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Approaching reverse causality with a Two-Stages Panel VAR: an application to growth and institutions

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Abstract: A number of florid academic debates in the field of empirical economics are characterized by vivid discussions around the issue of reverse causality. One of these is the discourse around the relationship between institutional quality and growth. This thesis attempts to propose a flexible two-stages econometric procedure for the incorporation of reverse causality in the empirical analysis and for the empirical study of path dependence through IRA, and it presents an application to the institutions-growth relationship. Whereas the first stage is aimed at demeaning the variables of interest of measurable time-and-space variation and of both time and country fixed effects, the second estimates a Panel Vector Autoregressive model on the demeaned variables by simple OLS: thanks to the presence of the first stage, results are robust to the inclusion of a pooled intercept, which remains statistically insignificant, and to the misspecification of causal priority when making identifying restrictions. It is also found that previous attempts to estimate the institutions-growth relationship by means of a PVAR – without the inclusion of a first stage – greatly overestimate both the magnitude of parameters and the persistence of path dependence, as represented by Impulse Response Functions, due to omitted variable bias. Finally, it is recognized that the current computational possibilities of statistical softwares with respect to PVARs pose quite strict limitations to the application of the proposed procedure, and a future project aimed at programming more flexible packages is envisaged.

Keywords: Panel Vector Autoregressive models, Reverse causality, Institutions, Economic Development, Social capital

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Till min familj, och till Sverige.

1 Introduction

The interest of economic research in the relationship between institutions and economic development – and, occasionally, culture –, has experienced a tremendous increase since the publication of Acemoglu, Johnson and Robinson's: "The Colonial Origins of Comparative Development: An Empirical Investigation", in 2001. In fact, not only has this paper given rise to a sustained academic debate, it has also led to the consideration of institutions in empirical studies as a praxis rather than a peculiarity. It has started a whole field of empirical research in the field, drawing a neat separation from neoclassical growth theory, which constituted the framework of all previous empirical studies on growth and, partially, on institutions – see, for instance, Barro (1991) and Keefer and Knack (1997). In doing so, it has turned into a textbook example of instrumental variable approaches in microeconometrics as a solution to endogeneity problems, especially reverse causality.

However, it also presents a number of criticalities, of which some have been clearly highlighted in lengthy and heated debates. Among these, it is worth mentioning the point raised by Sachs (2001) regarding the actual meaning of its empirical results: by using colonizers' mortality rates as an instrument to resolve the reverse causality issue that arises from regressing a measure of economic development on one of institutional quality, the authors are simply measuring the continued (and present) effect of geography on economic growth – argues Sachs –, for mortality rates induced by malaria are by all means still a current concern in developing countries characterized by a disadvantaged geography, i.e. one that is prone to tropical diseases proliferation and that impedes their eradication by condemning the country to a poverty trap through lack of resources.

Econometrically speaking, the only way a similar diatribe could be solved would be to control explicitly for geographical characteristics in the regression. Because, however, it is virtually impossible to control for all relevant ones in a cross-country analysis such as the one proposed by Acemoglu, Johnson and Robinson (and in the previous empirical attempts it largely superated), the only viable solution would be to control for unobserved heterogeneity in a longitudinal analysis: in other and simpler words, to include country fixed effects, which is only allowed when considering panels rather than cross sections. Nevertheless, in the specific example of Acemoglu, Johnson and Robinson (2001), the whole empirical structure of the study is dependent on using cross sections: because colonizers' mortality rate inevitably refer to a fixed moment of the past and, therefore, lacks time-variability, its use is not compatible with the inclusion of country fixed effects: a suitable instrument in a longitudinal setting should in fact vary both in time and across sections (i.e. countries). Finding such an instrument could be a way towards the resolution of the never ending debate with Jeffrey Sachs, yet it would present the usual issues of any instrumental variable approach, that is the hardly obtainable compliance with both the relevance and (strict) exogeneity assumptions for a correct identification, which have detracted from numerous empirical approaches in the topic of growth and institutions (indeed, Acemoglu, Johnson and Robinson's merit, within a cross-sectional framework, is much related to the use of a good instrument).

In this regard, and in extremely simplified words, this study proposes to address reverse causality by embracing it rather than trying to exclude it. This is done by partially reverting from micro to macroeconometrics, and in particular by estimating a Panel Vector Autoregressive model (henceforth PVAR), that is a dynamic system of endogenous variables, which estimates at the same time the impact of institutional quality on economic development, and vice versa. Considering a two-way relationship is then expected to bring the advantage of an even greater accuracy as compared to single equation models, both in terms of consistency and efficiency of the estimated parameters. Because of the computational effort that would derive by including all necessary controls in a PVAR, a first stage is envisaged in order to demean the two core variables, institutional quality and per capita GDP, from all relevant and observable variation, via a two-way fixed effect model specified for each of the two variables. The estimated coefficients, then, shall not embed the correlation of the variables of interest with third covariates, country (sectional) characteristics such as geography, and global occurrences, whose influence is taken care of during the first stage. The analysis is carried out on a longitudinal dataset comprising 164 countries and 21 years (from 1990 to 2010): the vast dataset and the substantial variation across the temporal and spatial dimension for the variables of interest was envisaged to further strengthen accuracy of estimates. However, despite the necessity to apply this econometric procedure to a specific research question in order to deliver a sound proposal for future research, it is worth underlying the procedure itself constitutes the core of this master thesis, for it can be generalized to any similar instance of endogeneity driven by simultaneous correlation. In the context of the institutions-growth relationship, there have been only two attempts of estimating a PVAR model to overcome simultaneity to the current date, which are however both characterized by a narrower consideration of econometric issues arising when applying such a model, as Section 2.1 will discuss with greater care.

There is, however, an additional characteristic of Acemoglu, Johnson and Robinson's analysis, and in the institutions-growth relationship investigation more in general, which this procedure aims at addressing. Namely, they assume the persistence of institutions' quality from the late nineteenth century to the present based on previous qualitative studies: such assumption is moreover crucial to their identification strategy, for it motivates the use of settlers' mortality rate as a valid instrument. In fact, they elucubrate the chain of causations to be:

(potential) settler mortality $\Rightarrow$ settlements

$\Rightarrow$ early institutions $\Rightarrow$ current institutions

$\Rightarrow$ current performance.

FIGURE 1: Source: Acemoglu, Johnson and Robinson (2001)

They are, however, not alone in treating such persistence as an assumption. The concept of path dependence, as it is more precisely defined, was coined by Paul David, and has been frequently indicated as a conceptual breakthrough in economics, particularly in the fields of political and development economics, and in historical sociology. Indeed, it has been largely claimed to be the key missing conceptual framework to explain the persistence of institutional and cultural outcomes, as correlated with economic development (North, 1991; Mahoney, 2000; Williamson, 2000; Greener, 2005). At the same time, however, these same scholars, and the same theorizer of the concept (David, 1994; 2007), also underline how its arbitrary use and the myriad of definitions it was attributed – often unfortunately trivial, as the simple claim that "history matters" – might turn into abuse, creating confusion and detracting value from the conceptual application, in academic research, of path dependence.

The importance of establishing and following a clear and universally adoptable definition of path dependence, and the necessity to test its presence empirically are crucial not only for empirical studies on institutions and growth, but more in general to avoid discarding a potentially fundamental concept for the understanding of societal evolution and to envisage an easier analysis tool for future research. Luckily enough, the use of a PVAR model, while providing an alternative approach to the issue of reverse causality, is also beneficial to testing and measuring path dependence according to its most precise definition, the one it was given by its theorizer Paul David, although with some limitations which are inherently contingent to the availability of the data in use.

The thesis proceeds as follows. Section 2 provides a literature review aimed not only at positioning this study, but also at summarizing the existing definitions of institutional quality and culture in light of the empirical strategy proposed, which is instead outlined in Section 4. Section 3 will describe the data in use and their main characteristics, whereas section 5 illustrates the empirical results. Their robustness is tested in Section 6, and Section 7 discusses some limitations of this study. Finally, Section 8 concludes the paper.

2 Literature review

There are many aspects this literature review aims at covering. As a premise, it will discuss two studies which proposed a similar approach to the present one, and underline their respective differences. Then, it will discuss how previous literature suggests to consider culture in the empirical strategy, and outline how the latter has been defined so far, to provide the reader with the necessary context to evaluate the choice of data in use to proxy it. Hence, it will illustrate the issue of reverse causality in the relationship between institutions and growth, in the literature, hopefully motivating the empirical choices that will follow in Section 4. Finally, it will provide greater detail as to how the empirical strategy matches the original definition of path dependence.

2.1 Previous PVAR approaches to the institutional quality and growth relationship

There are two studies that share the main purpose of this thesis, that is to include reverse causality between growth and institutional quality by estimating a PVAR. These are Chong and Calderón (2000) and Goés (2015): this section will illustrate what they consist of and in what they differ from this thesis.

Let us begin by overviewing Chong and Calderón (2000). Firstly, it should be noted that it was published before Acemoglu, Johnson and Robinson (2001): it constitutes therefore a notable contribution to the literature, because it recognizes the necessity to include both causality and feedback effects in the institutions and growth relationship related literature before the latter even gained momentum in the academic discourse. The study applies a PVAR model, whereby the data comprise 55 countries and 24 years (from 1972 to 1995). Following the neoclassical growth current, it includes as controls initial primary education enrollment, to account for human capital endowment in a given country, initial GDP, and regional dummies, for Latin America and Africa. The authors' main objective lies in testing the presence of feedbacks from GDP growth to institutional quality improvement, and in obtaining a more precise measure of their relationship compared to the microeconometric case which assumes a single direction of causality – a purpose it shares with the present study. Nevertheless, in order to apply OLS, they average both variables in 5 or 10 years periods, based on the consideration that institutional quality indicators are rather persistent and therefore constant in the short and medium run. Therefore, besides not measuring the persistence of institutional quality, for it is assumed, the authors end up with a rather limited sample size ($T < 5$), which poses a threat to the consistency and statistical significance of the parameters: this issue is further worsened by the fact that they also run the model separately for developing and developed countries (lowering the sample size on the sectional dimension), and by the omission of fixed effects from the model. The latter is on the one hand understandable, because the inclusion of controls already represents a notable computational burden for the PVAR, but on the other hand problematic, given the intention to obtain unbiased estimators and precise measures of causal and feedback effects as a percentage of total linear dependence of the two variables, in presence of a limited sample size.

Some of these issues are already taken care of in Goés (2015) which, however, steps backward for what concerns ensuring unbiasedness of the estimators. Indeed, on a sample of 114 countries and 10 years, the author employs the Arellano-Bond procedure (which implies the use of first differences (FD), hence the subtraction of fixed effects) to apply GMM/IV method to the PVAR estimation: whereas, as he mentions, this allows to include a pooled intercept in the individual equations, it also implies a reduction of the time dimension of the dataset, further cut by running the estimation once again separately for developed and developing countries, with similar sample size potential issues as those discussed for Chong and Calderón (2000). Indeed, the motivation for splitting the sample is different, and it lies in the intention of conducting separately the Impulse Response Analysis (henceforth IRA) aimed at measuring the persistence of an exogenous shock to either institutional quality or economic growth in the system, which is then not assumed to be in place. The latter, as Section 4.3 will explain in greater detail, is done also in the present study, but on the whole sample.

With respect to these two studies, a first improvement this thesis attempts to bring forward is the focus on both an accurate measure of causal and feedback effects, and on the conceptualization of the IRA as an empirical application of the study of path dependence of the considered variables. Most importantly, however, this study enlarges the two just illustrated by including the first stage of estimation, preliminary to the PVAR, aimed at demeaning the two variables of interest so to eliminate as much as possible any potential omitted variable bias (thereby including relevant time-and-space controls and both sectional and time fixed effects) and at allowing the application of a simple OLS method without including a constant in individual equations – which is at the moment not allowed by the existing range of PVAR software packages. Moreover, through the first stage, it allows to include in the analysis the role played by culture, which previous literature points out as a relevant determinant of potentially both institutional quality and economic growth, as explained in the next section. Overall, the empirical strategy proposed could constitute a valid approach also in other topical areas affected by a similar reverse causality issue: all due differences can be accounted for through a well-tailored first stage regressions' specification. In this regard, it should be noted that both Chong and Calderón (2000) and Goés (2015), the latter in particular, provide a more limited overview of topic-wise relevant literature, which is mirrored in the absence of concerns regarding social capital and in a less detailed attention to the definition of institutional quality when choosing the data. Indeed, their literature reviews are mostly focused on related studies from the econometric point of view. Section 5, along with results, will present the evidence relative to omitted variable bias in Goés (2015).

2.2 Institutions and culture: definitions and role

Precisely because the stated objective of the first stage is to demean the variables of all possible sources of endogeneity, it is of foremost importance to identify the latter, and to define both regressors and dependent variables as accurately as possible so to ensure a meaningful specification of first stage models and a tailored data collection. Such definition constitutes the objective of this section, as based on literature from different fields up to the current date.

As paradoxical as it may sound for research fields that stimulate vivid academic debates such as the one mentioned above between Acemoglu and co-authors, and Sachs, the clear definition of institutions (and culture) is far from precise or universally shared in political and development economics. In the present brief literature review, "Institutions" can refer to all juridical and political arrangements, from the Form of State or Government to the electoral law, more general legislations, their enforcement degree, the easiness of enforcing a private contract or setting up a business. A further distinction is often made between economic, judicial and political institutions. Nonetheless, the need of empirical studies on this topic to identify the institutional drivers of economic growth has led to some consistency, from the nineties, in utilizing proxies that focus on the protection of property rights: this is also the case of Barro (1991), Keefer and Knack (1997), Acemoglu, Johnson and Robinson (2001), as well as Chong and Calderón (2000) and Goés (2015), who refer to different indicators and data sources due also to notable situations of data scarcity.

On more theoretical terms, North (1991) had already introduced the ampler definition of "rules of the game", that is a view of institutions as incentives for agents in society, which supersedes empirical considerations such as the choice of specific indicators.

If some convention, although blurry, has been achieved for institutions, the definition of culture is extremely more confused. Another problematic consideration is that national and local cultures, obviously, can be expected to exhibit different degrees of influence over economic development and/or institutions depending, also, on how they are measured. The present literature review has identified three main sets of definitions, which are clearly related, yet different in their interpretative nuances:

I. Following Putnam (1993), culture as social capital is defined as "the features of social organizations, such as trust, norms and networks, that can improve the efficiency of society by facilitating coordinated action". This line has been followed by Guiso, Sapienza and Zingales (henceforth GSZ) (2007; 2016); it had been implicitly envisaged already by Banfield and Banfield (1958) in their illustrative – although qualitative – study of Southern Italy's culture and political participation; it was successfully empirically tested – concerning trust – in Nunn (2011).

II. Platteau (2000) has more clearly defined the observations made by Banfield and Banfield and has defined "general and limited morality", which indicate the pervasiveness of cooperation and trust respectively in the whole community or limited to the nuclear family – the latter case is defined "amoral familism". These were explicitly adopted by Tabellini (2008; 2010) in his theoretical and empirical study of the formation mechanism of culture, and their effects on economic growth in European regions. Both authors, in a sense, acknowledge and deepen Putnam's definition of social capital in the respective areas of application.

III. A third, completely different dimension over which culture is measured concerns instead the spectrum between individualism and collectivism, borrowed from the field of social psychology and defined as whether individuals are expected to take care of themselves or to rely on integrated, loyal networks, as in Gorodnichenko and Roland (2016), and partially in Licht et al. (2007).

Finally, confusion arises when culture is defined as an informal institution, as in David (2007): indeed, culture intended as a code of conduct for individuals in their interactions is an unwritten "rule of the game". This study, however, will treat culture as social capital and as a separate variable, in econometric terms, from institutional quality: I shall therefore supersede the conceptualization of culture as an informal institution.

2.3 Institutions, culture and economic development: a two-way relationship

I already mentioned that both Chong and Calderón (2000) and Goés (2015) recognize the importance of including feedback effects from economic growth to institutional quality when considering the relationship between the two. I have also hinted, however, that both omit the consideration of social capital – or culture, in broader terms – as a relevant third element in this cause and feedback relationship. In this section, I highlight how previous literature has suggested paying attention to social capital, and review unidirectional approaches to the relationship between all three variables. Firstly, it should be noted that nearly all publications under analysis take economic growth, or long-term development, as the ultimate phenomenon which they attempt to explain. To name some major contributions, this is done by Barro (1991); Keefer and Knack (1997); Acemoglu, Johnson and Robinson (2001); Acemoglu et al. (2002); Acemoglu and Johnson (2005); Tabellini (2010); Nunn (2011); GSZ (2016).

Of these, the first five are mostly concerned with the role of formal institutional quality as a driver of economic growth in large cross-sections of countries. In particular, Barro and Keefer and Knack picture the discovery of institutional quality as an important explanatory factor of growth, in a methodological framework which is still strongly anchored to neoclassical models of growth. Acemoglu and co-authors' works, instead, abandon such theoretical context and focus on proving causality of institutions on growth. On the other hand, the last three papers mentioned in this brief list (which is a synthetic representation of the much wider work done by the aforementioned authors) focus instead on the causal effect of social capital on growth, and do so in more restricted cross-sections which are observed over prolonged periods of time. Nunn (2011), for instance, finds that a negative shock to social capital in African countries, namely the increase in kidnappings associated with slavery commerce in the sixteenth century, has translated into a negative impact on growth up to the current date. Similarly, GSZ (2016) find in the Communal experience of Italian cities in the Middle Age a substantial driver of the current difference of economic outcomes for Northern and Southern regions in the country, through a different evolution of social capital. In a cross-section of European regions, Tabellini (2010) implements an instrumental variable strategy to explain different economic growth levels with differences in social capital (mostly trust, as measured by the World Value Survey).

Nonetheless, vague exceptions relative to the direction of hypothesized unilateral causality links exist: for instance, Inglehart and Baker (2000) test the influence of economic development in systematic changes of basic cultural values and find that, although econometric results confirm their hypothesis, culture is largely path dependent, regardless of economic development. It is worth recalling that, by applying standard microeconometric techniques such as instrumental variables or GMM, the aim of researchers is inevitably that of identifying one direction of causality between culture and institutions, or between one of the two and economic development. Indeed, empirical studies find evidence for both directions of causality. In general, those who maintain that culture causes institutional quality or arrangements, measure the former in line with Putnam's social capital's definition (Nunn, 2011; Licht et al., 2007; GSZ, 2007). Again Tabellini (2010) instead, while attributing to social capital primary importance, implicitly assumes it is in turn caused by institutions (used as instrumental variable so that institutions only affect economic development through culture). On the contrary, Acemoglu et al. (2002) assert the opposite, in particular that all cultural influences on development disappear once measuring the quality of institutions correctly. There are exceptions that consider feedback effects, besides the already mentioned Chong and Calderón (2000) and Goés (2015) which however, do not resort to Vector Autoregressive approaches. These are Gorodnichenko and Roland (2016) – empirically – and Williamson (2000).[1] The latter, in particular, constitutes a review-based attempt to highlight the understatement of culture in economic literature and to orderly consider the chain of causation among the three variables. Both studies conclude that culture influences institutions (and the opposite is a weaker feedback effect). Figure 2 reports Williamson's analysis of both the envisaged causation (solid arrows) and feedback chains (dashed arrows), as well as his conception of the elements' relative pace of evolution, in the second column, which suggests further empirical analysis of their path dependence. Each element is considered relative to the research field which has explored it in greater detail. Besides the need of more clarity in pointing arrows, however, what strongly emerges from analyzing this body of literature is that culture is relevant to the institutions-growth relationship, and therefore it should not be neglected.

[1] In an appraisable attempt to contextualize together the studies from different fields of research (outlined at the bottom of Figure 2) to the date of his writing, which hardly ever referenced each other.

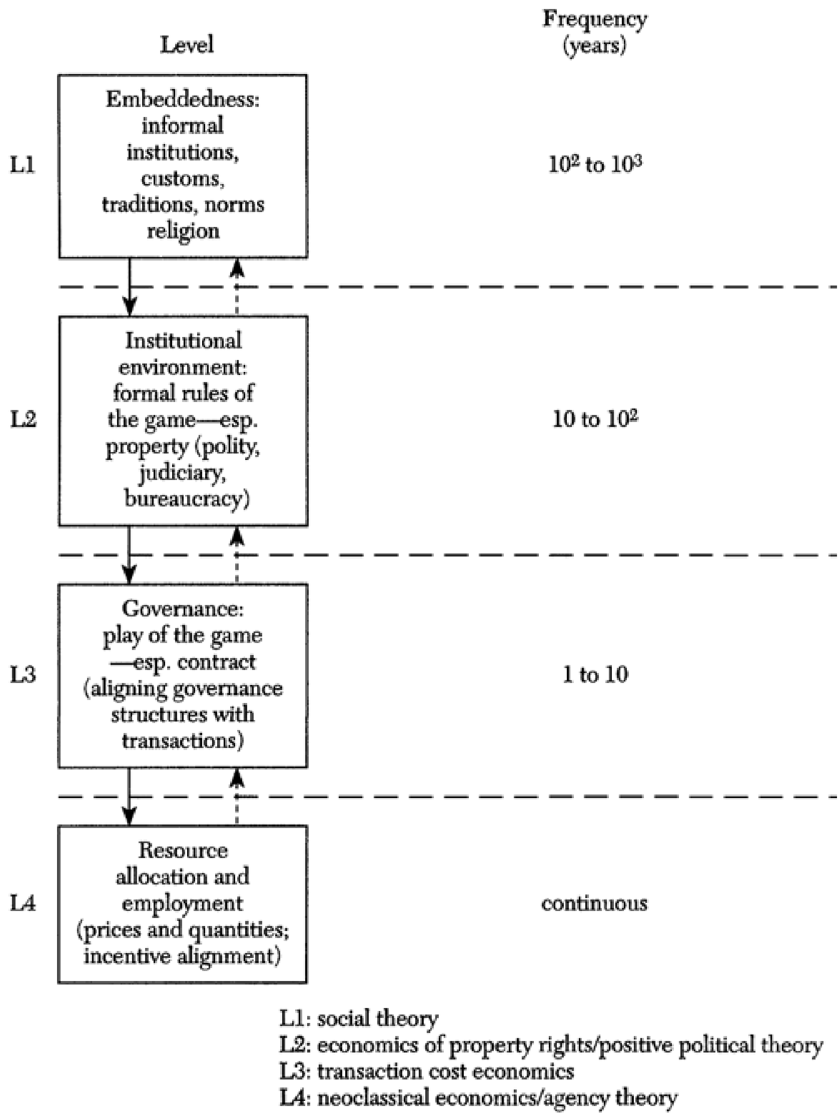


FIGURE 2: Causation and feedbacks. Source: Williamson (2000)

2.4 A brief logical elucubration: culture before institutions

Inasmuch as these considerations are to be made, the present suggestion is that, if a unidirectional arrow between culture and institutions (or culture and economic development) is to be pointed, as data availability for the present empirical strategy partially requires, it should follow Williamson's. The motivation is a purely logical elucubration, and becomes clearer when considering the undoubted fact that institutional arrangements are influenced by a number of determinants, of which culture is only one (Figure 3). Other determinants (or shocks) can be, for instance, constraints imposed by supranational organizations or by a dominating external country (e.g. the American occupation in Japan established most of economic institutions and the Constitution that are still in place; the much known distinction between Civil and Common Law jurisdiction countries can be traced back exactly to the nationality of nineteenth century colonizers): all exogenous institutional shocks of this nature could hardly have a more lasting impact on culture – and the American occupation of Japan once again stands out as an excellent illustration. Although counterexamples can be found in the pre-Christian world – e.g. by considering the effect of the Roman conquest of provinces outside of the Italian peninsula — these are so remote that it becomes arduous to even draw a line between the exogenously introduced culture and the pre-existent, local one. In any case, it is hardly questionable that exogenous influences can have an abrupt and immediate impact on institutions, whereas the same is not possible for culture, which can be influenced only by a pervasive, long and continued influx of external cultural values' transmitters – as that of American films, music and literature along the second half of the twentieth century in Europe.[2] These instances, however, are less relevant for this thesis, which analyzes a twenty-one years' time-span across the end of last century and the beginning of the current one.

This logical assumption, as made in this study, is crucial in order to include social capital as a regressor of the first stage, rather than as an element of the vector of variables in the PVAR model – a choice motivated, as hinted above, purely by difficulties in measuring social capital thoroughly with available data. Let us underline this logical line by contradiction: if institutions, whose nature can be driven by exogenous factors, were to cause culture, one might reasonably argue the latter should be affected (contemporaneously or with a short lag) by the same exogenous determinants, which is not observed also because, whereas formal institutional change is codified hence observable (e.g. Constitutional modifications), cultural change is not, and is determined by transmission mechanisms characterized by long time-spans – e.g. see Tabellini (2008).

[2]Which of course concerns also cases such as the Japanese one, but surely over a much longer time horizon with respect to the impact on institutions.

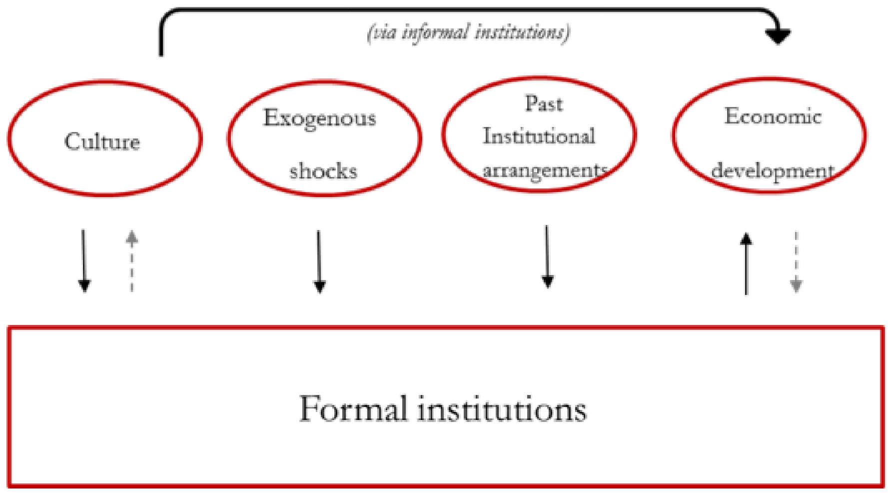


Figure 3: Visual summary of the conjecture: dashed arrows indicate feedback effects, solid ones direct impact, as in Fig. 2

2.5 Path dependence

As hinted previously, the concept of path dependence, relative to institutional quality (or arrangements) and to social capital, is often invoked without providing a clear definition or an empirical test for its presence, as it is the case in Acemoglu, Johnson and Robinson (2001).

Indeed, the father of path dependence's concept, Paul David, is unsurprisingly the only scholar who provided its complete formal definition even in this specific context. In very general terms, he presents two examples of it, respectively in technologies and informal institutions: just as the qwerty keyboard is possibly more inefficient than other keys' dispositions, yet the most widespread due to lock-in effects, the current habit of shaking hands when greeting someone is an unnecessary yet pervasive legacy of a past in which it had the function of showing one was unarmed (David, 2007). In more specific words instead, David defines path dependence as "non-ergodic", where ergodic means, simplifying, that a process can assume all its possible realizations (assuming these are finite) with equal probability, thus by the Central Limit Theorem it will visit all of them, in a sufficiently long time-span. Indeed, in common English, the term ergodic refers to an event which will reoccur with zero probability. Non-ergodic, therefore, means that the probability of a given state's future realization is not uniformly distributed across the n possible states. In fact, a path dependent process' stochastic system is asymptotically distributed so that each realization is a function of previous ones (i.e. it is a Markov process), which implies that a transient shock to the process may exert a long-lasting influence over future realizations of a variable. An

autoregressive framework combined with IRA therefore appears ideal for the study of path dependence: the duration of exogenous shocks' ramifications depends on the nature of the estimated autoregressive coefficients. Possibly, considering an autoregressive system, such as a Vector Autoregressive model, rather than a single autoregressive process, represents an even improved setting to test this definition empirically, especially in the context of growth and institutions, for the reasons presented in Section 2.3. This, clearly, provided that such system is well specified and stable (as defined in Section 4.3). Path dependence can then be analyzed by studying the Impulse Response function associated to that system[3]: by sending an hypothetical exogenous unit shock into the system through one of the single equations' residuals, I am able to test for how long it will exert a significant impact on each core variable, and with which magnitude.
One possible drawback derives from the relatively short time-span considered in the analysis (21 years), which might be reasonably expected to affect most similar empirical studies due to data availability issues. The main disadvantage, however, is caused by not including social capital explicitly in the vector of variables which enters the PVAR, which would instead allow to observe the persistence of a shock to social capital or, more meaningfully, the effects on social capital of a shock in another core variable: although this is not possible given the data at hand, the coefficients on which the IRA is based are estimated on demeaned variables which are therefore "cleaned" of the influence of social capital.

I can nevertheless elucubrate qualitatively on social capital's path dependence. Let us turn back to the reasoning in Figure 3, and let us safely assume, based on the general (although imprecisely defined) consensus, that both institutions and culture are autoregressive processes. Then the simple idea that institutions are generally subject to more exogenous shocks which indeed are able to influence their evolution compared to culture should be enough to state that culture is led to modify its future realizations' probability distribution less often and, hence, in a sufficiently long time period, to remain unchanged for a longer time. In other words, culture is "relatively more path dependent" than institutions. Note that this does not imply that the persistence of social capital is assumed. As it will become clearer when reading Sections 3 and 4, I tried to separate as much as possible the time-varying component of social capital (represented by political participation) from its more stable counterpart (e.g. habits, morality, which should be captured by fixed effects over a 21 years span). Feasibility, therefore, imposes only one constraint: the time variation of social capital has been explicitly included in the first stage, despite its exclusion from the second stage does not allow to measure the duration of its dependence.

[3]The latter is derived in Section 4.3, and is illustrated in Section 5.2.

3 The data

3.1 Sources and collection

The longitudinal dataset in use comprises 164 countries, the complete list of which is available in Appendix C – and 21 years – from 1990 to 2010, for a total of 3444 observations. Let us begin by describing the data in use relative to the three main variables of interest, so far denominated as economic development, institutional quality and social capital. In this study, the first is represented by the variable GDP per capita, in current US dollars, from the World Bank open database, where the denomination in per capita and current dollar terms is chosen to guarantee a higher degree of comparability across countries.

Choosing an indicator of institutional quality has instead proven more challenging: among all possible alternatives, the one which ensured the widest data coverage in terms of the intended longitudinal dataset is the "Institutional quality dataset" proposed in Kuncic (2014). The latter, which is theoretically rooted in the literature discussed above and in particular in North's definition and Williamson's classification of institutional variables, constructs three indexes of institutional quality – respectively legal, economic and political – which summarize other pre-existent indicators, as those developed by the World Bank, the Polity project, the International Country Risk guide and Transparency International, which were also considered as potential data sources for this exercise. Besides the clear advantage of covering virtually different aspects of institutional quality within a single indicator, Kuncic's database also stands out for its uniquely comprehensive longitudinal coverage: it encompasses, in fact, 197 countries and 21 years (1990 to 2010), which constitute the starting point for the creation of the dataset in use. Moreover, because the present empirical strategy computes pooled estimators instead of fractioning the analysis panel by panel, among all the indicators in Kuncic's dataset, I focused on those expressed in absolute terms – yet all standardized within the range [0,1] –, rather than on rankings. In particular, I picked the absolute indicator of economic institutions' quality over the other two (legal and political). While the reader may possibly find this choice restrictive, it is worth mentioning that the economic institutions' indicator is the result of the aggregation of indexes of: regulations concerning credit and labor markets, and business-related regulations; overall regulatory quality; financial and business freedom; freedom of the press. It should not surprise the reader, therefore, that the chosen index is highly correlated with the other two. The overall correlation with political and legal institutional quality, within the selected sample, is respectively of 0.7421 and 0.7389, with much higher values displayed for specific countries. The choice of the aforementioned indicator is also motivated by the lower occurrence of missing observations, which is of crucial importance when working with numerous panels, especially in light of the two-staged empirical strategy proposed in this study.

Social capital, finally, represents the most problematic of all three variables when it comes to data collection. Indeed, a comprehensive analysis of the literature reveals the virtual absence of similarly vast longitudinal studies, for the majority focus on narrower realities: referring to previously cited studies, Tabellini (2010) treats the regions of Europe; GSZ, (2016), limit their brilliant analysis to Italian municipalities. They both refer their data selection to the above-mentioned works of Putnam (1993) and Banfield and Banfield (1958), yet their narrower focus allows to combine indicators of different aspects of social capital which turned out to be hardly replicable in bulk for the present study. For instance, Tabellini utilizes questions on trust from the World Value Survey, whereas GSZ (2016) combine turnout rates – proxies of political participation – with the number of organ donation and non-profit organizations in each municipality. Both ways were explored: the World Value Survey, because of the very little variability allowed in the answers to the only viable question on trust (due to cross-country coverage feasibility) and the excessive time and frequency differences in how survey waves were conducted in different countries, did not constitute a good indicator. Statistical techniques such as interpolation cannot, by construction, add information and improve the indicator's variability, which was deemed insufficient and too approximative to bring a valuable contribution to the empirical strategy. The number of non-profit and organ donor organizations was not a viable option for a dataset as vast as the one in use: even the hypothetical collection of the indicator only for capital cities would not have brought similar added value as in GSZ (2016): the much wider cross-country difference in economic development and access to financial resources would have rendered the indicator meaningless. Moreover, altruism, on a 21 years span (instead of approximately 1000 years in GSZ), is extremely likely to be captured by country fixed effects, which are accounted for in the first stage of the empirical strategy.

On the other hand, turnout rates, once controlling for elements such as compulsory voting or absence of elections, still appeared to constitute a possible indicator of political participation and, hence, of the time-varying aspect of social capital.
These were obtained from the Voter Turnout Database from IDEA, the International Institute for Democracy and Electoral Assistance, and regard both parliamentary and presidential elections, as they represent the most important electoral moments under, respectively, a parliamentary and presidential form of government. When both were present in a country, both were kept: whereas the parliamentary form of government usually only provides for parliamentary elections, both can be present under the presidential form of government. Presidential elections, therefore, for their undoubted political importance in those countries

where they exist, contribute to the variability of the data. IDEA also provides for a comprehensive list of countries were voting is compulsory: if applicable in the period 1990–2010 and also enforced, I generate a dummy variable named *Compulsory*[4] taking value 1, to take this into account in our analysis. Finally, China and Saudi Arabia did not have any election in the considered time-span: their turnout rate is set to 0.

For what concerns instead secondary control variables, these have been all downloaded from the World Bank database, for the countries of interest, to be tested as regressors of respectively per capita GDP and institutional quality in the first stage of analysis. As such, and as it will become clearer to the reader when learning about the role played by the first stage within the empirical strategy, these covariates are expected to change across both time and space dimensions, and are chosen for inclusion in the first stage based on the Akaike criterion. In particular, those in use are: the rate of urban population, calculated over the total population; net exports, computed autonomously as the difference between total exports and total imports of goods and services, in current US dollars; net oda (Official Development Aid) received per capita, in current US dollars; the gini coefficient. A detailed description of which variables were chosen for each equation in the first stage is presented in Table 7, Appendix A.

In conclusion, and only for the purpose of providing the reader with graphical representations of descriptive statistics, a cluster variable is generated based on per capita GDP. That is, countries are assigned a cluster based on their average per capita GDP in the selected time-span, relative to the overall distribution of the per capita GDP variable, i.e. each cluster represents a quartile: since the distribution, however, is heavily right-skewed, the last quartile is broken into two clusters, as the following table illustrates.

Table 1: The *cluster* variable with respect to per capita GDP's percentiles

Percentiles	Assigned cluster	Range (US dollars)	Number of countries
[1, 25)	1	[163, 869)	25
[25, 50)	2	[869, 2704)	33
[50, 75)	3	[2704, 9425)	12
[75,89)	4	[9425, 24936)	21
[89, 99]	5	[24936, 87655]	17

[4] Detailed information on which countries enforce compulsory voting can be found by IDEA's website, at this link.

3.2 Data manipulation

In order to obtain a strongly balanced panel, and because of the primary role played by the residuals from the first stage for the success of the analysis, it is of foremost importance to apply statistical procedures aimed at minimizing the number of missing observations. The procedures implemented varied across variables, in order to mirror the most accurately the actual variation of each one.
Firstly, let us consider the turnout rate: besides controlling for compulsory voting and the absence of elections as mentioned above, cubic splin interpolation was applied (clearly, within individual panels). To improve the quality of interpolation results, I initially considered out-of-sample observations, namely from 1988 to 2012. The choice of a cubic functional form stands from the need to take into account that each election is an expression of the electorate's satisfaction with their political class, and that abstention is often interpreted as a form of protest: therefore, each voting occasion is to be considered as a standalone event whose turnout rate is not a gradual evolution from previous voting participation rates. This interpretation is naturally the result of personal opinions which could possibly not be in line with those of the reader. Because, for every country, the total number of observations to obtain is 21 (i.e. 20 splines) with an often much lower number of available observations, spline interpolation was preferable to polynomial, for the objective is to maintain the highest possible degree of accuracy (Hoffman and Frankel, 2001).

There is a specific, estimation-based need to perform interpolation on turnout, given the data at hand, which is well explained in Pesaran (2015a). Indeed, it is important to recall that in the present analysis, turnout is aimed at capturing the portion of social capital, i.e. political participation, which can vary within countries in a 21 years time-span; on the other hand, aspects such as trust and cooperation within communities which are more closely ascribable to culture, can be reasonably expected to remain (roughly) constant over such a short period. In econometric terms, the latter are expected to be captured by countries fixed effects in the first stage of analysis, which the following sections refer to as α_i. Following Pesaran, if the unbalance (i.e. the location of missing observations in time and space) is uncorrelated with the residuals – although allowed to correlate with both country and year fixed effects – then the two-way fixed effect estimators remain consistent and asymptotically normally distributed. However, this is not the case if missing observations are dropped, for then consistency and asymptotic normality would require independence between the country-specific unobserved characteristics and the location of missing observations (for instance, that would include the assumption of independence between the characteristics of a given country, such as its form of government, and the frequency of its elections, which would clearly be completely unsound).

Linear interpolation, instead, has been considered for all control variables exhibiting missing observations, namely *gini coefficient* and *oda*, to mirror a more acceptable gradual change in values. When out-of-sample observations were not helpful in supporting the process or unavailable, the 1990 and 2010 observations were replaced with the closest available one, instead of the country average, in order to respect as much as possible any possible trend in the data. Such substitution was then followed by the application of univariate linear interpolation.

Finally, in the extremely rare case in which some data points were absent for the institutional quality variable, these have been replaced by mean imputation, for in this case variation over time has never exhibited particularly extreme peaks or troughs, and because in econometric terms, substituting for the mean does not imply major impacts on the estimators compared to the initial situation with missing observations.
In those cases in which data for turnout or institutional quality were completely absent for a given country, that country has been dropped: substituting for the overall sample mean might have led to important biases in the results, given these variables constitute the core of the analysis. When this situation regarded the variable *oda*, the latter was assumed to be 0.

3.3 A brief overview of the data

Before turning to the empirical strategy, the reader may find it useful to recap the main characteristics of the data to which the models are applied. The following two maps (Figures 4 and 5) present respectively a graphical summary of each country's turnout rates and institutional quality, respectively – calculated as the average over the considered time period. At the same time, they illustrate graphically which countries were omitted from the sample due to data scarcity (the reader might also recall that the full list of countries in the sample can be found in Appendix C).

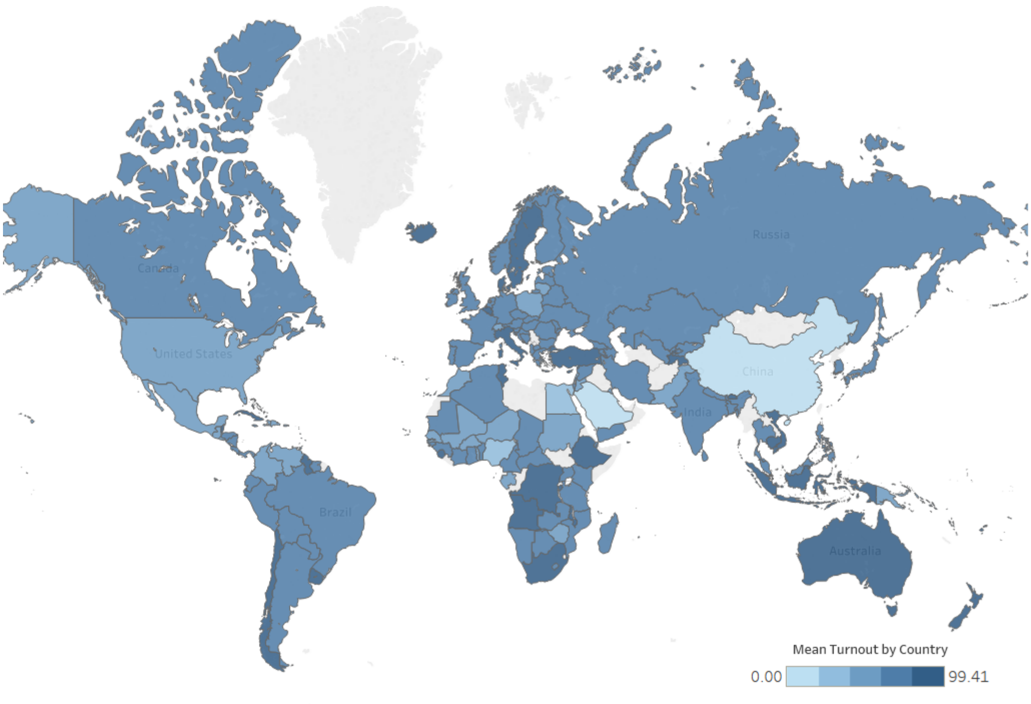


FIGURE 4: Mean turnout by country in the sample

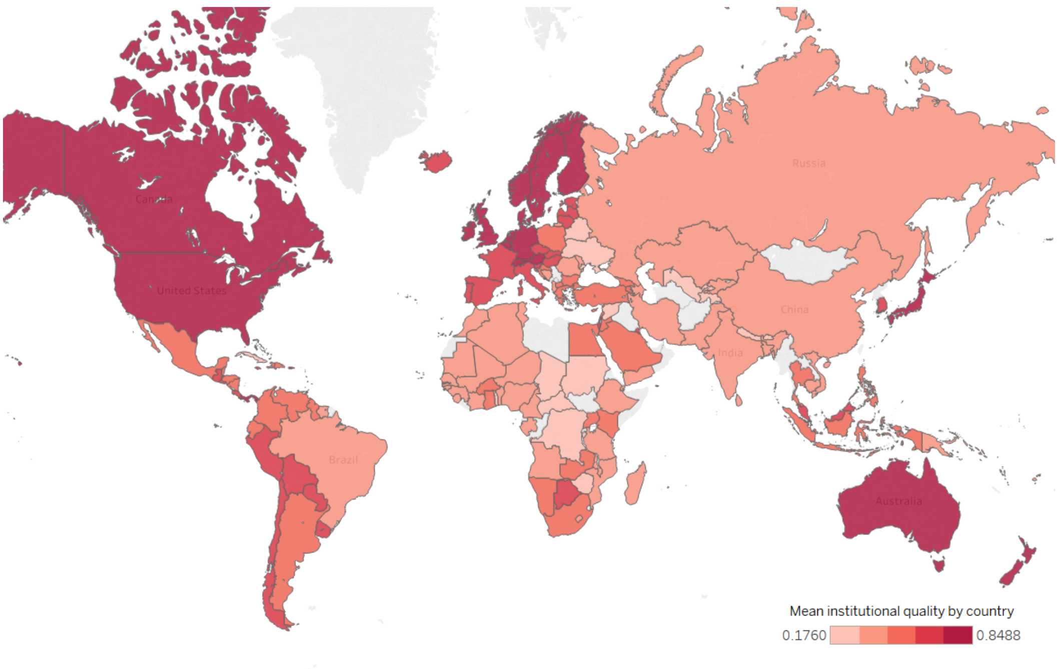


FIGURE 5: Mean Institutional Quality by country in the sample

Because of the highly skewed and volatile distribution of the per capita GDP variable, a graphical representation of this type is not particular helpful to understand it in relation to other core variables. Nevertheless, these tables can be complemented with the correlation matrix in Appendix A (Table 9). In terms of spatial variation, indeed, a quite sensible correlation between institutional quality and per capita GDP can be observed, as it can be expected following the analysis of previous literature in this topical area. On the other hand, political participation in the considered years seems to negatively correlate with per capita GDP over time: as the results from the first stage will underline in Section 5.1, this is robust to controlling for compulsory voting and the absence of elections. Indeed, a visual overview of time variation in the three core variables – where the time lines displayed are average year values by GDP clusters – renders the phenomenon more evident: in the aftermath of global economic downturn, political participation recovers from an otherwise declining trend.

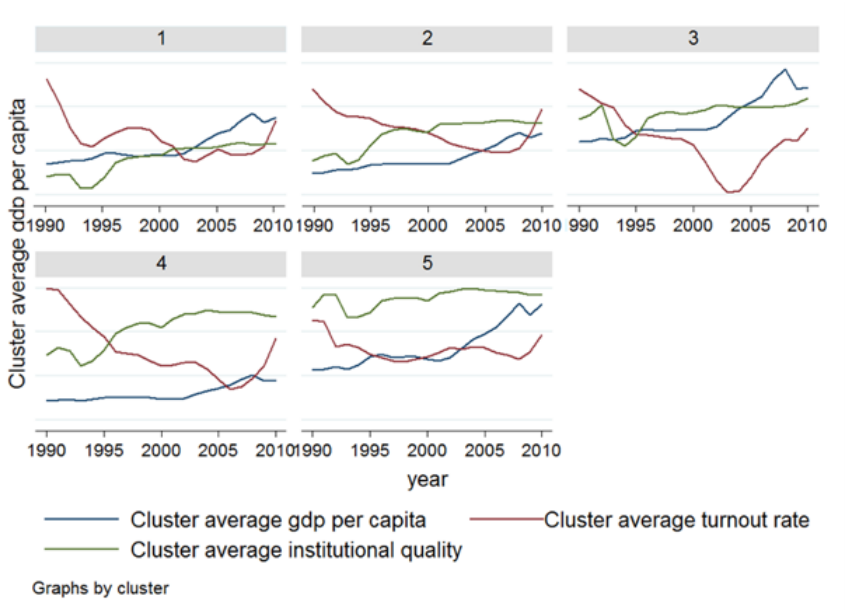


FIGURE 6: Turnout rates, institutional quality index and per capita GDP graphed by GDP cluster

4 Empirical strategy

As hinted already in previous sections, the empirical strategy of this study is divided into two stages, where the first is preliminary to the second. The latter, instead, constitutes the bulk of the analysis, for it consists of the PVAR model estimation over demeaned per capita GDP and institutional quality. In this section, the models used in each stage will be presented and motivated. The results will be instead presented in the next section, separately for each stage.

4.1 First stage

Speaking in extremely simplified terms, all VAR models are characterized by a similar trade-off, between the inclusion of potentially significant covariates in the system and the necessity to remain conservative to avoid excessive computational burden: in fact, each additional covariate adds an equation line to the system, plus as many estimators as the lags order of the model times the number of other equations in the system plus one, (i.e. the variance of the single-equation error). For instance, adding a third variable on top of the original two to a PVAR which lag-order is 2 This implies that the number of estimated parameters passes from 10 to 21 (in the reduced-form PVAR). This implies that, although from a methodological viewpoint the use of a VAR can be indicated for analyses which comprise possible reverse causality or feedback effects, using it alone would still bring to biased estimators following the omission of significant regressors from the system, especially in a topical context as the present one, which would require the inclusion of a potentially large number of control variables. For this reason, this study proposes the inclusion of a preliminary first stage aimed at demeaning the core variables – this is the name attributed jointly to per capita GDP and institutional quality for the rest of the discussion – from all relevant and measurable exogenous explanatory factors, before utilizing them in the PVAR, allowing to keep the number of endogenous variables in the latter to 2. That is, for a general core variable Y_{it} in the longitudinal dataset (where i denotes the country and t the year), the output of the first stage is the demeaned variable $\widehat{Y_{it}}$, which has the form:

$$\widehat{Y}_{it} = Y_{it} - \hat{u}_{it} = Y_{it} - \beta' X_{it} - \delta' \Omega_{it} - \alpha_i - \gamma_t \tag{1}$$

Where X_{it} is a vector of fundamental explanatory variables; β the vector of their respective coefficients[5]; Ω_{it} is a vector of time and space varying controls, $\widehat{u}_{it}$ is the residual from the demeaning model, as illustrated later by Equations (2b) and (3b); α_i are unobserved, time-invariant, country-specific factors and γ_t are unobserved, space-invariant, time-specific factors (i.e. country and year fixed effects).

In this case, the two core variables subject to demeaning are per capita GDP and institutional quality (indicated by economic institutional quality, as specified in Section 3.1). The reader might wonder, in light of the discussion above regarding social capital, why the latter is not included in the vector of core variables and, hence, in the PVAR model. Although advisable indeed, the issue resides in the criticalities of measuring social capital in a comprehensive way, for such a large selection of panels. As already mentioned in Section 2.2, in fact, social capital as defined by Banfield and Banfield (1958), Putnam (1993)

[5]In this study, turnout rates. Note that fundamental variables are the main regressors of first stage models. They are different from core variables, which are instead the variables included explicitly in the second stage (the PVAR), and appear in the first stage models as dependent variables, to be demeaned.

and consequently GSZ (2016) can be interpreted as a combination of political participation in a given geographical area, and other local cultural factors such as trust in fellow citizens and communitarian levels of cooperation or altruism. Whereas I approximate the first with turnout rates at parliamentary and presidential elections, the nature of survey data concerning trust and cooperation is hardly suitable for a longitudinal study as the present one. Because, however, the time-span in our focus is relatively narrow relative to the evolutionary dynamics of cultural factors – as underlined also by Williamson (2000) – I am confident they will be captured in the first stage by unobservable, time-invariant factors, or α_i, following the notation above. Nevertheless, such unobservable factors are not discernible from time-varying ones (γ_t), and cannot be measured *per se*: this, together with the fact that turnout rates alone are a rather partial and individually insufficient indicator of social capital, renders the inclusion of the latter in the vector of core variables impossible. In other words, whereas turnout and country fixed effects ensure that social capital's influence is subtracted from our indicators of economic development and institutional quality, and therefore it will not bias the second stage results, it is not possible in this study to explicitly include social capital as a core variable. A narrower study focused on the same research question, however, such as for instance a within-country analysis, could and should definitely include social capital as a core variable, for survey data (e.g. from the World Value Survey) could proxy trust and cooperation in a more meaningful way in that context.

Turning to the essence of the first stage, the following two-way fixed effect models are estimated (where Y_{it} indicates per capita GDP and I_{it} the institutional quality index):

$$Y_{it} = \beta_1' Turnout_{it} + \delta_1' \Omega_{1,it} + \alpha_{1,i} + \gamma_{1,t} + u_{1,it} \tag{2a}$$

$$I_{it} = \beta_2' Turnout_{it} + \delta_2' \Omega_{2,it} + \alpha_{2,i} + \gamma_{2,t} + u_{2,it} \tag{3a}$$

Respectively, the following demeaned variables are obtained:

$$\widehat{Y_{it}} = Y_{it} - \hat{u_{1,it}} = Y_{it} - \beta_1' Turnout_{it} - \delta_1' \Omega_{1,it} - \alpha_{1,i} - \gamma_{1,t} \tag{2b}$$

$$\widehat{I_{it}} = I_{it} - \hat{u_{2,it}} = I_{it} - \beta_2' Turnout_{it} - \delta_2' \Omega_{2,it} - \alpha_{2,i} - \gamma_{2,t} \tag{3b}$$

In both cases, the fundamental regressor, the one denoted as X_{it} in Equation (1), is the turnout rate. The composition of vector of controls $\Omega_{1,it}$ and $\Omega_{2,it}$, instead, was drawn from a number of potential covariates whose list can be found in Appendix A (Table 7), based on the Akaike criterion for model selection.[6] That is, controls are chosen to ensure the core variables are subtracted as much exogenous-led variation as possible. Appendix A also includes the demeaned variables' summary statistics, as well as their correlation between each other and with non-demeaned core variables (respectively Tables 10 and 9). The errors are clustered-robust at the country level, that is they are allowed to be serially correlated within countries, but independent across countries. The latter hypothesis is tested by means of the CD cross-sectional dependence test presented in Pesaran (2015b), which is chosen for its specific focus on large N settings and for its independence from choosing a spatial matrix, which the author himself points out as an advantage in cases were proximity may be of political nature rather than geographical. The null of cross-section independence of residuals is not rejected for both Equations (2a) and (3a), for their residuals present a test statistics of 1.825 and 1.797 respectively (less than the critical value 1.96, at the 95% confidence level). Under cross-sectional independence, which is further evidenced by the distribution of residuals as shown in Section 5.1, robust clustering represents a conservative approach. In fact, following Cameron and Miller (2015), it should avoid unreasonably low coefficients' standard errors and hence overstatement of coefficients' significance. Moreover, allowing serial correlation takes into account the possibility of non-measurable omitted covariates. Because a correct specification of the first stage is a crucial step for the validity of the whole procedure, avoiding such overstatement is of foremost importance.

The reader might also wonder what drives the choice of using two-way fixed, rather than random effects. Whereas both assume that all right-hand side regressors, conditional on the intercept α_i, are strictly exogenous, there are two distinctions between the two which render fixed effect more suitable in light of the economic reasoning relative to this analysis. Clearly, however, the case-specific compliance with the following two assumptions could vary and prefer random effects in different applications of this two-stages procedure. Rephrasing Pesaran (2015a):

I. whereas under fixed effects the individual unobservable characteristics α_i are finite for all sections (i.e. $\forall i = 1, 2, \ldots, 164$) but are allowed to correlate with regressors, under random effects each α_i is independent of regressors. Because, as argued above, social capital cannot be expected to be fully accounted for just by including turnout in the first stage specification, it is highly likely – and confirmed by our estimation – that the variation in social capital not captured by turnout, which I expect to be instead

[6] I preferred the Akaike over the Schwarz criterion for the latter penalizes models with a higher number of regressors. Because the aim is to detract as much relevant variation as possible, it was preferred not to penalize less parsimonious models. For a more detailed tractation, see Pesaran (2015a), Section 11.5.4.

represented by α_i, will be such that Corr(α_i, X_{it})$\neq$0, which makes fixed effects more suitable for this study.

II. Random and fixed effects are different in their treatment of cross-sectional variation in the intercept, as measured over a continuous spectrum by a parameter ψ, where $\psi \in [0,1]$. That is, under fixed effect there is maximal heterogeneity of intercepts across countries (in this case) so that ψ=0, whereas under random effects $\psi \geq 0$ and in particular, for $\psi \rightarrow$1, the random effects estimator converges to the OLS one (i.e. with a fully pooled intercept).[7] I can reasonably expect the unobservable individual effects – and the cross-sectional cultural variation it embeds – to affect growth and institutional quality to different degrees in different countries, which indicates fixed effect again as more suitable.[8]

4.2 Second stage: specification of the PVAR

There is a difference between the ideally envisaged specification of the second stage's PVAR and what is feasible under the computational constraints imposed by statistical softwares to date. In particular, the limited range of options allowed by the Stata packages *pvar* and *pvar2*, together with the unavailability of alternative packages in other softwares for the estimation of PVARs under a frequentist approach (to be distinguished from Bayesian) do not allow for a univocal, ideal specification of a PVAR, which is therefore subject to compromises. This section will outline the ideal specification, the estimated one under different Stata packages, and how the two differ in detail. Clearly, this will result in somewhat lengthy explanations: although they are necessary for the sake of correctness, the reader might also trust that everything that was possible was done to minimize all differences, and proceed with the ideal specification in mind. The ideal model is what Canova and Ciccarelli (2013) refer to as a PVAR without dynamic interdependencies[9], which reduced-form specification is:

$$\begin{bmatrix} \widehat{Y}_{it} \\ \widehat{I}_{it} \end{bmatrix} = \begin{bmatrix} \alpha_{1,11} & \alpha_{1,12} \\ \alpha_{1,21} & \alpha_{1,22} \end{bmatrix} \begin{bmatrix} \widehat{Y}_{i,t-1} \\ \widehat{I}_{i,t-1} \end{bmatrix} + \begin{bmatrix} \alpha_{2,11} & \alpha_{2,12} \\ \alpha_{2,21} & \alpha_{2,22} \end{bmatrix} \begin{bmatrix} \widehat{Y}_{i,t-2} \\ \widehat{I}_{i,t-2} \end{bmatrix} + \begin{bmatrix} \gamma_{11} & \gamma_{12} \\ \gamma_{21} & \gamma_{22} \end{bmatrix} \begin{bmatrix} t \\ t^2 \end{bmatrix} + \begin{bmatrix} e_{1,i\,t} \\ e_{2,i\,t} \end{bmatrix} \quad (4)$$

[7] For a full explanation, refer to Pesaran (2015a), Section 26.6.2.

[8] The Hausman test could in theory be used to measure the data-contingent value of ψ. Unfortunately, however, it cannot be performed when standard errors are clustered, as it is in our case: I stick to economic reasoning as a sufficient motivation to prefer fixed over random effects.

[9] Where for dynamic interdepencies they mean the inclusion of regressors, in single line equations, relative to a different country. Namely, whereas the dependent variable of each equation is denoted by the subscript it, some regressors would display the subscript jt where $i\neq j$.

Or, more compactly,

$$\widehat{C}_{it} = A_1\widehat{C}_{i,t-1} + A_2\widehat{C}_{i,t-2} + \Gamma T + e_{it} \tag{5}$$

Where the use of letter C denotes the vector of core variables, so that:

$$\widehat{C}_{it}' = \left[\widehat{Y}_{it}, \widehat{I}_{it}\right]; T' = \left[t, t^2\right] \tag{5_{annex}}$$

Note that the inclusion of lags up to the second order is based on the MMSC-Akaike information criterion (Henceforth MMSC-A). The time variable t and its square are introduced as exogenous regressors in each equation line of the model to take into account the presence of a quadratic time trend within panels: indeed, as suggested by Wooldridge (2011), this method can be preferable to detrending in panel data settings: the ΓT term matter is further analyzed in the discussion of second stage results in Section 5.2.[10] Note that some literature, including Canova and Ciccarelli (2013), would refer to this specification as a PVARX model, where the X denotes the inclusion of exogenous regressors in single equations. Note also that the elements of matrices A_1 and A_2 are pooled estimators, hence scalars. The standard errors are again robust (to heteroskedasticity) and clustered at the country level, which implies that conditional on the specification of the model, it is assumed there is no additional spatial correlation in the residuals (Cameron and Miller, 2015). As intended after the first stage, no intercept is specified in the PVAR. Notice also that in actual estimations of the PVAR, for a clearer interpretation of the estimated coefficients and of Impulse Response functions, the demeaned core variables have been standardized so that their distribution is centered around zero. Such transformation is denoted by a tilde, so that:

$$\tilde{Y}_{it} = \frac{\widehat{Y}_{it} - \bar{Y}_i}{\sigma_{Y,i}} \tag{6}$$

$$\tilde{I}_{it} = \frac{\widehat{I}_{it} - \bar{I}_i}{\sigma_{I,i}} \tag{7}$$

where $\bar{Y}_i$ and $\bar{I}_i$ are country averages. The trade-off that the use of the *pvar* and *pvar2* packages imposes is one between the inclusion of the exogenous ΓT term and an additional transformation of the demeaned core variables. Moreover, both are envisaged to estimate parameters by GMM, which requires some stratagem in order to correctly induce an OLS estimation.

[10] Detrending before the second stage was not a viable option in this case because of the different nature of time trends across panels, so that coefficients would still present sign errors which are instead avoided by explicitly controlling for time trends.

4.2.1 The *pvar* package estimation

Under the *pvar* package by I.Love and M.Abrigo, the ΓT term is included, but the demeaned core variables, because of the imposed GMM-style estimation, are subject to an additional transformation. The latter can be either a first difference (henceforth FD) transformation, in case the Arellano-Bond GMM estimator is chosen (Arellano and Bond, 1991), or the Forward Orthogonal Deviation (henceforth FOD) transformation if the Blundell-Bond GMM estimator is chosen (Blundell and Bond, 1998). They are also referred to as, respectively, *difference* and *system* GMM. The limited sample size in the temporal dimension ($T = 21$), and the notable change in interpretation brought by the use of FD (i.e. to look at the intertemporal change of levels, the flow, rather than at levels themselves) make the FOD transformation more appealing: in fact, the main objective of estimating the system as in system (4) is that parameters are estimated by OLS and variables are expressed in levels. Consider then FOD-transformed system, so that:

$$\tilde{C^*}_{it} = A_1\tilde{C^*}_{i,t-1} + A_2\tilde{C^*}_{i,t-2} + \Gamma T + e_{it} \tag{8}$$

where the FOD transformation, indicated with an asterisk, is:

$$\tilde{Y^*}_{it} = \left[(\tilde{Y}_{it} - \bar{\bar{\tilde{Y}}}_{it})\sqrt{\frac{T_{it}}{T_{it}+1}}\right] \tag{9}$$

$$\tilde{I^*}_{it} = \left[(\tilde{I}_{it} - \bar{\bar{\tilde{I}}}_{it})\sqrt{\frac{T_{it}}{T_{it}+1}}\right] \tag{10}$$

Another important characteristics of the *pvar* package is that, in order to estimate by GMM, it uses the lags of the core variables, untransformed and hence expressed in levels, as instruments of their FOD-transformed lags (i.e. the regressors). Suppose variables in the PVAR were untransformed, that is if levels were used to instrument levels: in that case GMM could be easily transformed into OLS by exactly identifying the system (i.e. by choosing 8 instruments for the 8 regressors in the whole system, and by instrumenting the latter exactly with themselves. In fact, the implicit first stage of the GMM estimation would not alter the regressor, for it would consist of, for a general standardized core variable $\tilde{C}_{it}$:

$$\tilde{C}_{it} = \tilde{C}_{it} + \mathcal{E}_{it} \tag{11}$$

$$\mathcal{E}_{it} = \tilde{C}_{it} - \tilde{C}_{it} = 0 \tag{12}$$

$$\hat{\tilde{C}}_{it} = \tilde{C}_{it} - \mathcal{E}_{it} = \tilde{C}_{it} \tag{13}$$

If instead, as it is the case in reality, the FOD-transformed variable is instrumented with the untrasformed version of itself, the error term from the implicit first stage of GMM would not be zero (in expectation), and its size would measure the compromise I am obliged to accept. Namely:

$$\tilde{C^*}_{it} = \tilde{C}_{it} + \mathcal{E}_{it} \tag{14}$$

$$\mathcal{E}_{it} = \tilde{C^*}_{it} - \tilde{C}_{it} \tag{15}$$

$$\hat{\tilde{C}}_{it} = \tilde{C^*}_{it} - \mathcal{E}_{it} \simeq \tilde{C^*}_{it} \tag{16}$$

Where $\hat{\tilde{C}}_{it}$ in Equation (16) is the core variable resulting from the implicit first stage of GMM: what the equation signifies is that, with the adopted stratagem, the latter is approximately equal to the initial standardized variable. Writing out Equation 15 is informative for the understanding of the magnitude of such approximation, as it shows that actually the compromise being made is not excessive:

$$\begin{aligned}
\mathcal{E}_{it} &= (\tilde{C}_{it} - \bar{\tilde{C}}_{i_t})\sqrt{\frac{T_{it}}{T_{it}+1}} - \tilde{C}_{it} \\
&= \tilde{C}_{it}\left[\sqrt{\frac{T_{it}}{T_{it}+1}} - 1\right] - \bar{\tilde{C}}_{i_t}\left[\sqrt{\frac{T_{it}}{T_{it}+1}}\right] \\
&\simeq q - p
\end{aligned} \tag{17}$$

Consider the terms q and p, as defined above. Firstly, the untransformed standardized variable $\tilde{C}_{it}$ is multiplied by a quantity, namely $\left[\sqrt{\frac{T_{it}}{T_{it}+1}} - 1\right]$, which is by construction only slightly negative. Therefore, the value is only slightly negatively biased, but the bias is consistent between the two core variables in the system. Moreover, it should be noted that, also by construction, the negative bias is largest in the first period under consideration, 1990, and steadily decreasing ever after. If anything, it could be said that the FOD-transformation assigns a higher weight to later observations. Let us now turn to the term p: regardless of the square root term, which is always slightly negative, but decreasing over time, it is worth pointing out that the standardization of the core variables was such that their distribution would be centered around zero. Indeed, the term $\bar{\tilde{C}}_{i_t}$ is equal to, over the whole sample, -4.43e-09 for standardized per capita GDP and to 1.01e-09 for institutional quality.[11] It is easy to see, therefore, that in expectation term p is equal to 0, so that the only bias present is the small and time-weighted negative bias discussed for term q. Clearly, however, this is made possible by the standardization of core variables, which

[11] It is not exactly equal to zero because the standardization, as shown by Equations (6) and (7), is carried out individually for each panel, i.e. at the country level.

becomes necessary in light of the constraints imposed by the *pvar* package. Because of this ploy, however, the *pvar* is definitely to be preferred to *pvar2*, which criticalities cannot be reasonably contained.

4.2.2 The *pvar2* package estimation

Under the *pvar2* package, in fact, it is possible to avoid transforming the core variables, whereas it is not possible to include the ΓT term in the system, which becomes:

$$\tilde{C}_{it} = A_1\tilde{C}_{i,t-1} + A_2\tilde{C}_{i,t-2} + e_{it} \tag{18}$$

Since, as previously hinted, detrending does not bring the same effects as explicitly including the time variable as an exogenous regressor, using this package leaves sign error in the estimated autoregressive coefficients, or better said sign oscillations. This is especially true for what concerns the second autoregressive lags, as Section 5.2 will explain more closely.

4.3 Path dependence: Impulse Response Analysis and system stability

In Section 2.5, I introduced the concept of Impulse Response Functions as a measure of path dependence. Namely, rephrasing the reasoning within the PVAR framework, the duration of the persistence of an exogenous shock into the system determines the latter's path dependence; the persistence of an exogenous shock to a specific core variable on the current and future values of itself can be instead seen as path dependence in that variable. The IRA addresses both cases, provided that the system is stable. In this section, I will proceed first by describing under what conditions the PVAR is stable, and then proceed on to explain the derivation of the Impulse Response Functions and of the Orthogonalized Impulse Response Functions (henceforth IRFs and OIRFs, respectively). For the sake of simplicity, let us supersede the use of excessive notation to indicate standardization and FOD-transformation of variables: as the reader knows from the previous section, depending on what package is used variables undergo different transformations, yet the derivation of the stability condition and of IRFs and OIRFs is the same. Without loss of generality, let us focus on the autoregressive component of the PVAR. Leaving aside the ΓT term, and introducing lag operators, the system can be written as[12]:

$$(1 - A_1L - A_2L)\widehat{C}_{it} = e_{it} \tag{19}$$

[12]This derivation is based on Pesaran(2015a), Section A.18.2.

Where $(1 - A_1L - A_2L) = A(L)$. Note that the latter has the same functional form of the characteristic equation of the system, which is:

$$c(z) = I_2 - A_1 z - A_2 z^2 \tag{21}$$

Then, (19) can be re-written in matrix form as:

$$\begin{bmatrix} \widehat{C}_{it} \\ \widehat{C}_{i,t-1} \end{bmatrix} = \begin{bmatrix} A_1 & A_2 \\ I_2 & 0 \end{bmatrix} \begin{bmatrix} \widehat{C}_{i,t-1} \\ \widehat{C}_{i,t-2} \end{bmatrix} + \begin{bmatrix} e_{it} \\ 0 \end{bmatrix} \tag{22}$$

Or, in a more compact form:

$$\xi_t = F\xi_{t-1} + v_t \tag{23}$$

Where, of course:

$$\xi_t' = [\widehat{C}_{it}; \widehat{C}_{it-}1] \tag{24}$$

$$F = \begin{bmatrix} A_1 & A_2 \\ I_2 & 0 \end{bmatrix} \tag{25}$$

$$v_t' = [e_{it}; 0] \tag{26}$$

Assuming the initial values of $\widehat{C}$ are known, that is ξ_0, by recursive substitution the Vector Moving Average representation (VMA) of (23) is:

$$\xi_t = F\xi_0 + \sum_{s=0}^{t-1} F^s v_{t-s} \tag{27}$$

The long-term properties of the system, that is its stability, depend on the moduli of matrix F, the companion matrix of the PVAR, which must be strictly less than 1. Note that this condition is equivalent to say that the roots of the characteristic equation, (21), lie outside the unit circle, i.e. that $|z_i| > 1$, with $i = 1, 2$. Conditional on stability (let us anticipate it is satisfied under all specifications proposed in Section 5.2), let us re-write the complete moving average representation of the PVAR (i.e. including the ΓT term) as:

$$\widehat{C}_{it} = \sum_{j=0}^{\infty} D_j T_{t-j} + \sum_{j=0}^{\infty} \phi_j e_{t-j} \tag{28}$$

Where ϕ_j, the IRF, is defined as:

$$\phi_j = \begin{cases} I_2 & \text{if j=0} \\ \sum\limits_{q=1}^{j} \phi_{t-q} A_q & \text{if j=1,2,3...} \end{cases} \tag{29}$$

Clearly then, the value of matrix ϕ_j's elements returns the impact of an exogenous shock in either residual from the two single equation lines in the PVAR, on the value of each core variable at time j. The above derivation takes the reduced form PVAR as its starting point. This implies that the variance-covariance matrix of the errors is equal to:

$$\Sigma_e = \begin{bmatrix} E(e_{1,\,it})^2 & E(e_{1,i\,t}; e_{2,it}) \\ E(e_{1,i\,t}; e_{2,it}) & E(e_{2,i\,t})^2 \end{bmatrix} = \begin{bmatrix} \sigma_1^2 & \sigma_{12} \\ \sigma_{12} & \sigma_2^2 \end{bmatrix} \tag{30}$$

Because $\sigma_{12} \neq 0$, this also implies that an exogenous shock into, for instance, the single line equation of $\widehat{Y}_{it}$ is transmitted also to the single line equation that has $\widehat{I}_{it}$ as dependent variable, through the correlation of the errors. Identifying restrictions, however, can artificially impose $\sigma_{12} = 0$: using the Cholesky decomposition[13], let B be an upper triangular matrix such that $B'B = \Sigma_e$. The role of B is to impose that $\widehat{I}_{it}$ is causally prior to $\widehat{Y}_{it}$, i.e. that whereas $\widehat{I}_{it}$ is allowed to have a contemporaneous effect on $\widehat{Y}_{it}$, the opposite is not true. B will then have the form $\begin{bmatrix} 1 & b_{12} \\ 0 & 1 \end{bmatrix}$, where b_{12} is the contemporaneous coefficient of $\widehat{I}_{it}$ as a regressor of $\widehat{Y}_{it}$. Only with this framework in mind I can specify the OIRF, which is $B\phi_j$, i.e. the impact on each core variable of a unitary exogenous shock in the error of its line equation in the PVAR, as orthogonalized from movements in the error of the other line equation.

5 Results and discussion

5.1 First stage

The first stage regressions, as specified in Equations (2a) and (3a), return the following results:

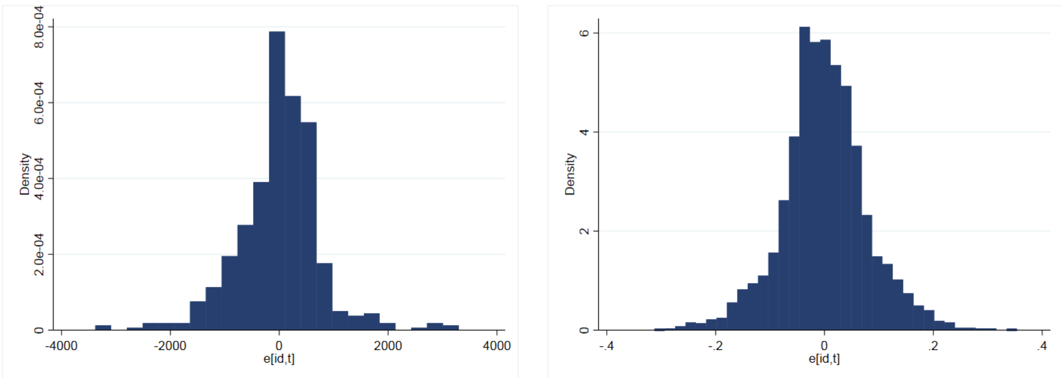
[13] Many other forms of decomposition could be used to impose identifying restrictions.

Table 2: First stage regressions results

	Y_{i_t}	I_{i_t}
Turnout$_{i_t}$	-7.02**	-0.001***
	(3.025)	(0.000)
Compulsory voting dummy$_i$	-1865.053***	-0.490
	(279.417)	(0.632)
Net exports$_{i_t}$	0	0
	(0.000)	(0.000)
Urban population$_{i_t}$	0	
	(0.000)	
Net oda received$_{i_t}$	-4.513**	
	(1.902)	
gini$_{i_t}$	-14.456**	
	(6.10)	
α_i	yes	yes
γ_t	yes	yes
intercept	yes**	yes**
N	164	164
T	21	21
R^2 within country	0.54	0.5
ρ (Fraction of σ_e due to α_i)	0.920	0.815

Notes: the displayed standard errors are robust and clustered at country level. Level of statistical significance of coefficients:***(1%), **(5%), *(10%).

Firstly, following the data overview in Section 3.3, the reader should not be surprised that the coefficients of turnout are negative and significant. Moreover, compared to the magnitude of both dependent variables, these negative coefficients are very small. It should also not surprise that the intercept is statistically significant, for in the first stage I_{it} is not included as a regressor of Y_{it} and vice versa. The rest of the coefficients – besides those of net exports and urban population, perhaps – appear to be in line with theoretical expectations which, together with the distribution of residuals as shown in Figure 7, supports the belief that the first stage regressions are well specified. As to why the above-mentioned coefficients are not statistically different from zero, the reason lies certainly in multicollinearity: in particular, a deeper analysis of correlation among controls shows a strong correlation of both net exports and urban population with the oda and the time variables (the latter meant as a proxy of year fixed effects variable), where, however, the sign of the correlation varies with the country's level of development. Therefore, one might expect multicollinearity to affect the significance of their coefficients and the variation over economic development levels to push them towards a sample average which lies around zero. The coefficients, therefore, are completely uninformative. This, however, should not come as a surprise to the reader: the objective of the first stage, in fact, is not to obtain a sound measure of coefficients, but rather to clean the two core variables of third influences in an effective way. This implies that the choice of covariates is, rather than aimed at measuring impacts, directed towards the best possible fit of the data (in this case through the Akaike criterion), regardless of multicollinearity issues. If any improvement can be brought to the first stage specification, therefore, it should concern the criterion applied to the selection of the best-fitting model, not the obtained estimates of coefficients. Finally, it should be noted that first stage variables are not standardized: the measure of coefficients in general (i.e not restricted to net exports and urban population) is not meant to be informative, since very different units of measurement are used (see Table 11 in Appendix A for summary statistics of control variables).



((A)) *Per capita GDP as dependent variable*

((B)) *Institutional quality as dependent variable*

Figure 7: Distribution of residuals from the first stage

5.2 Second stage

The following table illustrates the results obtained from estimating Equation (8) with the *pvar* package, that is the preferred specification of the base model.

TABLE 3: *pvar* package results

	$\widehat{\tilde{Y}^*}_{it}$	$\widehat{\tilde{I}^*}_{it}$
$\widehat{\tilde{Y}^*}_{i,t-1}$	.109***	.173**
	(0.691)	(0.040)
$\widehat{\tilde{Y}^*}_{i,t-2}$	.169**	.106**
	(0.078)	(0.050)
$\widehat{\tilde{I}^*}_{i,t-1}$	.233***	.146*
	(0.042)	(0.079)
$\widehat{\tilde{I}^*}_{i,t-2}$	.204***	.128**
	(0.031)	(0.059)
t	yes	yes
t^2	yes	yes
intercept	no	no
N	164	164
T	21	21

Notes: the displayed standard errors are robust and clustered at country level.
Level of statistical significance of coefficients:***(1%), **(5%), *(10%).

The following instead is the variance covariance matrix Σ_e attached to the estimated model:

$$\Sigma_e = \begin{bmatrix} \sigma_1^2 & \sigma_{12} \\ \sigma_{12} & \sigma_2^2 \end{bmatrix} = \begin{bmatrix} .260 & \\ -.113 & .208 \end{bmatrix} \tag{31}$$

Where subscript 1 denotes the equation with institutional quality as dependent variable, and 2 the equation with per capita GDP as dependent variable. Note also that the system is stable according to the definition given in Section 4.3 (the moduli of the companion matrix are equal to .728, -.409, .001, -.001).

Both core variables are confirmed to follow an autoregressive behaviour with positive signs, and to influence each other positively, as expected. Note that, because the variables are standardized so that their mean is zero and their variance 1, coefficients can approximately be read as half the impact: for instance, a unitary increase in the standardized version of demeaned per capita GDP implies a lagged effect on the standardized demeaned institutional quality equal to 10% of the total range. Because the latter is [-1, 1], the coefficient can be read as 20% of the maximal value, had the range been constrained to positive values only. It is also interesting to notice that the lagged effect of institutional quality on growth is approximately double that of growth on institutional quality. The following are the IRFs and OIRFs obtained under this specification. The headings indicate the variables as impulse:response:

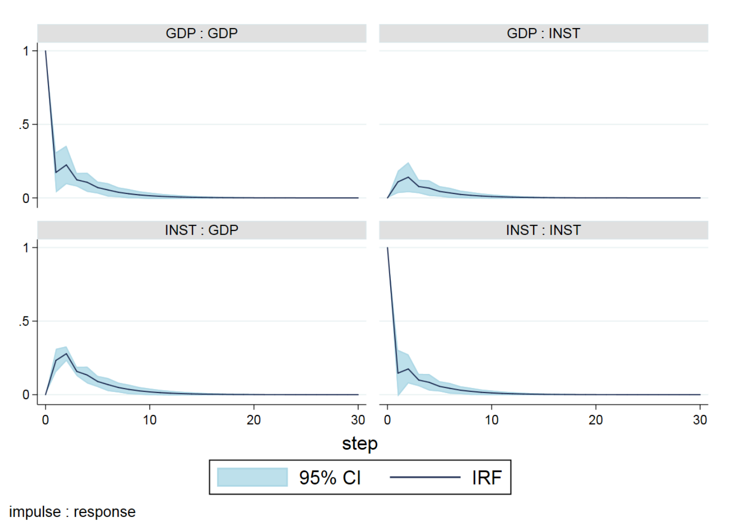


FIGURE 8: IRF from base PVAR, Equation (8), *pvar* package

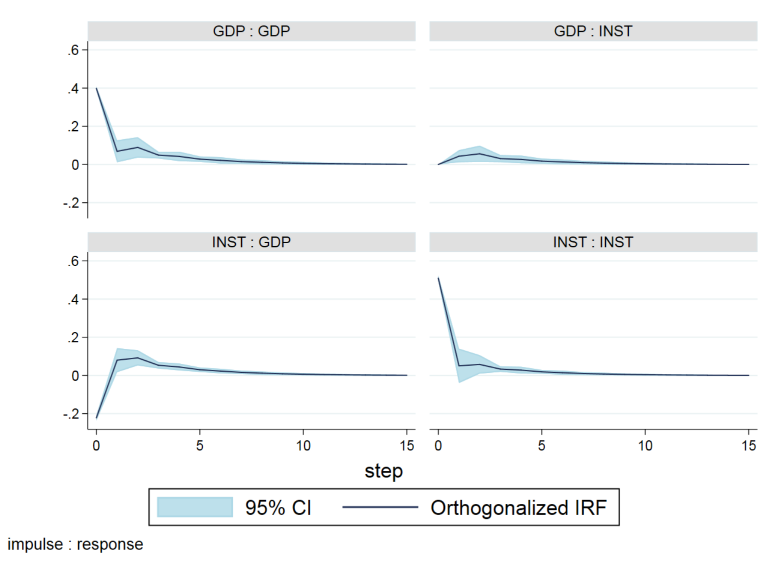


FIGURE 9: OIRF from base PVAR, Equation (8), *pvar* package

As expected, the IRFs exhibit a strong and fastly-decaying autoregressive behaviour of both variables, and a stronger and more significant effect of institutional quality on growth rather than the opposite: if the exogenous shocks are isolated by single equations, as displayed by the OIRFs, the effects are downscaled. Nevertheless, the actual effect of an hypothetical exogenous shock on both variables is clearly more truthfully represented by the IRFs, which allow non-zero correlation between the residuals of single equations. As to the negative value of the OIRFs representing the impact of a positive exogenous shock in institutional quality on growth, I interpret it as an adjustment period, because the impact resorts to positive within the immediate short-term. Nonetheless, I will return on the negativity of the OIRF when discussing the difference in results between the *pvar* and *pvar2* packages. Speaking of the latter, which differs from *pvar* for the exclusion of the ΓT term and the absence of any additional transformation in the core variables, it returns the following results and IRFs[14]:

Table 4: *pvar2* package results

	$\widehat{\widetilde{Y}}_{it}$	$\widehat{\widetilde{I}}_{it}$
$\widehat{\widetilde{Y}}_{i,t-1}$	1.218***	.149***
	(0.031)	(0.021)
$\widehat{\widetilde{Y}}_{i,t-2}$	-.312***	-.131***
	(0.030)	(0.020)
$\widehat{\widetilde{I}}_{i,t-1}$	.038***	1.21***
	(0.014)	(0.017)
$\widehat{\widetilde{I}}_{i,t-2}$	.059***	-.343***
	(0.013)	(0.021)
t	no	no
t^2	no	no
intercept	no	no
N	164	164
T	21	21

Notes: the displayed standard errors are robust and clustered at country level.
Level of statistical significance of coefficients:***(1%), **(5%), *(10%).

Following the same notation as above, the Σ_e matrix is:

$$\Sigma_e = \begin{bmatrix} \sigma_1^2 & \sigma_{12} \\ \sigma_{12} & \sigma_2^2 \end{bmatrix} = \begin{bmatrix} .104 & \\ .026 & .083 \end{bmatrix} \tag{32}$$

The moduli of the associated companion matrix are .880, .833, .365, .365, which implies stability also for this estimated system.

[14]Unfortunately, the *pvar2* package does not return OIRFs.

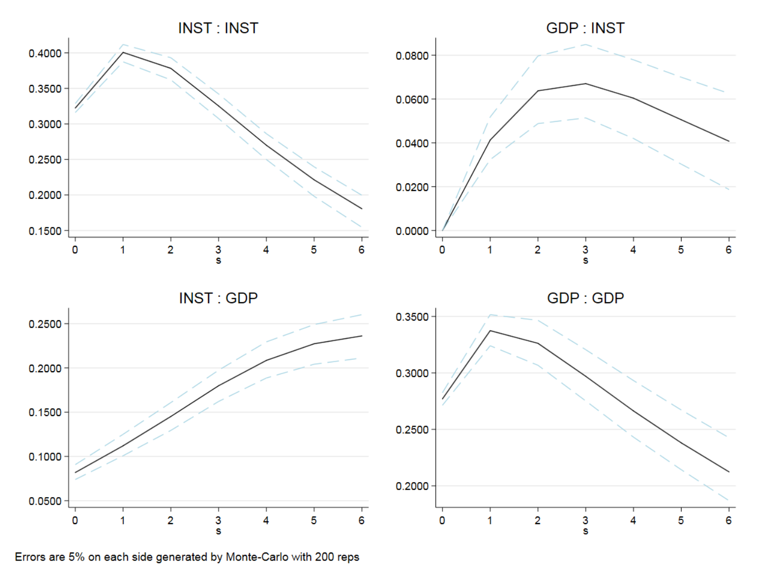


FIGURE 10: IRF from *pvar2* package estimation

The are two stark observations to make relative to the bias of estimated coefficients under this alternative specification. Firstly, as already anticipated, second order autoregressive components' bias results in a sign error. Secondly, the magnitude of positive coefficients is notably amplified relative to the *pvar* specification. It is necessary to asses whether these differences are attributable to the absence of the ΓT term, and hence are to be considered as omitted variable bias, or whether they are due to the absence of the FOD transformation-induced negative bias. Appendix B presents a *pvar*-package estimated model without the ΓT term. That is, a model with the FOD transformation-induced bias and without the ΓT. The results strongly support the ΓT hypothesis. As to why this happens even after controlling for time fixed effects in the first stage, the interpretation I give is that whereas the latter represent global time-varying factors, the ΓT, because it is specified as an exogenous regressor in each individual regression of the PVAR, may capture country-level, time-varying variables and trends which were not included in the first stage (quite simply, due to lack of data). Because it accounts for both the omitted variables and the trends, its inclusion is not equivalent to detrending the data within-country. Moreover, different panels might exhibit trends of different orders: detrending uniformly across panels might lead to unnecessary loss of information. The reader might finally wonder why the ΓT term is not controlled for in the first stage. In extremely simple terms, besides potential issues of multicollinearity with year fixed effects (γ_t), inclusion in the second stage proves more effective in improving the in-sample fit. In addition, as the interpretation of results could turn more difficult in presence of trends over hypothetically longer time horizons, inclusion in the second stage is a more conservative approach for the sake of generalizations of this study.

Turning to the IRFs:

I. Once adjusting for the scale of the graph, all but the bottom-left panels display similar dynamics to the *pvar* package case – besides the non-immediate peaking of autoregressive IRFs. The difference lies in the overstatement of these dynamics' magnitude.

II. The bottom-left panel exhibits concavity yet does not converge to 0 in the first 6 periods.[15] Indeed, Appendix B also confirms that the persistence of exogenous shocks is overstated with respect to the case in which the ΓT term is included.

III. This IRFs replicate very closely those obtained in Goés (2015), which suggests that the latter's result are inflated due to misspecification, due to both the absence of the first stage (as suggested by the results in Appendix B) and to the exclusion of the ΓT term. Note that Goés specifies only one lag of autoregressive components: because the sign errors caused by the exclusion of the ΓT term appear for the coefficients of second order autoregressive components, specifying a PVAR or order 1 can hide the issue of time trends to the eye of the researcher.

In conclusion, the reader should acknowledge that neither package supports autocorrelation tests for residuals. These are, however, stationary at the 95% confidence level according to the Im-Pesaran-Shin test for panel unit roots, which is preferred because it works under the assumption of fixed T and large N, which is conform to the present case (Im, Pesaran and Shin, 2003). Moreover, it should be noted that standard errors' clustering was adopted to avoid incidence of leftover residuals' serial correlation on the significance of coefficients via wrong standard errors: indeed, serial correlation is allowed within panels, so that the truthfulness and reliability of significance tests of coefficients is not impacted by serial correlation of residuals. What could potentially be affected, instead, is the covariance of single equation lines' residuals σ_{12} (Pesaran, 2015a) hence the IRFs: the OIRFs, then, by imposing said covariance to be equal to zero, can be seen as a robustness check *per se*.

Finally, because of the apparent resemblance of the procedure proposed with two-stages least squares and, to some extent, to GMM, the reader might argue that covariates of first stage's regressions should be orthogonal to the PVAR errors, and that in presence of overidentification (i.e. when the number of chosen covariates exceeds the number of instrumented variables, as in this case) this could be tested. In this regard, three aspects should

[15] Under the *pvar2* package, it is also not possible to extend the horizon of IRFs to show the convergence period.

be clarified. Firstly, the first stage is only meant to clean core variables from correlation with third ones, and it was not envisaged to comply with moment conditions. Secondly, because the PVAR presented is one without dynamic interdependencies – again following Canova and Ciccarelli's nomenclature – and hence, conditional on a correct specification of the first stage, the variables on the right hand side of each PVAR's equation are weakly exogenous to their single equation's residual, in a dynamic panel setting the consistency and efficiency of the estimators is ensured (Baltagi, 2013). In simpler words, because both the first and the second stage return pooled estimators which result from treating observations as stacked over panels (i.e. variables are regressed on each other within country, and a pooled estimator of all the estimated relationships is obtained, if one were to extremely simplify), each within-country relationship should not suffer from omitted variable bias and yield altogether a consistent pooled estimator. Elements of potential interdependencies such as international trade, globalization and the global financial crisis are being considered via the inclusion of net exports and year fixed effects in the first stage. Thirdly and finally, neither two-stages least squares nor GMM would admit fixed effects as instruments, yet they are included in the present first stage. Moreover, because the two are not discernible one from the other, it is technically impossible to construct an informative J statistics to conduct the Hansen's test of overidentifying restrictions. Besides the aforementioned role played by time fixed effects to sustain the assumption of dynamic homogeneity of panels, I have also argued before that the importance of country fixed effects is paramount to this study, for they are expected to capture a significant portion of social capital's variation. Indeed, country fixed effects constitute a sizable amount of variation in residuals, as shown in Table 2 (respectively, 92% for the per capita GDP first stage regression and 82% for the institutional quality one).

5.3 Does the first stage make a difference?

Before turning to robustness checks of the proposed procedure, it is legitimate to wonder whether the latter brings any added value at all, compared to an analysis without any demeaning first stage, as in Goés (2015) – still including the ΓT term though. To answer this question, the following model is estimated:

$$\tilde{C^*}_{it} = A_1 \tilde{C^*}_{i,t-1} + A_2 \tilde{C^*}_{i,t-2} + \Gamma T + e_{it} \tag{33}$$

This model differs from the one specified by Equation (8) in that core variables are not subject to the first stage demeaning, but only to standardization and FOD transformation (the *pvar* package is used). Setting aside the notation, this table shows, equation by equation, the results from the models in (8) and (33), where the latter are highlighted in light gray.

Table 5: Results with and without including the first stage

	(8)	(33)	(8)	(33)
	Y_{it}	Y_{it}	I_{it}	I_{it}
$Y_{i,t-1}$	.109***	.188*	.173**	.080
	(0.691)	(0.112)	(0.040)	(0.050)
$Y_{i,t-2}$	.169**	.199**	.106**	.085**
	(0.078)	(0.068)	(0.050)	(0.043)
$I_{i,t-1}$	.233***	.346***	.146*	.148**
	(0.042)	(0.035)	(0.079)	(0.058)
$I_{i,t-2}$	.204***	.298***	.128**	.127**
	(0.031)	(0.033)	(0.059)	(0.050)
t	yes	yes	yes	yes
t^2	yes	yes	yes	yes
intercept	no	no	no	no
N	164	164	164	164
T	21	21	21	21

Notes: the displayed standard errors are robust and clustered at country level.
Level of statistical significance of coefficients:***(1%), **(5%), *(10%).

It can be noted that under Equation (33) both the autoregressive component of per capita GDP and the impact of institutional quality on per capita GDP are larger. Conversely, the reverse impact is underestimated both in terms of magnitude and statistical significance. This is then mirrored also by the IRFs and OIRFs reported below, as derived from (33) (Figures 11 and 12, respectively). In addition, the adjustment mechanism of the impact of an exogenous shock in institutional quality on per capita GDP appears to be underestimated in its magnitude. Clearly, both core variables, under (33), carry in the model the effects of other variables: whereas for per capita GDP their nature can be varied, and indeed the range of potential drivers chosen for this analysis could turn out to be still limited, in the case of institutional quality it becomes clear that, under (33), social capital's effect on per capita GDP, as represented by unobservable sectional fixed effects and turnout, are absorbed by the institutional quality indicator. Conditional on a correct specification then, the first stage does make a notable difference, at least in this specific application.

FIGURE 11: IRF from Equation (33)

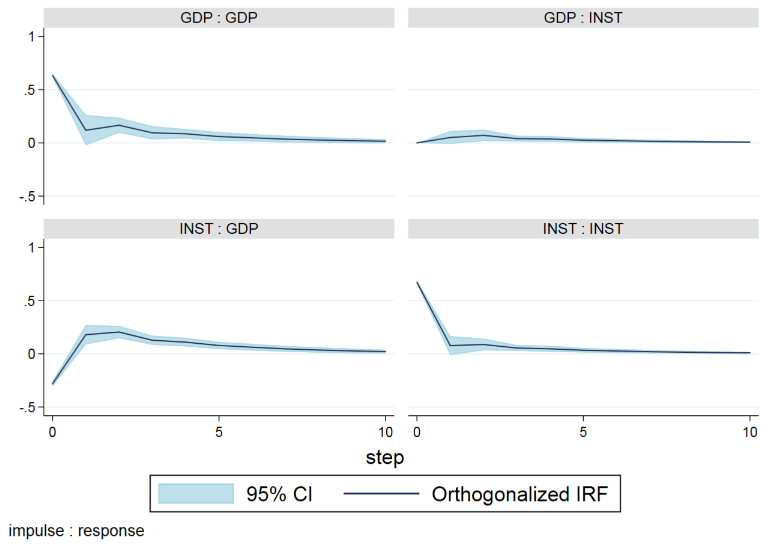


FIGURE 12: OIRF from Equation (33)

6 Robustness checks

In light of the empirical strategy described in Section 4.2, and of the computational limits outlined, there are two types of robustness checks I present. These are the inclusion of a pooled constant in the PVAR, and the application of the opposite Cholesky decomposition with respect to the one specified previously.

6.1 Including a pooled intercept

Because the aim of the procedure is to enable estimation of the PVAR coefficients by OLS, in the absence of a constant, the main robustness check needed is the inclusion of said constant, to verify whether it is significant or not, and, if yes, what is its sign. Once again, there are notable computational obstacles in the way (neither the *pvar* nor the *pvar2* packages allow for the inclusion of an intercept). This robustness check is carried out with the use of the *xtvar* package which, however, estimates the PVAR coefficients as Least Square Dummy Estimators (henceforth LSDV). Following Pesaran (2015a), the LSDV estimator is equivalent to a fixed effect one, but computationally more burdensome. Because, however, it incorporates spatial fixed effects in the PVAR, it is necessary to obtain the demeaned variables without pre-demeaning them of the α_i term. Moreover, because the *xtvar* package does not allow to include exogenous regressors and hence impedes to include the ΓT term, the newly demeaned variables were also detrended, within-country.[16] The MMSC-A criterion then selects one lag as the optimal specification. The obtained results are:

Table 6: LSDV with intercept robustness check, results

	$\widehat{\tilde{Y}^{det}}_{it}$	$\widehat{\tilde{I}^{det}}_{it}$
$\widehat{\tilde{Y}^{det}}_{i,t-1}$	.125***	.155***
	(0.018)	(0.033)
$\widehat{\tilde{I}^{det}}_{i,t-1}$	-.038***	.266***
	(0.009)	(0.017)
intercept	-.001	.003
	(0.003)	(0.005)
t	no	no
t^2	no	no
N	164	164
T	21	21

Notes: the displayed standard errors are robust and clustered at country level.
Level of statistical significance of coefficients:***(1%), **(5%), *(10%).

They display a non-significant pooled intercept, which supports the usefulness and correct specification of the first stage. The unexpected negative sign of the coefficient representing

[16]Which, as outlined in Section 5.2, does not entirely solve the omitted variable bias and therefore leaves the estimated coefficients overstated.

the lagged effect of institutional quality on growth can be attributed to the absence of ΓT term, as argued in Section 5.2.

6.2 Applying the opposite Cholesky decomposition

Should the purely qualitative hypothesis that institutional quality is causally prior to growth not hold true, it would be interesting to test whether the results obtained and presented in Section 5.2 are robust to the opposite assumption, i.e. that growth is causally prior to institutional quality. The expectation is that results will remain mostly unchanged, for the correlation of residuals, as extracted from the Σ_e matrix, is rather weak ($\sigma_{12} = -0.113$). This is done by re-estimating the IRFs and OIRFs under the *pvar* package[17], which I found to be preferrable in terms of specification possibilities, and by changing the elements of matrix B. That is, the estimated model is, in its reduced form:

$$\tilde{C^*}_{it} = A_1 \tilde{C^*}_{i,t-1} + A_2 \tilde{C^*}_{i,t-2} + \Gamma T + e_{it} \tag{8}$$

and the B matrix used to reconstruct its structural form is of the form $\begin{bmatrix} 1 & 0 \\ b_{21} & 1 \end{bmatrix}$, where b_{21} is the contemporaneous coefficient of per capita GDP when institutional quality is the dependent variable. As the following exhibits show, the results are robust in terms of estimated IRFs and OIRFs. The main difference, in the OIRFs, is the potential underestimation (or overestimation, depending on the point of view) of the short-term impact of an increase in institutional quality on GDP (after one year, the effect under the new Cholesky decomposition is not statistically significant, whereas it is under the original one). It can be concluded that, as expected, within the context of this specific case study, results are mostly robust to theoretical misconceptions of causal priority.

[17]Since the identifying restriction does not affect the estimation of the reduced form PVAR, but only its IRFs and OIRFs.

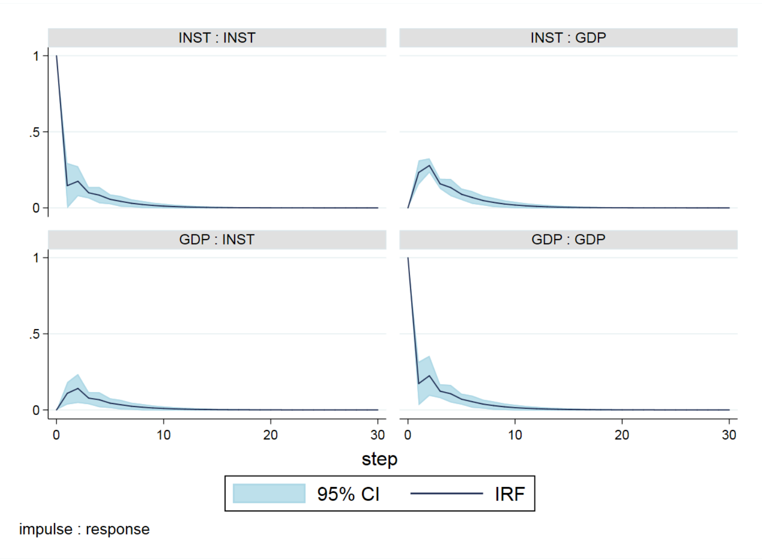


FIGURE 13: IRF from PVAR when per capita GDP is assumed to be causally prior to institutional quality

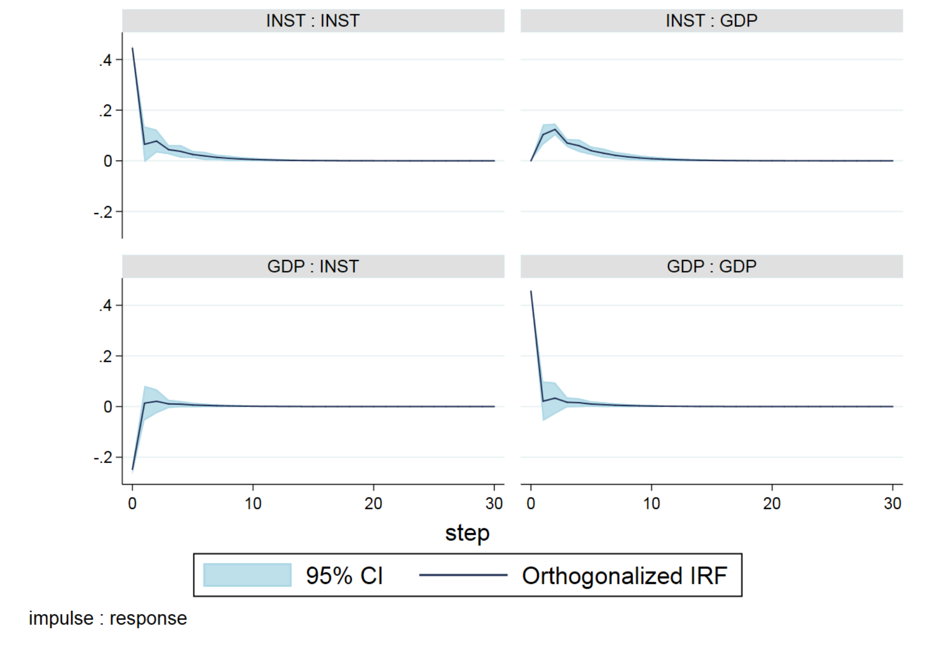


FIGURE 14: OIRF from PVAR when per capita GDP is assumed to be causally prior to institutional quality

7 Limitations and potential solutions

There are a number of limitations affecting this study. Some of them are peculiar to this applied exercise (i.e. to the institutions-growth relationship). Just as a summary of aspects which have been already discussed in previous sections, these are:

I. **Computational obstacles**: the current availability of PVAR packages in statistical softwares represents a major obstacle for any potential future application of this procedure. The *pvar* and *pvar2* packages in Stata, as developed by M.Abrigo and I.Love, are the most elaborate to date, yet they constrain the user to rely on GMM estimators and only include a limited array of specification options, as discussed in Section 4.2. To date, a number of compromises can be reached by carefully "playing around" with the options, but there are still notable limitations to the possibility of running robustness checks (for instance, by including an intercept or by being able to add exogenous variables in the Ω_{it} vector of Equation (1)), without being obliged to use those also as instruments for a GMM estimation. Developing a more flexible software package was beyond the scope of this master thesis, but it is certainly a research interest for future projects.

II. **Consideration of Social Capital**: as hinted previously, social capital should theoretically be included in the second stage, yet data availability issues did not allow to have a valid proxy with the desired frequency and spatial coverage, for the survey-based variables available were scattered point-estimates in time, available for a more restricted sample than the 164 countries used in this study. Moreover, interpolation would have not been a feasible solution, because firstly the variation in the data would have remained too scarce in relation to the 21 years time-span I am considering [18], and secondly because, as a consequence, country fixed effects would have likely resulted the only suitable predictor of social capital.

III. **The strategic use of panel data to approach sample size**: the use of longitudinal data might not be suitable to different case-studies as it is for this thesis. Other researchers might be interested in cases of reverse causality in a narrower context, e.g. in a single country. Whereas considering smaller cross-sections in a PVAR rather than a normal VAR model might still be preferable (for instance, because of the wider information set), data contingency often imposes constraints on the granularity one can aim for. Relative to data contingency issues, in those cases where data scarcity makes the application of this procedure problematic for the properties of the estimators – especially for particularly small T –, it might be advisable to take a Bayesian rather than a frequentist approach. Clearly, a Bayesian PVAR (BPVAR) requires a completely different approach to the specification which this study does not cover.

IV. **A wide range of alternative choices**: finally, besides limitations, it is worth mentioning that most of the estimation techniques applied in this specific study could

[18] And, of course, interpolation *per se* does not add variability.

be easily changed to adapt to other researchers' necessities. For instance, in the first stage, others might be motivated by theory to choose random rather than fixed effects and, in the latter case, to estimate parameters as Least Square Dummy Estimators instead of applying Generalized Least Squares. Alternatively, although as explained in next paragraph the first stage *per se* cannot be estimated by IV or GMM, the procedure might be revised by making the first stage closer to that of a GMM estimation of the PVAR parameters (yet still different from GMM), by expliciting the moment conditions to be met in the first stage along a weighting matrix to signal their relative importance (if fixed effects can be safely excluded from the first stage). The long list of necessary choices for the application of the envisaged procedure to the institutions-growth' case study makes the number of alternative empirical choices too long to be able to provide an exhaustive list of them. Nevertheless, this flexibility might represent a strength of this approach rather than a limitation: by implementing the necessary changes, the procedure can be applied to a wide range of contexts. In this sense, including a satisfactory array of options in future research – e.g. in the creation of a new comprehensive software package – would certainly constitute a priority.

Other limitations of the procedure prescend by the institutions-growth' relationship and are instead of a more general nature: clearly, the most pressing one is the huge importance of correctly specifying the first stage. The latter, however, is also highly data-contingent: despite controlling for both time and section fixed effects, it is necessary to have a reasonable set of time and space varying covariates to properly account for potential omitted variable bias. Notice that, because each core variable in vector C_{it} has its own first stage equation, it is not possible to estimate the first stage by IV or GMM with the aim of accounting for omitted variable bias. In fact, conditional on the core variables being a significant regressor of each other, a well-specified IV or GMM estimated model would deprive the demeaned core variable of its dependence from other elements of C_{it}, thus artificially making the coefficients of interest of the PVAR insignificant.

Data contingency in terms of measurement error in the core variables can produce a similar issue if the variation in the core variables as reported by the data is lower than the actual one (or, if the nature of core variables is such that they exhibit a very low variability over time or over space): such a variable might be easily captured by the fixed effects in the first stage equations of other core variables.

8 Conclusions

In this study, I estimate a Panel Vector Autoregressive model (PVAR) to assess the two-way relationship between institutional quality and per capita GDP over time, in a longitudinal dataset including 164 countries over the period 1990–2010. I elaborate a two-stages procedure to ensure the maximal attainable accuracy of both the estimated coefficients and the Impulse Response Functions associated with the model, for the PVAR estimation is preceded by a two-way fixed effect model for each variable of interest which is aimed at demeaning them of all accountable time-and-space varying factors (except, of course, the other variables of interest). The rationale is that of incorporating into an econometric model the unavoidable endogeneity of both variables when considered as explanatory factors of one another, because of the presence of reverse causality – which has affected a number of prominent studies addressing the relationship between institutional quality and economic development. Moreover, a system of endogenous variables such as a PVAR enables, conditional on an identifying assumption of causal priority based on theory, to measure path dependence of the variables in analysis, a concept which has been largely misused in economic and sociological literature insofar. Summarizing, this thesis has produced mainly four results.

Firstly, the quality of institutions and per capita GDP significantly affect each other positively, with the inclusion of lagged effects (here, two lags were included to fit best the data, according to the MMSC-A criterion for model selection). It should be noted, however, that the selection of the lags' order is contingent to the data at hand, and in this case the temporal sample size amounts to 21 years. Within the sample in use, the autoregressive components of each variable are significant, positive, and comparable in terms of magnitude – a unitary increase at $t = 0$ implies an increase between 11% and 17% of the original one, in the following two periods ($t = 1, 2$). On the other hand, the lagged effect of institutional quality on economic development is larger than the effect in the opposite direction, and more persistent in terms of size (a unitary increase in institutional quality at $t = 0$ implies a lagged effect on per capita GDP of approximately 23% and 20% of the original increase, whereas the corresponding approximate figures for the opposite effect are 17% and 11%). Secondly, previous studies that also attempt to incorporate reverse causality by means of a PVAR model, such as Chong and Calderón (2000) and Goés (2015), because of what I find to be omitted variable bias, exasperate both the magnitude of their mutual impacts and the persistence of exogenous shocks on the time dynamics of the variables. The latter, in fact, which I ascribed to the definition of path dependence from Paul David, is less persistent than in the aforementioned studies – which results I am able to replicate conditional on misspecification, namely neglecting the presence of country-specific time trends in the relationship, which this discussion has referred to as the ΓT term. Thirdly, the isolated impact on per capita GDP of a positive unit shock in the quality of institutions' indicator, as indicated by the OIRF, is found to be subject to adjustment mechanisms in the very short term (i.e. approximately one year), a finding which is again conditional on complete specification of the PVAR – the inclusion of the ΓT term.

The main reason that differentiates this thesis from the aforementioned studies, however, is the inclusion of the first stage of estimation, aimed at "cleaning" the two variables of interest of all measurable time-and-space varying explanatory factors whose correlation with core variables that might bias the PVAR results if omitted. Indeed, the fourth and fundamental finding is the validity of including such first stage, as indicated by the reduction of biases in the coefficients, as explained is Section 5.3. In this sense, previous studies such as Goés (2015) are suggested to report biases due to both the exclusion of an adequate demeaning stage and to the exclusion of explicit time dynamics in the model (which are interpreted to capture the effect of further omitted covariates in the first stage, for instance because of lack of data). Moreover, the main advantage of expliciting this two-stages procedure is that it can be potentially applied to a wide range of research topics affected by reverse causality. Indeed, it can be enlarged and modified to tackle other specific case-studies with accuracy: just as an instance, provided the system is overidentified[19], it is possible to incorporate a GMM-style weighting matrix on the conditions a researcher may want to apply to the autoregressive components of the PVAR. The different scope of the first stage in this study, however, as represented by the use of variables motivated by economic theory instead of autoregressive components or first differences as first-stage "instruments", together with the inclusion of both spatial and temporal fixed effects, distances the procedure from both the Arellano-Bond and Blundell-Bond procedures, which constitute a praxis in the case of PVAR models.

Clearly, the procedure still presents a number of limitations which, summarizing, can be currently ascribed to the combination of data contingency – an issue that characterizes most empirical studies in all social sciences in any case –, and of computational barriers posed by the currently available software packages dedicated to the estimation of PVARs in a frequentist approach.[20] In this regard, it is my intention, in the future, to program a more flexible software package in order to allow not only a more precise estimation of the procedure described for the present case-study, but also to leave room to other researchers to fine-tune it to diverse topical applications. This project, which constitutes the core of

[19]That is, that the number of first stage regressors is strictly greater than the endogenous regressors in the PVAR, where the latter is equal to the number of core variables times the selected lag order.

[20]The latter have also made it necessary to dig into a certain degree of technicality over my discussion, for which I would like to apologize to the reader.

the future research advisable in this thesis, shall also incorporate the possibility to carry out tests which were not always possible to run in this study. This, therefore, is nothing but a partial and exploratory analysis.

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Appendix A. Demeaning core variables: control variables and summary statistics

The reader may recall from Section 4 that the first stage consists of the estimation of the following equation models, aimed at demeaning the GDP and institutional quality index variables:

$$Y_{it} = \beta_1' Turnout_{it} + \delta_1' \Omega_{1,it} + \alpha_{1,i} + \gamma_{1,t} + u_{1,it} \tag{2a}$$

$$I_{it} = \beta_2' Turnout_{it} + \delta_2' \Omega_{2,it} + \alpha_{2,i} + \gamma_{2,t} + u_{2,it} \tag{3a}$$

Based on the Akaike criterion of model selections, the elements of $\Omega_{n,it}$ where n = {1,2} were selected among:

Table 7: Data sources and first stage covariates

Variable	Type/ denomination	Source	Employed in:
Compulsory voting	Dummy=1 if voting compulsory in country i at time t	Self-created based on IDEA information	(2a), (3a)
Voting age	Minimum age to vote in country i at time t	IDEA	
Population	Number of inhabitants	WB	
Urban population	% population living in cities	WB	(2a), (3a)
Gini coefficient	Gini coefficient estimate	WB	(2a)
High_Ten	% of country i GNI held by richest 10% of population	WB	
Patents	Patent application from residents of country i at time t	WB	
Oda	Net ODA received per capita	WB	(2a)
Literacy	Literacy rate of population above 15	WB	
Life	Life expectancy at birth, in years	WB	
Net exports	Exports in current US $ - Imports in current US $	Self-created based on WB data	(2a), (3a)
Inflation	CPI index, growth rate in country i from t-1 to t, relative to time t	WB	
Agriculture value added	As a % of GDP	WB	
C02 emissions	In tons per capita	WB	
Domestic credit	To the private sector, as a % of GDP	WB	

TABLE 8: Original core variables: summary statistics

	Y_{it}	I_{it}	$Turnout_{it}$
N	3444	3444	3444
Mean	9047.806	0.501	68.783
Median	2326.952	0.496	70.795
Standard deviation	16617.73	0.195	23.16
Kurtosis	24.824	2.391	4.21
Max	192989.200	.952	100
Min	65.011	.012	0

TABLE 9: Demeaned core variables: summary statistics

	$\widehat{Y}_{it}$	$\widehat{I}_{it}$
N	3444	3444
Mean	8930.300	.502
Median	7711.574	.486
Standard deviation	3160.084	.179
Kurtosis	2.704	2.472
Max	15133.960	.569
Min	5787.382	.364

TABLE 10: Demeaned and original core variables: correlation matrix

correlation matrix				
	Y$_{it}$	I$_{it}$	$\widehat{Y}_{it}$	$\widehat{I}_{it}$
Y_{it}	1			
I_{it}	0.128	1		
$\widehat{Y_{it}}$	0.147	0.257	1	
$\widehat{I_{it}}$	0.144	0.916	0.280	1

Table 11: Control variables in use, summary statistics

	Urban pop$_{it}$	gini$_{it}$	oda$_{it}$	Net exports$_{it}$
N	3444	3444	3444	3444
Mean	.532	40.474	96.407	-6.05e+08
Median	.519	39.500	36.345	-1.46e+08
Standard deviation	.244	9.996	366.181	5.10e+10
Kurtosis	2.003	2.091	542.2191	132.992
Max	1	65.800	12012.97	3.28e+11
Min	.054	16.200	-133.559	-7.71e+11

Table 11 highlights a vast difference in units of measurement used for control variables. Moreover, notable differences emerge relative to their distributions (for instance, as it would be reasonable to expect, the variable oda has a much more skewed distribution compared to other covariates). As the summary statistics for the net exports variable indicate, the distribution is skewed towards net importers.

Clearly, not many inferences can be made from summary statistics over the whole sample. On the other hand, presenting country-level summary statistics would result excessive for the scope of this thesis. The reader might want to believe, however, that at the country level these controls correlate very highly in absolute value (especially oda, net exports and both of them with the time variable), although the sign of such correlation is highly dependent on the level of economic development of the country, and on whether the latter has particular features relative to international trade (e.g., oil exporting countries show a lower correlation of net exports with other control variables, as one could reasonably expect).

Appendix B. A *pvar* package estimation without ΓT term

The alternative model estimated, in its reduced form, is:

$$\tilde{C^*}_{it} = A_1\tilde{C^*}_{i,t-1} + A_2\tilde{C^*}_{i,t-2} + e_{it} \tag{8*}$$

The results obtained are:

TABLE 12: *pvar* package results omitting the ΓT term

	$\widehat{\tilde{Y^*}}_{it}$	$\widehat{\tilde{I^*}}_{it}$
$\widehat{\tilde{Y^*}}_{i,t-1}$	1.16***	-.105***
	(0.009)	(0.012)
$\widehat{\tilde{Y^*}}_{i,t-2}$	-.260***	-.090***
	(019)	(0.018)
$\widehat{\tilde{I^*}}_{i,t-1}$	-.105***	1.202***
	(0.014)	(0.010)
$\widehat{\tilde{I^*}}_{i,t-2}$	.168***	-.315***
	(0.013)	(0.012)
t	no	no
t^2	no	no
intercept	no	no
N	164	164
T	21	21

Notes: the displayed standard errors are robust and clustered at country level.
Level of statistical significance of coefficients:(1%), (5%), (10%).

Also in this case, the system is stable under the definition presented in Section 4.3. The moduli of the companion matrix are: .880, .834, .364, .364. Whereas the IRFs are extremely close to those from the *pvar2* specification (Figure 12), the OIRFs obtained are:

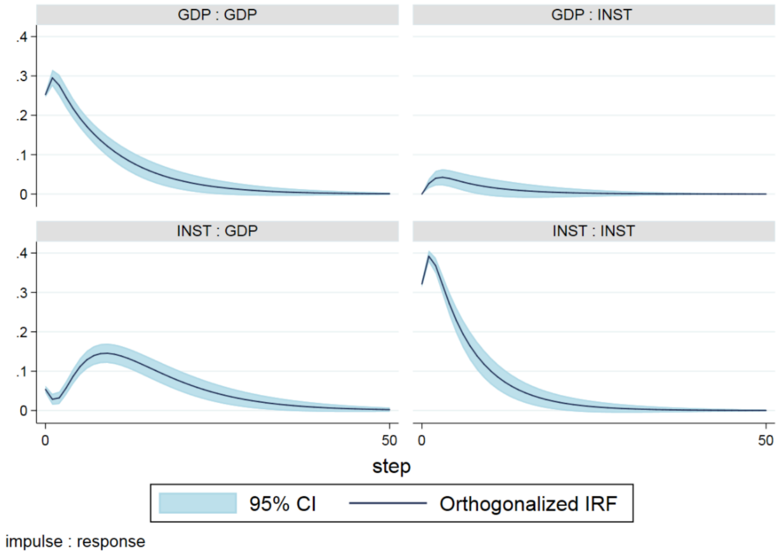

FIGURE 15: OIRF from Equation (8*)

Appendix C. List of countries in sample

The complete list of countries in sample in alphabetical order is:

A Albania; Algeria; Angola; Antigua and Barbuda; Argentina; Armenia; Australia; Austria; Azerbaijan.

B: Bahamas, The; Bahrain; Bangladesh; Barbados; Belarus; Belgium; Belize; Benin; Bhutan; Bolivia; Bosnia and Herzegovina; Botswana; Brazil; Bulgaria; Burkina Faso; Burundi.

C: Cabo Verde; Cambodia; Cameroon; Canada; Central African Republic; Chad; Chile; China; Colombia; Comoros; Congo Dem. Rep.; Costa Rica; Cote d'Ivoire; Croatia; Cuba; Cyprus; Czech Republic.

D: Denmark; Djibouti; Dominica; Dominican Republic.

E: Ecuador; Egypt Arab Rep.; El Salvador; Estonia; Ethiopia.

F: Fiji; Finland; France.

G: Gabon; Gambia The; Georgia; Germany; Ghana; Greece; Grenada; Guatemala; Guinea; Guinea-Bissau; Guyana.

H: Haiti; Honduras; Hungary.

I: Iceland; India; Indonesia; Iran Islamic Rep.; Ireland; Israel; Italy.

J: Jamaica; Japan; Jordan.

K: Kazakhstan; Kenya; Kiribati; Korea Rep.; Kuwait; Kyrgyz Republic.

L: Latvia; Lebanon; Lesotho; Liechtenstein; Lithuania; Luxembourg.

M: Macedonia; Madagascar; Malawi; Malaysia; Maldives; Mali; Malta; Mauritania; Mauritius; Mexico; Moldova; Morocco; Mozambique.

N: Namibia; Nauru; Nepal; Netherlands; New Zealand; Nicaragua; Niger; Nigeria; Norway.

P: Pakistan; Palau; Panama; Papua New Guinea; Paraguay; Peru; Philippines; Poland; Portugal.

R: Republic of The Congo (Brazzaville); Romania; Russian Federation.

S: Samoa; Sao Tome and Principe; Saudi Arabia; Senegal; Sierra Leone; Singapore; Slovak Republic; Slovenia; Solomon Islands; South Africa; Spain; Sri Lanka; St. Lucia; Sudan; Suriname; Sweden; Switzerland; Syrian Arab Republic.

T: Tajikistan; Tanzania; Thailand; Timor-Leste; Togo; Tonga; Trinidad and Tobago; Tunisia; Turkey; Tuvalu.

U: Uganda; Ukraine; United Kingdom; United States; Uruguay; Uzbekistan.

V: Vanuatu; Venezuela; Vietnam.

Y: Yemen Rep..

Z: Zambia; Zimbabwe.