are considered as "deep" determinants because they offer a more fundamental explanation of comparative prosperity.
[9] The quality of human capital and innovation are also potential "proximate" determinants of the ECI. As discussed in Section 2, they are two mechanisms through which institutions affect ECI. For this reason, they are not included here, but I will discuss this possibility in Section 6 in some detail.

5.3. Panel data estimates

The major objective of this paper is to explore the role of institutions in explaining the cross-country variation in ECI. Hence, setting up a cross-country regression is relevant for the current study, given that this paper aims to investigate the long-term relationship between institutions and ECI. Furthermore, most of the geographic controls and the instrumental variable used in the benchmark model are relatively stable over time. Hence, the cross-sectional regression that utilizes averaged data over time allows us to capture the long-run relationship between ECI and the quality of institutions.

For these reasons, estimating cross-sectional regressions is a conventional approach in the comparative development literature.[10] However, I recognize that we cannot properly control for the effect of unobserved country-specific factors in a cross-sectional framework. In this regard, cross-country estimates may yield a spurious relationship between institutional quality and ECI if the unobserved country-specific characteristics are correlated with both these variables. To test this possibility, this paper estimates a dynamic panel data including non-overlapping 5-year periods from 1970 to 2010, using the system GMM estimator of Blundell and Bond (1998).[11] The model is specified as follows:

$$ECI_{i,t} = \alpha + \delta ECI_{i,t-1} + \beta INS_{i,t} + \gamma X_{i,t} + \varepsilon_i + \mu_t \qquad [2]$$

where subscripts i and t stand for country i and period t, respectively. ECI is the Economic Complexity Index. INS denotes institutions, measured by the EFW index. X is a set of control variables, including trade openness, the fraction of arable land in total land area, the level of financial development, and government spending. The choice of those controls is discussed earlier, and they are similar to the proximate determinants included in Table 9. The baseline controls, except arable land, are time-invariant, and are therefore excluded in Eq. [2]. The time-period is mainly dictated by the availability of data. ε and μ reflect unobserved country- and time-specific factors, respectively.

Estimating Eq. [2], using the system GMM estimator, helps solve several econometric issues. First, the first-differenced equations, besides the level equations, control for the effect of country-specific characteristics. Second, the lagged dependent variable accounts for the dynamic characteristics of the ECI (Sweet & Maggio, 2015). Third, the instrument used in the

[10] See Owen (2017) for an extensive review about econometric methods used in this literature.
[11] Previous research investigating the determinants of economic growth has popularly used panel data including non-overlapping 5-year periods (see, e.g., Berg et al., 2018).

benchmark model is not applicable in this context because it is time-invariant. Hence, I use lags of endogenous regressors as the valid instruments, following Blundell and Bond (1998). A major issue of using this approach is the problem of instrument proliferation, as highlighted by Roodman (2009). To deal with this problem, this paper collapses the instrument set and uses specific lags as instruments, following Roodman (2009).[12]

The system GMM estimates that potentially cater for the endogeneity bias are presented in Table 10.[13] The consistency of these estimates, however, critically depends on some diagnostic tests. As suggested by Roodman (2009), the number of instruments should be ideally less than the number of cross-sectional units, which is the number of countries in this context. In addition, the validity of the instruments is checked by performing autocorrelation tests. The high p-values of the AR(2) test support evidence of the absence of second-order serial correlation in the error term, justifying the validity of the instruments (Arellano & Bond, 1991). The results of the Hansen tests of over-identifying restrictions are reported to check whether the instruments satisfy the exclusion restriction. Accordingly, we fail to reject the null of exogeneity of the instruments, which provides some evidence for the validity of the internal instruments used in both the difference and level equations. I collapse the instrument in column (1) and (2). The effect of institutions on ECI is positive and statistically significant at the 1% level. However, the number of instruments, shown in column (2), is very close to the number of countries, which may bias the results. Next, I restrict the instrument set by using the second- and the third-order lags.[14] The baseline estimates remain unchanged, but the problem of instrument proliferation remains unresolved (column 2-3). Hence, I restrict the instrument set by both collapsing and using specific lags in column (5) and (6). Then, the number of instruments is much smaller than the number of countries.

According to results shown in Table 10, the estimated coefficients of the lagged ECI is positive and statistically significant at the 1% level, except in column (5). This justifies the use of a dynamic panel data specification. It also suggests that ECI is path-dependent, implicating

[12] See Roodman (2009) for discussions on collapsing the set of instruments. This is done via specifying the suffix "collapse" and/or the specific number of lags in the STATA command "xtabond2". All variables in the right-hand side of Eq. [2] are treated as endogenous regressors, thus need to be instrumented by their lags.

[13] Before discussing the system GMM estimates, I estimate Eq. [2] but exclude the lagged dependent variable, using fixed-effects and pooled OLS regression. Results reported in Table A4 in the online appendix show that institutions have a positive significant effect on economic complexity in all cases. This is consistent with the baseline findings. These results, however, do not necessarily reflect causation due to some concerns about endogeneity bias.

[14] Results remain quantitatively unchanged, but the number of instruments keeps growing significantly when deeper lags are used. Hence, I use the second- and third-order lags as valid instruments.

that countries with an initial high level of ECI are able to produce more sophisticated products in the future. This notion is consistent with the findings of Hausmann and Rodrik (2003), Hausmann et al. (2007), Hidalgo et al. (2007) and Hidalgo and Hausmann (2009). Moreover, the effect of institutions on the ECI is still precisely estimated at the 5% level of significance. Hence, pooling data across time does not alter the baseline findings.

The results, however, may be inconsistent if the instruments are weakly correlated with the endogenous variables even in large samples. Using the internal instruments does not allow us to perform some conventional diagnostics tests of instrument strength as discussed in the baseline estimates (Berg et al., 2018).[15] Furthermore, the instrument set, albeit being kept smaller than the number of countries, is still relatively large (e.g., 31 instruments are used in column 6 of Table 10). Although the system GMM estimates allow us to control for the unobserved country-specific characteristics, I argue that the cross-sectional estimates, using an exogenous instrumental variable, are more informative when exploring the long-run relationship between ECI and institutions. In particular, the system GMM estimator comprises of a set of regressions in differences and in levels. Taking first-difference removes the cross-country variation in both ECI and institutions, which is the main interest of this study. Furthermore, the cross-sectional regression is more informative about the long-run relationship than the system GMM estimates, given that we have a relatively short time-series dimension. For these reasons, I maintain the use of the baseline estimates to draw inference on the causal relationship between institutions and ECI.

5.4. Additional robustness tests

Results of some additional sensitivity tests are reported in the online appendix.

First, I check whether the baseline results are robust to allowing for a potential non-linear relationship between complexity and institutions. To this end, I perform the Ramsey RESET test of functional form misspecification, using estimates in column (4) of Table 1. Results shown in Table A5 indicate that we fail to reject the null of correct functional form specification of the baseline estimates, which is supportive of a linear relationship. Next, I further check potential nonlinearities by performing a non-parametric estimation using Kernel-weighted local polynomial smoothing. As shown in Figure A1, we hardly find any evidence of a

[15] Berg et al. (2018) point out that the system GMM estimator has gained popularity in the growth determinants literature. The authors, however, are concerned that conventional tests of instrument strength are not applicable when the internal instruments are adopted. This raises some concerns about the reliability of the system GMM estimates even in large samples.

nonlinear correlation between institutions and complexity, except for a few outliers at the upper and lower bound of economic freedom. I further test for nonlinearity by allowing institutions to enter the benchmark model in a quadratic form. Results presented in Table A6 do not support the quadratic relationship between institutions and complexity. Thus, the baseline estimates are insensitive to checking for nonlinearities.

Second, I investigate the sensitivity of the baseline results to excluding some influential observations in Table A7. Specifically, I re-estimate column (4) of Table 1 but exclude countries whose standardized residuals are larger than 1.96 or smaller than -1.96. I also remove countries with a Cook's distance bigger than the conventional value calculated by four over the number of observations. In addition, I conduct robust regressions, following Li (1985). In all cases, the quantitative aspect of the baseline findings remains largely unchanged.[16]

6. Transmission channels

This paper hypothesizes that institutions exert a positive influence on ECI via enhancing human capital accumulation and providing incentives for innovative activities. This section, therefore, tests these mechanisms by controlling for the measure of human capital and innovation in the benchmark model. The effect of institutions on ECI may be less precisely estimated or become statistically insignificant when I account for the mediating channels of influence.[17] In particular, this paper employs the cognitive skills measure of human capital used by Hanushek and Woessmann (2012a). The reason behind this is that cognitive skills are a stronger predictor of cross-country differences in economic growth compared to years of schooling (Hanushek & Woessmann, 2008, 2012a, 2012b).[18] Furthermore, I use the percentage of R&D expenditure in GDP as the proxy for innovative activities.[19] Data are taken in 2000 to minimize the endogeneity bias due to reverse causality.[20] I also estimate the effect of institutions on the transmission channels.

Estimation results are shown in Table 11. They indicate that the quality of institutions positively affects human capital and innovative activities (column 1 and 3). In particular, the

[16] Australia is an interesting outlier with good institutional quality and a low level of the ECI. This requires further investigation in future research, which will be discussed in Section 7.
[17] This explains why "proximate" determinants of economic performance (e.g., human capital, physical capital, and technology) are not included as controls in the fundamental determinants literature (see, e.g., Acemoglu et al., 2001; Acemoglu et al., 2005; Knowles & Owen, 2010; Acemoglu et al., 2014).
[18] As Hanushek and Woessmann (2012a) note "a year of schooling in Peru is assumed to create the same increase in productive human capital as a year of schooling in Japan."
[19] This has been the widely used proxy for innovation (see, e.g., Wang, 2013).
[20] Note that ECI is averaged across 2000 to 2010.

estimated effect of institutions on human capital accumulation and innovation is positive and statistically significant at the 1% level. This finding is in line with the arguments in Section 2. As represented in column (2) and (4), the effect of institutions on ECI is less precisely estimated once I control for human capital and innovation. Furthermore, the size of the effect decreases considerably, relative to the baseline estimates. In contrast, the effect of human capital and innovation on ECI is statistically significant at the 1% level (column 2 and 4). These results suggest that institutions affect ECI via its influence on human capital and innovation, which is supportive of the main hypotheses discussed in Section 2.

Using the estimates in Table 11, I further conduct the mediation tests of MacKinnon et al. (1995) to test the transmission channels. Results are shown in Table 12. The null hypothesis of the mediation tests is that the indirect effect of institutions, working through human capital and innovation, equals zero. The test results indicate that we can reject the null hypothesis in all cases at the 5% level of significance (Table 12). This suggests that human capital and innovation are two important channels through which institutions positively affect ECI.

7. Conclusion

A recent line of research focuses on economic complexity, a measure of a country's productive structure, as a strong predictor of comparative prosperity across the globe. However, much less is known about the root causes of complexity. This paper, therefore, goes beyond the current literature by examining the extent to which institutions help explain cross-country differences in economic complexity. For this purpose, I employ the EFW index as the proxy for institutions. This indicator reflects a cluster of institutions and policies, which offers a more comprehensive coverage of the institutional environment. I posit that institutions act as a catalyst for human capital accumulation and innovative activities, thus enhancing complexity.

This study employs cross-sectional data for 108 countries to test the above proposition. To address endogeneity concerns, I use the ultraviolet radiation (UV-R) index of Andersen et al. (2016) to isolate the exogenous variation in institutions. Results are consistent with the findings of Ang et al. (2018) that UV-R is strongly correlated with cross-country differences in institutions. In addition, I find that the exogenous component of variation in institutions, generated by UV-R, exerts a strong and positive effect on economic complexity. Using the UCI method of Conley et al. (2012), I find that this finding is largely insensitive to partially deviating from the exclusion restriction. The fractionally Anderson-Rubin test results also suggest that the positive effect of institutions on ECI remains broadly unchanged when the exogeneity assumption is slightly violated (Berkowitz et al., 2012). Moreover, the baseline findings are robust to using the conventional settler mortality instrument for institutions of Acemoglu et al. (2001). To further check the sensitivity of the baseline estimates, I conduct several robustness tests, including using other measures of institutions and complexity and controlling for additional covariates. The effect of institutions on ECI remains very precisely estimated after performing these tests. Furthermore, I estimate a dynamic panel data, using the system GMM estimator that accounts for unobserved country- and time-specific factors and the endogeneity bias. The baseline estimates are broadly insensitive to this consideration.

The empirical findings of this paper offer several implications. First, future studies examining the link between economic complexity and economic performance should take into consideration the effect of institutions on complexity. Given that the literature on economic complexity is relatively thin but growing quickly, this paper provides some suggestions about the choice of control variables for subsequent studies. Second, this paper is the first study that employs UV-R as an instrumental variable for institutions to overcome the endogeneity bias. This lends empirical credence to the new institutional theory of sunlight, proposed by Ang et al. (2018). In particular, countries with high intensity of UV-R will have less motivation to invest in institutional building, thereby suffering from poor institutions. Future research can also employ this potential instrument for institutions without limiting the sample size to former colonies as when using other conventional instruments (e.g., legal origins and the log of settler mortality).

Third, this study establishes a link between institutions and economic complexity across countries. However, there are significant variations in the quality of institutions and economic complexity across regions within a country. Subsequent studies, therefore, may focus on a single country to explore the link between subnational institutions and the ECI. Besides the cross-country data on economic complexity, the Observatory of Economic Complexity also provides the Product Complexity index at a highly disaggregated level. Thus, a potential avenue for future research is to investigate the link between subnational institutions and firms' capacity to produce and export sophisticated products. This micro-level approach may provide promising policy implications. Finally, this study sheds some light on the channels through which institutions affect economic complexity, including human capital and innovation. Hence, the effect of institutions on the ECI can be magnified by these mechanisms. This implies that policies aiming to improve the institutional environment, the quality of human capital, and innovative activities at the same time may have a larger effect on the productive structure.

The final thought of this paper is that exploring the relationship between economic complexity and institutions in Australia would be an interesting extension to the current research. As illustrated in Figure 2, Australia experiences an intermediate level of economic complexity. Part of the reason for this is that natural resources constitute a considerable proportion of its exports (Hartmann et al., 2017). In addition, Hartmann et al. (2017) argue that the measure of economic complexity of Hidalgo and Hausmann (2009) may underestimate the actual level of complexity in Australia. By contrast, Australia managed to establish a good institutional environment over the past decades, being among the freest economies in the world today (Figure 3). This can be partially explained by the effect of the massive migration of Europeans, beginning in the sixteenth century, as found by Acemoglu et al. (2001). For these reasons, I believe that Australia is a special case that requires further investigation.

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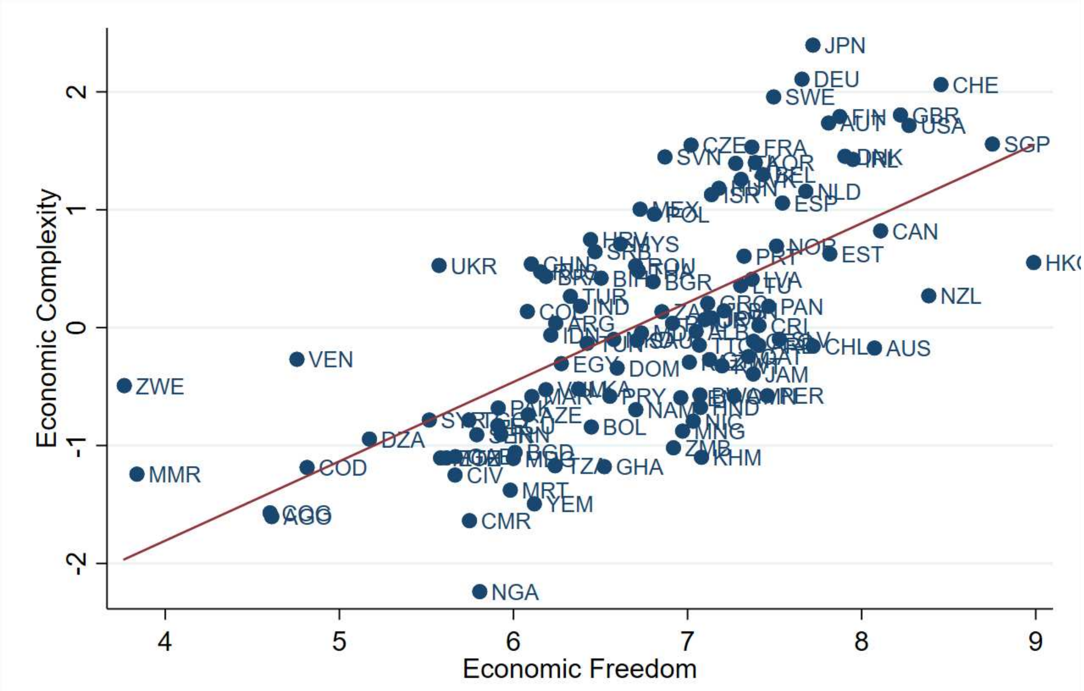

Figure 1. Scatter plot of Economic Freedom and Economic Complexity

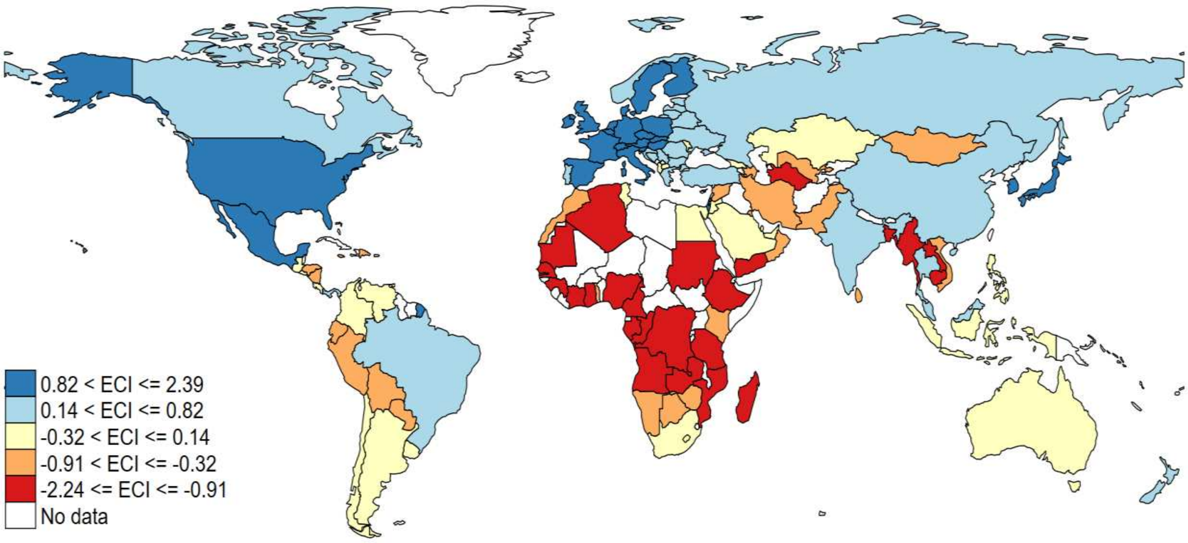

Figure 2. Spatial distribution of Economic Complexity across countries

Notes: The figure illustrates the cross-country variation in Economic Complexity over the period 2000-2010. High values (shaded blue) reflect more complex economies while low values (shaded red) reflect simple economies.

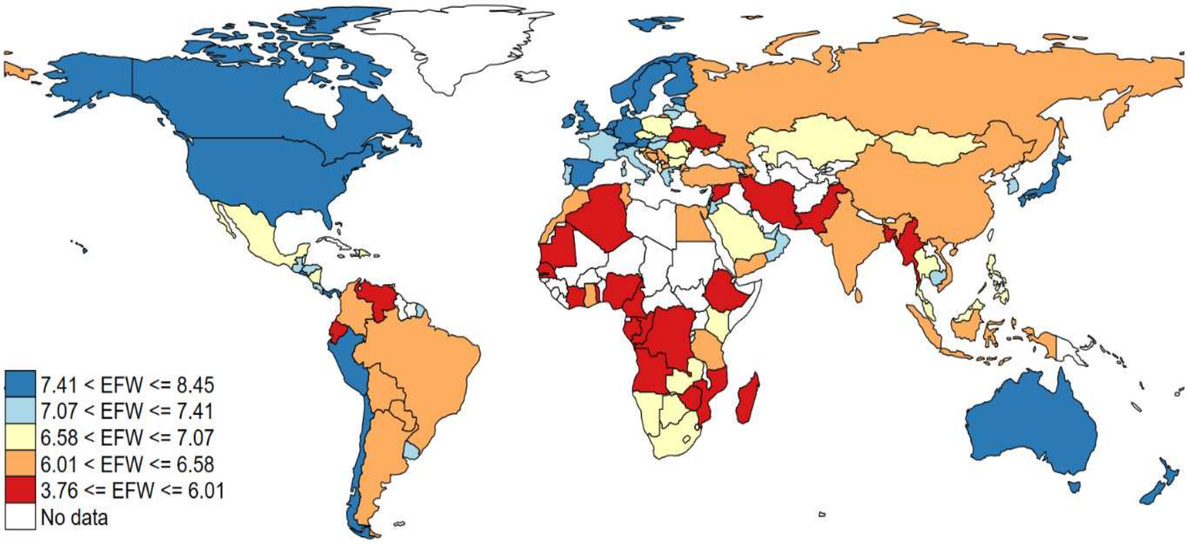

Figure 3. Spatial distribution of Economic Freedom across countries

Notes: The figure illustrates the cross-country variation in the institutional quality, measured by the Economic Freedom across the period 2000-2010. High values (shaded blue) reflect good institutions while low values (shaded red) reflect the low institutional quality.

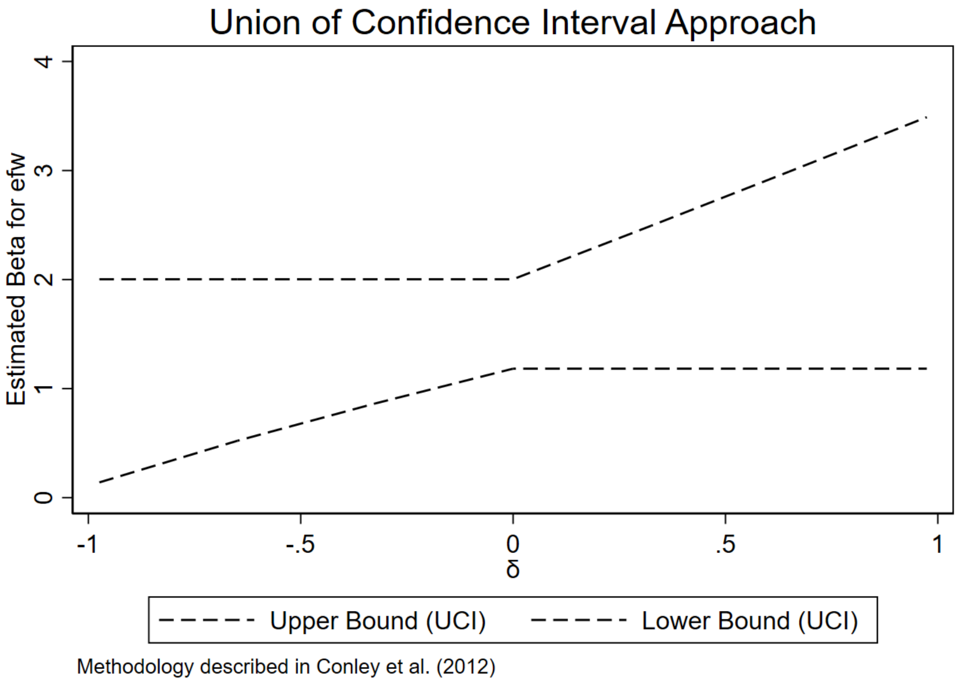

Figure 4. Bounds of the 95% confidence intervals of β, associated with different values of δ

Table 1. Institutions and economic complexity, main results

	(1)	(2)	(3)	(4)
	Unconditional estimates	Include geographic controls	Include continent dummies	Full specification (Baseline estimates)
Panel A. First-stage regression. Dependent variable: Economic Freedom				
Log (UV-R)	-0.815***	-1.069***	-0.801***	-1.158***
	[0.117]	[0.130]	[0.176]	[0.236]
Panel B. Second-stage regression. Dependent variable: Economic Complexity				
Economic Freedom	1.593***	1.351***	1.097***	1.146***
	[0.209]	[0.165]	[0.289]	[0.262]
Mean Elevation		-0.042		-0.087
		[0.269]		[0.232]
Distance to coast		0.000		-0.000
		[0.000]		[0.000]
Landlocked dummy		-0.004		-0.030
		[0.344]		[0.293]
Land suitability		-0.513		-0.266
		[0.344]		[0.351]
Arable land		0.014**		0.005
		[0.007]		[0.007]
Africa dummy			0.910	1.027
			[0.772]	[0.737]
America dummy			0.681	0.822
			[0.588]	[0.594]
Europe dummy			1.412***	1.386**
			[0.544]	[0.553]
Asia dummy			1.030	1.173*
			[0.647]	[0.630]
Observations	108	99	105	99
R-squared	-0.153	0.185	0.450	0.414
F-test of excluded instruments	48.42	67.43	20.79	24.10
Anderson-Rubin	116.46	80.67	12.35	19.10
Wald test [p-value]	[0.000]	[0.000]	[0.001]	[0.000]
Cragg-Donald weak identification test	36.766	47.182	11.958	18.208
Endogeneity [p-value]	0.000	0.000	0.011	0.003

Notes: Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All control variables and continent dummies are included in the first-stage regression but are omitted to conserve space. The intercept estimates are not reported for brevity.

Table 2. Plausibly exogenous bounds test

Variable	Lower Bound	Upper Bound
Economic Freedom	0.141	3.489
Constant	-28.508	4.024

Notes: This table presents upper and lower bounds of 95% confidence intervals for the effect of institutions on economic complexity. Results are estimated using the UCI method of Conley et al. (2012), performed by the "plausexog" command in STATA.

Table 3. Fractionally resampled Anderson-Rubin (FAR) test

	Full sample statistic	Full sample p-value	FAR p-value	Reps.	N
AR-test	42.299	0.000	0.000	10000	108

Notes: I use the baseline estimates in column (4) of Table 1 to perform the test. The null hypothesis is that institutional quality has no significant effect on ECI. Results are estimated using the "far" command in STATA, following Berkowitz et al. (2012).

Table 4. Using log of settler mortality as an instrument for institutions

	(1)	(2)	(3)
First-stage regression. Dependent variable is Economic Freedom			
Log of settlers' mortality	-0.239**		-0.177*
	[0.113]		[0.093]
Log (UV-R)		-1.414***	-1.167**
		[0.417]	[0.434]
Second-stage regression. Dependent variable is Economic Complexity			
Economic Freedom	1.593***	1.048***	1.148***
	[0.547]	[0.274]	[0.263]
Observations	53	52	52
R-squared	-0.231	0.463	0.384
F-test of excluded instruments	4.47	11.48	7.87
Anderson-Rubin Wald test	17.08	18.38	20.34
[p-value]	[0.000]	[0.000]	[0.000]
Cragg-Donald weak identification test	2.847	10.824	6.465
Hansen test of over-identification			0.316
Baseline Controls	Yes	Yes	Yes
Continent FE	Yes	Yes	Yes

Notes: Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. See also notes to Table 1.

Table 5. Using different measures of economic complexity

	(1)	(2)	(3)
Panel A. First-stage regression. Dependent variable is Economic Freedom			
Log (UV-R)	-1.158***	-1.158***	-1.181***
	[0.236]	[0.236]	[0.236]
Panel B. Second-stage regression			
Dependent variable	ECI (1964-2010)	ECI+	Fitness Index
Economic Freedom	1.176***	0.855***	1.311***
	[0.223]	[0.227]	[0.483]
Observations	99	99	95
R-squared	0.398	0.527	0.318
F-test of excluded instruments	24.10	24.10	24.96
Anderson-Rubin Wald test	29.58	14.02	6.10
[p-value]	[0.000]	[0.000]	[0.016]
Cragg-Donald weak identification test	18.208	18.208	19.336
Baseline controls	Yes	Yes	Yes
Continent FEs	Yes	Yes	Yes

Notes: Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. See also notes to Table 1.

Table 6. Using alternative measures of institutions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
First-stage regression. Dependent variables are different measures of institutions used in the second-stage regression							
Log [UV-R]	-1.784***	-1.809***	-1.655***	-1.612***	-1.199***	-1.355**	-2.064***
	[0.276]	[0.277]	[0.267]	[0.246]	[0.263]	[0.553]	[0.459]
Panel B. Second-stage regression. Dependent variable is Economic Freedom							
Rule of Law	0.749***						
	[0.127]						
Control of Corruption		0.739***					
		[0.127]					
Government Effectiveness			0.808***				
			[0.124]				
Political Stability				0.829***			
				[0.180]			
Voice and Accountability					1.115***		
					[0.266]		
Constraints on Executive						0.978**	
						[0.390]	
Risk of Expropriation							0.646***
							[0.109]
Observations	105	105	105	105	105	104	86
R-squared	0.763	0.768	0.805	0.557	0.549	-0.482	0.715
F-test of excluded instruments	47.71	42.52	38.26	42.81	20.80	6.01	20.20
Anderson-Rubin Wald test	19.91	19.91	19.91	19.91	19.91	19.47	17.01
[p-value]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Cragg-Donald weak identification test	41.556	43.306	37.916	39.031	19.480	6.039	20.872
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Continent FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. See also notes to Table 1.

Table 7. Including additional control variables

	(1)	(2)	(3)	(4)
	Add geographic controls	Add ethnolinguistic diversity	Add natural resources variables	Add all variables
First-stage regression. Dependent variable is Economic Freedom				
Log (UV-R)	-1.091***	-1.139***	-0.963***	-0.774**
	[0.328]	[0.289]	[0.225]	[0.309]
Panel B. Second-stage regression. Dependent variable is Economic Complexity				
Economic Freedom	0.853***	1.424***	1.291***	1.148**
	[0.267]	[0.343]	[0.318]	[0.482]
Precipitation	0.019			0.010
	[0.016]			[0.019]
Malaria	-0.918***			-0.823**
	[0.261]			[0.363]
Latitude	0.027			-0.559
	[0.570]			[0.896]
Longitude	0.027			-0.294
	[0.280]			[0.412]
Ethnolinguistic Diversity		-0.006		0.164
		[0.341]		[0.348]
Gold			-0.004	-0.013
			[0.012]	[0.017]
Iron			0.014	-0.004
			[0.104]	[0.085]
Silver			-0.018	-0.008
			[0.097]	[0.083]
Zinc			0.016	0.048
			[0.100]	[0.095]
Oil			0.000	0.000
			[0.000]	[0.000]
Observations	99	87	92	84
R-squared	0.613	0.273	0.400	0.566
F-test of excluded instruments	11.06	15.57	18.37	6.28
Anderson-Rubin Wald test [p-value]	9.96 [0.002]	23.24 [0.000]	21.56 [0.000]	9.75 [0.003]
Cragg-Donald weak identification test	9.802	12.948	11.026	4.838
Baseline Controls	Yes	Yes	Yes	Yes
Continent FE	Yes	Yes	Yes	Yes

Notes: Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. See also notes to Table 1.

Table 8. Controlling for the effect of historical confounders

	(1)	(2)	(3)	(4)	(5)
First-stage regression. Dependent variable is Economic Freedom					
Log (UV-R)	-1.158***	-1.181***	-1.088***	-1.138***	-1.112***
	[0.237]	[0.209]	[0.232]	[0.222]	[0.243]
Second-stage regression. Dependent variable is Economic Complexity					
Economic Freedom	1.146***	1.159***	1.219***	1.144***	1.040***
	[0.258]	[0.257]	[0.284]	[0.260]	[0.259]
State history	0.642				
	[0.508]				
Common Law LO		-0.617**			
		[0.273]			
Mixed Law LO		0.094			
		[0.420]			
Neolithic Transition			0.096*		
			[0.057]		
Predicted Genetic Diversity				-48.533	
				[65.420]	
Predicted Genetic Diversity squared				31.804	
				[46.633]	
Genetic Distance to the USA					-0.066
					[0.221]
Observations	99	99	99	99	94
R-squared	0.420	0.452	0.381	0.424	0.499
F-test of excluded instruments	23.95	31.65	21.86	26.33	20.96
Anderson-Rubin Wald test	19.13	22.01	19.54	20.64	13.76
[p-value]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Cragg-Donald weak identification test	18.031	23.203	16.064	18.377	15.348
Baseline Controls	Yes	Yes	Yes	Yes	Yes
Continent FE	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. See also notes to Table 1.

Table 9. Controlling for the effect of contemporary confounders

	(1)	(2)	(3)	(4)
First-stage regression. Dependent variable is Economic Freedom				
Log (UV-R)	-1.148***	-0.743***	-1.062***	-0.748***
	[0.251]	[0.256]	[0.248]	[0.269]
Second-stage regression. Dependent variable is Economic Complexity				
Economic Freedom	1.067***	1.233***	1.041***	0.892**
	[0.246]	[0.465]	[0.267]	[0.378]
Trade openness	-0.732**			-0.670**
	[0.287]			[0.308]
Financial development		0.253		0.502
		[0.462]		[0.379]
Government size			0.948	1.899
			[1.850]	[1.733]
Observations	96	84	93	78
R-squared	0.483	0.234	0.468	0.541
F-test of excluded instruments	20.91	8.45	18.28	7.68
Anderson-Rubin Wald test	17.97	13.78	13.78	7.52
[p-value]	[0.000]	[0.000]	[0.000]	[0.008]
Cragg-Donald weak identification test	17.445	4.661	14.389	4.305
Baseline Controls	Yes	Yes	Yes	Yes
Continent FE	Yes	Yes	Yes	Yes

Notes: Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. See also notes to Table 1.

Table 10. Two-step system GMM estimates of panel data

Dependent variable: Economic Complexity	Collapsed instruments		Restricted instruments		Collapsed and restricted instruments	
	(1)	(2)	(3)	(4)	(5)	(6)
ECI_{t-1}	0.921***	0.864***	0.914***	0.915***	0.298	0.919***
	[0.076]	[0.055]	[0.038]	[0.025]	[0.213]	[0.073]
Economic Freedom	0.082**	0.110***	0.052*	0.066**	0.147**	0.103**
	[0.034]	[0.030]	[0.031]	[0.029]	[0.057]	[0.040]
Trade openness		0.074*		0.046		0.039
		[0.038]		[0.041]		[0.105]
Arable land		0.436		0.315**		-0.516
		[0.270]		[0.127]		[0.807]
Financial development		-0.018		0.019		-0.063
		[0.048]		[0.044]		[0.089]
Government size		-0.003		-0.002		-0.008
		[0.004]		[0.005]		[0.006]
Observations	808	663	808	663	808	663
Number of countries	113	110	113	110	113	110
Number of instruments	34	102	50	162	26	31
AR(1) test [p-value]	0.004	0.008	0.004	0.008	0.062	0.010
AR(2) test [p-value]	0.547	0.687	0.550	0.670	0.928	0.594
Hansen test of over-identification [p-value]	0.133	0.609	0.072	1.000	0.541	0.760
Difference-in-Hansen test of [p-value]						
Instruments for levels	0.106	0.607	0.242	1.000	0.863	0.873
Instruments for initial economic complexity	0.670	0.508	0.680	1.000	0.544	0.463
Instruments for IV-type	0.039	0.827	0.139	1.000	0.351	0.792
Period FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. See also notes to Table 1.

Table 11. Mediated effects of human capital and innovation

	(1)	(2)	(3)	(4)
First-stage regression. Dependent variable is Economic Freedom				
Log (UV-R)	-0.936***	-0.778***	-1.273***	-0.904***
	[0.254]	[0.280]	[0.324]	[0.309]
	(1)	(2)	(3)	(4)
Panel B. Second-stage regression				
Dependent variable:	Human Capital	ECI	Innovation	ECI
Economic Freedom	0.842***	0.634*	1.207***	0.339
	[0.224]	[0.366]	[0.293]	[0.236]
Human capital		0.708***		
		[0.270]		
Innovation				0.467***
				[0.143]
Observations	59	59	49	49
R-squared	0.196	0.667	0.368	0.794
F-test of excluded instruments	13.55	7.70	15.40	8.53
Anderson-Rubin Wald test	17.15	2.14	11.82	1.36
[p-value]	[0.000]	[0.150]	[0.001]	[0.251]
Cragg-Donald weak identification test	10.999	5.176	20.263	8.745
Baseline Controls	Yes	Yes	Yes	Yes
Continent FE	Yes	Yes	Yes	Yes

Notes: Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. See also notes to Table 1.

Table 12. Mediation tests

	Human Capital			Innovation		
	Coeff.	Std. error	Test statistic [p-value]	Coeff.	Std. error	Test statistic [p-value]
Sobel	0.596**	0.277	2.151[0.031]	0.564**	0.220	2.559[0.010]
Aroian	0.596**	0.284	2.101[0.036]	0.564**	0.224	2.514[0.012]
Goodman	0.596**	0.271	2.204[0.027]	0.564***	0.216	2.607[0.009]

Notes: *** and ** denote statistical significance at the 1% and 5% level, respectively. Following MacKinnon et al. (1995), the test statistics are calculated as follows: Sobel: $z = \beta_1\beta_3/SQRT\left(\beta_3^2 s_{\beta_1}^2 + \beta_1^2 s_{\beta_3}^2\right)$, Aroian: $z = \beta_1\beta_3/SQRT\left(\beta_3^2 s_{\beta_1}^2 + \beta_1^2 s_{\beta_3}^2 + s_{\beta_1}^2 s_{\beta_3}^2\right)$, and Goodman: $z = \beta_1\beta_3/SQRT\left(\beta_3^2 s_{\beta_1}^2 + \beta_1^2 s_{\beta_3}^2 - s_{\beta_1}^2 s_{\beta_3}^2\right)$, where β_1 reflects the effect of institutions on the mechanisms, β_3 measures the effect of the mechanisms on ECI, s_{β_1} and s_{β_3} are the standard errors of β_1 and β_3, respectively. These values are reported in Table 11. The coefficients shown above equal β_1 times β_3, which measures the indirect effect of institutions on ECI working through human capital and innovation (MacKinnon et al., 1995).