

# Reply to the comments on ‘Institutions and Economic Development: Theory, Policy and History’

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**Abstract:** This article is a reply to the comments made on my target article, ‘Institutions and Economic Development: Theory, Policy and History’, at the beginning of this issue.

## 1. Introduction

I would like to begin by thanking the commentators for their comments on my target article (Chang, 2011). There are some commentators – Amitava Dutt (2011), Kenneth Jameson (2011) and Jaime Ros (2011) – who have made critical comments from methodological positions that are similar to mine. Their points are well taken. What few minor disagreements I have with them are probably not worth discussing in this article. Some others have accepted most of my substantive arguments, but coming from different methodological positions – Mwangi Kimenyi (2011) and Robbert Maseland (2011) from the orthodox position or David Ruccio (2011) from the Marxist position – they have some basic problems with my arguments. Kimenyi and Maseland take issue with my empirical methodology and Ruccio with my failure to engage in class analysis. However, in the case of these commentators, I think a productive dialogue is possible, as indeed Ruccio himself suggests it should be, although I do not think this reply is the place to conduct it.

Unfortunately, the reactions of the other commentators have been mostly very negative. Their criticisms fall into one of the following three types. First, they argue that I make extreme and one-sided theoretical claims against the dominant discourse on institutions and development. Second, they point out that even those criticisms of mine that are not so extreme are irrelevant because I am not attacking the ‘real thing’ but some distorted and/or partial version of the orthodox institutional literature. Third, they say that my arguments are based on

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an unscientific empirical methodology, which invalidates my evidence-based (as opposed to theoretical) criticisms. Having defined me as an extremist deploying a ‘straw man’ argument and using dubious empirical methodologies, most of these commentators seem to feel justified not to engage with my substantive criticisms.

In the rest of the article, I first discuss how many of the commentators in this issue misunderstand my arguments – sometimes even to the extent of believing that I am saying the opposite of what I actually say (section 2). Then I explain how many of them mistakenly accuse me of attacking some unrepresentative and/or simplified version of the mainstream institutional theory, rather than the ‘real thing’ (section 3). Then I discuss how the criticism by some of the commentators that my arguments are based on selective examples, rather than systematic empirical evidence, is based on a flawed methodological position (section 4). I then discuss a few substantive criticisms that have been levelled against me (section 5). This is followed by a brief conclusion (section 6).

## **2. Chang is an anti-econometrics communist who thinks institutions do not matter: misunderstanding my arguments**

Many of the commentators misunderstand at least some of my arguments – sometimes even to the extent of criticizing me for saying the exact opposite of what I say. Let me discuss a few prominent examples and speculate why there are so many big misunderstandings.

### *Institutions do not matter?*

The most flawed but surprisingly frequent misunderstanding of my target article is that I say institutions do not matter for economic growth and development – Peter Boettke and Alexander Fink (2011), Maria Bruower (2011), Christopher Clague (2011), Eelke de Jong (2011), Arielle John and Virgil Storr (2011) and Philip Keefer (2011). For example, John and Storr argue that, in my article, ‘the conclusion that institutions (in particular property, contract and the rule of law) are *unnecessary* for growth has not been convincingly argued [*italics added*]’ (John and Storr, 2011: 5). Similarly, de Jong (2011) thinks that I say that ‘values, institutions, and economic development are unrelated’, while Brouwer says that I ‘[challenge] the view that institutions cause growth [and contend] that causality runs the other way’ (Brouwer, 2011: 2). Boettke and Fink (2011) and Keefer (2011) are so spooked by my apparent claim that institutions do not matter that they go as far as naming their commentaries, respectively, ‘Institutions First’ and ‘Institutions *Really* Don’t Matter for Development?’.

These are gross misunderstandings of my argument. Of course, unlike the above-mentioned commentators, I do *not* believe that institutions are necessarily the most important determinant of economic development. This is a point that Dutt, one of the other commentators, also makes, when he says that it is ‘unclear why institutions should be thought of as fundamental determinants of

growth and development' (Dutt, 2011: 5) when 'institutions are affected by other economic changes' (ibid.). In my target article, I also point out that the dominant discourse on institutions and development has paid insufficient (although not no) attention to the impacts of economic development on institutions, which I argue may well be stronger than the causality in the other direction. On top of that, I argue that the relationships between institutions and economic development are a lot more complex than what the dominant discourse theorizes.

However, it is a mystery to me how all of this can be read as arguing that institutions do not matter. Indeed, had I believed in such an argument, I would not have wasted my time writing many articles discussing the role of institutions in our economic life (e.g., Chang, 2002a; Chang and Evans, 2005), editing a volume on institutions and economic development (Chang, 2007), and, above all, accepting the invitation from the *Journal of Institutional Economics* to write my target article in the first place.

How is this degree of misunderstanding possible? I do not think my exposition was so muddled up as to make people think that I am saying the opposite of what I am saying. It is even less likely that the above commentators have wilfully distorted my argument. I can only surmise that, seeing me criticize their own school of thought, some of the commentators decided that I am their enemy and therefore must be in denial of all the main conclusions of their school.

But why cannot they accept that someone can criticize the dominant institutional discourse – or the New Institutional Economics (NIE), if they will – and still believe that institutions are important determinants of economic development? Why do they believe that being critical of a discourse is the same as denying everything that it says? It is truly sad to see some of the brightest minds in institutional economics spring into knee-jerk reactions of the most unreasonable kind.

### *In praise of central planning?*

Another common misunderstanding is that I am an advocate of central planning, or at least some form of socialism. So, I am lectured that '[t]he failed experiments of socialism and development planning have forcefully demonstrated to us that the path to prosperity is paved by decentralized coordination of individual plans guided by market prices' (Boettke and Fink, 2011: 5) and that the state should 'renounce central planning' and 'abstain from central allocation of production factors' (Brouwer, 2011: 4 and 5). I am also told by Young Bak Choi (2011) that my argument is of the 'ilk' of 'African Socialism, dependency theory, and self-reliance' (Ibid.: 5).

Any fair-minded reader would agree that these are unreasonable misinterpretations of my argument. Where in my article do I advocate central planning or self-reliance? Of course, I say critical things about the free-market approach. I also provide quite a few examples in which state involvement (sometimes the use of state-owned enterprises (SOEs), sometimes interventionist

industrial policy, and sometimes collaboration with the private sector) has led to successful economic outcomes. However, there is a huge difference between doing those things and advocating central planning.

Why are the above-cited commentators misinterpreting my argument so much? Perhaps they are so steeped in linear thinking, which I criticize in my target article, that for them any deviation from the free market is tantamount to central planning. Or, given that the above-cited commentators happen to be most strongly influenced by the Austrian school, they are perhaps fighting yesterday's ideological war against Marxism, under whose shadow the Austrian school has evolved. Perhaps the Austrian economists still see Marxists as their main enemies, which makes them mistakenly think that all their critics are Marxists who support central planning and/or self-reliance. I would never know the exact reason, but this particular misinterpretation is so outlandish I do not know how to respond to it.

### *Rejection of cross-section econometrics?*

It is also claimed by more than one commentator that I 'reject . . . cross-country empirical evidence' (Keefer, 2011: 2) or that I 'suggest that [cross-section] studies have no value' (Kimenyi, 2011: 2). I will discuss the issue of appropriate empirical methodology in greater detail in section 4, but I must make it clear at this point that I do not reject the value of cross-section econometric studies in my target article.

All I say is that there is too much reliance on cross-section econometric evidence and that we need other types of evidence – such as time-series econometrics, historical narratives and comparative historical studies. I do not know how this can be read as saying that I reject cross-section econometrics.

Once again, the widespread prejudice that all critics of orthodox economics reject the very use of econometrics seems to have prevented these commentators from understanding what I think was a clear message in my target article – that cross-section econometric studies need to be complemented by other types of empirical studies. This is very unfortunate, to say the least.

### *Concluding thoughts*

I can discuss in detail some more examples of misunderstanding of my argument and try to make sense of them – for example, Choi's claim that I am 'most emphatic about doing away with [. . .] intellectual property rights (IPRs)' (Choi, 2011: 4) or Boettke and Fink's criticism that I 'assume that third world governments act like effectively constrained first best governments as known from the developed world' (Boettke and Fink, 2011: 4). But I think I have already made my point – many leading researchers have been blinded by what can only be described as their ideological prejudices so much that they have not been able to take a critic's arguments for what they are. When at least half of the commentators in the issue start with such a fundamental misunderstanding

of my arguments, it is difficult to see that there is a sufficient basis for a reasoned debate.

### 3. Barking up the wrong tree?: The (alleged) irrelevance of my criticisms

Having painted me as an extremist – who denies that institutions matter, argues that culture has no relationship with economic development, believes in central planning, advocates North Korean-style self-reliance, would abolish intellectual property rights, naively assumes that real-world states act like Plato’s *Philosopher King*, denies the usefulness of cross-section econometrics, and what have you – many commentators claim that even what they see as the less outrageous of my criticisms of the dominant institutional discourse are, while not incorrect, really irrelevant, because I do not engage with the ‘real thing’.

Let me examine one by one these ‘irrelevance’ arguments, of which I have managed to identify (at least) four (somewhat overlapping) variants.

#### *My criticisms do not apply to the core arguments of the dominant discourse*

A group of commentators has argued that my criticisms only deal with the marginal elements of the dominant institutional discourse. As far as they are concerned, I have not really disproved the core arguments of orthodox institutional economics, so whatever few insignificant victories I have apparently scored against some marginal elements of it (especially against the vulgarized version used by the practitioners – more on this in the section after next) is pyrrhic.

Keefer argues that ‘the most important contributions to the study of institutions and development have nothing to do with’ the issues that I discuss in my target article, such as ‘legal systems, state-owned enterprises, financial regulation and corporate governance to corruption and political systems’ (Keefer, 2011: 3). He then goes on to argue that the orthodox institutional discourse is really only about property rights – and really just about its security, not even about ‘whether private ownership is better than public’ (Ibid.), picking on my criticism of its belief in the superiority of private property. Similarly, Boettke and Fink (2011) claim that I conflate institutions, or what they call ‘the foundational rules of governance, which determine how well protected persons and property are’ (Ibid.: 3), with policies, such as ‘democracy, modern bureaucracy, intellectual property rights, limited liability, bankruptcy laws, banking, central banks, and securities regulations’ (Ibid.).<sup>1</sup> John and Storr also

<sup>1</sup> I actually discuss only some of these in my target article, so I can only assume that Boettke and Fink are referring to, without citing, my discussion in my earlier work (Chang, 2002b, chapter 3), where I discuss all of them. However, I am somewhat mystified by their decision not to mention the judiciary, social welfare institutions and labour regulations, which I also discuss there. Could it be that the first is too close to the heart of their ‘institutions’, which makes it difficult for them to dismiss it as irrelevant, while the latter two are just too much off their political scale?

argue that '[r]ather than focusing on private property, contract enforcement and the rule of law exclusively' (John and Storr, 2011: 2), I mistakenly criticize the orthodox school for advocating other 'non-essential' institutions, such as the financial regulatory system and the corporate governance system.

Now, to begin with, is it really true that the orthodox institutional economics is *only* about the 'fundamental' institutions of property (those that guarantee the security of property rights or, somewhat more broadly, private property, contract enforcement, and the rule of law)? Is it really true that those who work in the tradition have never said anything more than this 'minimalist' version?

This is patently not true. The works by Rafael La Porta *et al.* (1997, 2008) in which corporate governance institutions play the key role, are the best examples of how the literature is not simply about property rights in some abstract and minimalist sense (more on this later).

But, even supposing that Keefer, Boettke and Fink, John and Storr, and others are right in arguing that the orthodox institutional literature is really only about property rights, there are quite a few serious substantive problems with this line of argument.

The first of them is that of definition. If, as Keefer argues, legal systems have nothing to do with the security of property right, what does? When many properties are financial assets, many of which are shares in companies, how can John and Storr say that financial regulation and corporate governance are irrelevant to the discussion of property rights? If we take out all these supposedly irrelevant elements – the legal system, financial regulation, institutions of corporate governance – how much is left of the property rights system in a modern capitalist economy? If we ignore all those things that I (and most other people) think are institutions but that Boettke and Fink insist are *not institutions but policies*, what real-world institutions do we have left to talk about? We are back to the problems involved in defining the property rights system, which I raised in my target article.

Second, it is misleading to argue – as Keefer (2011) and, to a lesser extent, Boettke and Fink (2011) do – that orthodox institutional economics is only about the security, but not the form, of property rights. Most other commentators in this issue working in the tradition of NIE, including John and Storr (2011) cited above, would disagree with this reading of the literature. Keefer cites the works by Daron Acemoglu as examples of how it is the security, rather than the form, of property rights that matters in the NIE (Acemoglu *et al.*, 2001; Acemoglu and Robinson, 2006). However, Acemoglu and his co-authors use indicators like 'risk of expropriation' and 'checks on executive power' in order to measure the quality of institutions. This implies that the most important role of institutions is to prevent political interferences in the exercise of private property rights, which is in turn predicated on the view that private property is superior.

Third, as I argue in my target article, it is not even true that more secure property rights are always better for economic development – Kimenyi (2011)

agrees. Sometimes, the weakening, or even the abolishment, of some property rights is helpful for economic development. If they so strongly believe that providing security of property rights is *the* ultimate function of institutions, would Keefer and others condemn the abolition of slavery in the USA after the Civil War or the land reforms in Japan, Korea and Taiwan after the Second World War? If not, they will have to accept that security of property rights is not always good and discuss which types of private property rights should be protected under which circumstances. Otherwise, talking of security of property rights is either an empty slogan that means all things to all people or an absurd advocacy of all private property rights that happen to exist regardless of their functionalities (not to speak of their justice).

Fourth, even more problematic for orthodox institutional economists is the fact that the weakening, or the destruction, of existing property rights has often been deliberately initiated by what they see as the ultimate bulwark against the encroachment on (private) property rights by the predatory states or populist demands – that is, the judiciary. Legal scholars point out that, contrary to its image – so prominent in orthodox institutional economics – as the dispenser of impersonal judgments based on clearly defined rules (that is, the rule of law), the judiciary often actively reinterprets the existing laws and *de facto* rewrites them, strengthening some property rights over others, which get weakened or even abolished (Upham, 2002; Deakin and Wilkinson, 2005; Michaels, 2009). Interestingly, this practice of rewriting laws by the judiciary is more prevalent in the Anglo-American common law system, which these economists argue to be better at promoting the rule of law. This is because, under the common law system, not only are judges more politically independent but also their decisions more immediately modify the laws, thanks to the greater recognition of case law in the system.<sup>2</sup>

*The dominant discourse – especially in its more recent versions – is a lot more sophisticated than what I portray it to be*

Some of the commentators have criticized me for attacking a ghost from the past, as I fail to recognize the more recent theoretical advances in orthodox institutional economics. Christopher Clague (2011), Jeffrey Nugent (2011) and Mary Shirley (2011) take this line of argument.

It is true that more recently orthodox authors have come to pay attention to some of the substantive issues (e.g., two-way causation between institutions and development) that I raise in my target article more than I give them credit for in that article. However, it is not enough to ‘acknowledge’ a certain point. Words are cheap, so to speak, and the proof is in action, not enough of which we have seen.

<sup>2</sup> However, some advocates of the civil law tradition argue that the civil law system is not necessarily less adaptive than the common law system. See Kerhuel and Fauvarque-Cosson (2009), pp. 821–822.

For example, as Shirley (2011) rightly points out, in his more recent works Douglass North clearly recognizes the two-way causality between institutions and economic development (e.g., North, 2005). However, this is only when he talks in abstract theoretical terms. When it comes to concrete analyses – of the rise of England, the divergence between North America and South America, or the contrasts between the Netherlands and Spain (chapters 9–10), it is the same old story. The initial differences in political and property-rights institutions are seen to have led to long-term divergences in economic performances. There is little actual discussion on how changes in material conditions due to economic development may – or may not – modify institutions. Even less is said on how economic development, through its impacts on institutions as well as on material living conditions, changes individuals and how those ‘new men (and women)’ influence the ways in which institutions subsequently evolve. Without these substantive discussions on the second leg of the causality (economic development to institutions), the recognition of the two-way causality does not add much to our knowledge.

For another example, Clague (2011) and Nugent (2011) argue that my criticisms are unfair because I do not fully take into account the more recent developments in orthodox institutional economics, like game theory, behavioural economics, computerized games and experimental economics.

To an extent, it is a matter of difference in opinion. I cannot claim to be an expert in any of the above-mentioned areas, except in behavioural economics *à la* Herbert Simon, which I consider to be incompatible with orthodox institutional economics, despite the lip service that some of its practitioners give to Simon’s concept of bounded rationality. However, I do not think any development in these fields has been big enough to change my assessment of orthodox institutional economics, although I am perfectly willing to accept that someone else may think differently.

For a concrete example, take the case of experimental economics, whose advance Clague (2011) thinks has significantly advanced our empirical understanding of the role of institutions. I agree that experimental economics has produced some very interesting empirical findings and important lessons for policy fine-tuning, but I wonder how much of these can be said to be real advances in our knowledge in the study of *institutions*. Most of experimental economics is not really about institutions – it is simply trying to identify the impacts of different designs in policies (e.g., microcredit, conditional cash transfer, immunization, fertilizer use, educational schemes) under more control (the works by Abhijit Duflo and Esther Banerjee, cited by Clague (2011), are best examples in this regard). Even when the research concerns institutions, like the caste system in India, it is not clear how many new insights we have gained from experiments by economists. For example, it is interesting to confirm through experiment that caste has a real effect on children’s self-confidence and thus performance in school, but that is what sociologists, anthropologists and indeed many (if not



all) ordinary Indians have been telling us for ages. This result looks original only because orthodox economists have not really thought very much about things like caste.

While admitting that there are legitimate differences in judgments, I think the above examples show that some of the commentators have been unfair in characterizing my arguments as setting up a ‘straw man’. Indeed, the easiest ‘straw man’ argument to make is to accuse the opponent of setting up a straw man.

*I criticize the whole orthodoxy for some questionable claims by an avant garde minority*

Some commentators argue that some of my criticisms, while valid, are not very meaningful because they apply only to minority claims strongly disputed even within the orthodoxy itself. For example, Shirley contends that the ‘legal origins’ argument that I criticize is ‘highly disputed by many new institutional economists’ (Shirley, 2011: 3). Clague (2011) makes an even stronger claim. He argues that my criticisms of the ‘legal origins’ literature, led by La Porta and others, do not make any dent in the orthodoxy because I am taking ‘provocative new claims by leading researchers to be the “dominant discourse”’ (Clague, 2011: 3).

I find these claims extraordinary. First of all, La Porta *et al.*’s claims are not ‘new’ or ‘provocative’. Not only have La Porta *et al.* been publishing along this line for nearly a decade and a half (their seminal articles were published in 1997 and 1998; La Porta *et al.*, 1997, 1998), their argument goes back to the origins of NIE, like the works by Douglass North and Barry Weingast. As for being ‘provocative’, while some people within the orthodoxy may have disputed La Porta *et al.*’s claims, their view that common law-style legal institutions are superior to other types of legal institutions is one of the longest-standing and the most central claims in the orthodox institutional literature.

Moreover, Clague’s characterization makes the ‘legal origins’ argument sound like something that is debated only in the ivory tower, but this cannot be further from the truth. When it comes to applications to the real world, there is no idea in orthodox institutional economics that has been more influential than the ‘legal origins’ argument.

The ‘legal origins’ argument has been the main intellectual influence on the World Bank’s increasingly influential ‘Doing Business’ index. The *Doing Business* reports have become the most circulated of all World Bank series (Michaels, 2009: 772). It has an enormous influence among developing country decision-makers, many of whom want their countries to score high in the index, given the prominence that the index has in the financial media and in donor circles. For example, the Rwandan president went as far as establishing a national Doing Business Unit in 2007 (Ibid.). Indeed, La Porta *et al.* (2008) themselves point out that their ideas, through the index, have ‘encouraged regulatory reforms in dozens of countries’ (Ibid.: 325). By becoming the finance minister of Bulgaria

in July 2009, Simeon Djankov – a frequent co-author of La Porta *et al.* and one of the creators of the *Doing Business* series at the Bank – has come to symbolize the influence that the ‘legal origins’ idea has gained in the real world.

We can also detect the influence of the ‘legal origins’ idea in the Bank’s other performance measurement indicators – such as the Country Policy and Institutional Assessment (CPIA), which is the critical criterion in its aid allocation, or the indexes used by the studies in the *Governance Matters* series, cited in my target article.

To sum up, it is wrong to characterize La Porta *et al.*’s arguments as ‘provocative new claims’ by an *avant garde* minority, not least because it is one of the oldest and most central ideas in orthodox institutional economics, as some of the commentators examined in the previous section also argue (e.g., Boettke and Fink, 2011; Keefer, 2011). Moreover, their ideas have been intellectually and financially backed up by a major international organization, that is, the World Bank, and had far more influences than most ideas cited as great advances in NIE by Clague, Shirley, and others in this issue – such as advances in game theory and in experimental economics. Given this, I can only interpret the position taken by commentators like Shirley and Clague as an attempt to disown what they see as a particularly problematic element of the orthodoxy, in the face of a criticism that they cannot easily dismiss.

*Lost in translation?: My criticisms apply only to the vulgarized version of the orthodoxy used by practitioners*

Some of the commentators claim that my criticisms do not affect the validity of orthodox institutional economics because they apply only to those vulgarized versions that policy practitioners use. So, according to John and Storr, I am ‘challenging the form of [the “institutions matter”] hypothesis embraced by some of the development and foreign aid organizations’ (John and Storr, 2011: 2), so my ‘quarrel is, thus, fundamentally against the practices of economic development not the scholarship regarding economic development’ (Ibid.). For Clague, my criticisms, especially those that relate to what I call the extreme voluntarism of the Global Standards Institutions (GSI) discourse, should be directed not ‘against the academic literature ... but against the pronouncements of international organizations, the media, business groups, and activists promoting one or another cause’, who ‘have the incentive to simplify their messages’ (Clague, 2011: 5). Nugent rejects my criticisms for attacking not the dominant academic discourse but the mistaken practice by ‘some important multilateral donor organizations [that] are sometimes overly zealous in advocating some Washington consensus-type reforms and overly powerful in pushing their adoption by developing countries’ (Nugent, 2011: 5). John Wallis also takes the view that my criticisms only apply to the ‘simple and common-sense version of “good” institutions’ adopted by ‘the policy community’ (Wallis, 2011: 3), although he has a more sophisticated view of the matter than

the others cited above and tries to explain this simplification not by the simple-mindedness of policy-makers but by ‘the absence of an academic consensus on what policies would work, in the sense of being feasible to implement’ (Ibid.).

I agree that some subtle theoretical points in academic discourses can, and do, get ‘lost in translation’ by the practitioners. But, unlike what the above commentators imply, modifications of the theories in the process of ‘translation’ by practitioners are not always simplifications. If the original theories are based on assumptions that patently do not hold in reality or if they assume away some important real-world factors (as they often do), modification of the theory by practitioners may make it *less*, rather than more, simplistic. Even when policies have been drawn up by practitioners without regard to the finer points of economic theory, they may produce superior economic results than can ‘purer’ theories. As I point out in my target article, what Singapore has done is an effrontery to all kinds of economics – Neoclassical, New Institutional, Marxist, you name it – but it has been economically very successful.

Anyway, even if it were true that those ‘vulgarized’ versions of orthodox institutional theory – such as the United States Agency for International Development (USAID)’s ‘rule of law’ project, various indexes generated by the World Bank (the Doing Business index, the CPIA index and the Governance index) or the World Bank’s land titling programmes inspired by Hernando De Soto’s work on property rights – have grossly simplified their theoretical messages, theorists cannot wash their hands of these vulgarized versions so easily. Before the fall of the Berlin wall, many people rightly criticized some Marxists for trying to defend Karl Marx’s theory by claiming that what goes on in ‘actually existing socialist societies’ has nothing to do with Marx’s ‘true’ teachings. What is being done by the above-mentioned commentators in this issue is basically the same as that practice.

Am I being too harsh? Why should scholars be responsible for all of those who are mangling up their ideas and mis-applying them in their names? They probably do not have to be, if the ‘vulgarization’ is conducted by some fringe groups that have little impact on the rest of the world, even though we can still ask them how many of the shortcomings of the vulgarized version are due to inherent problems in the theory and how many are due to the deficiencies of the group that is doing the vulgarization. However, when their ideas are adopted by the World Bank, the International Monetary Fund (IMF), the US Treasury, the USAID, and other similarly powerful organizations that have enormous influences on what happens to the real world, scholars cannot, and should not, absolve themselves that easily. Once these organizations adopt certain ideas, they usually back them up with extensive intellectual proselytizing and/or huge amounts of money, as seen in the example of ‘Doing Business’, discussed above. When they are backed up by such organizations, ideas also gain greater legitimacy among private sector investors, so their impacts can be even bigger than indicated by the money directly spent by the organizations in question. Given this, scholars have a duty to speak

out, if they feel that their ideas are seriously vulgarized by practitioners and are producing the wrong policies.

Despite this, none of the commentators in this issue who condemn the simplification by the practitioners has so far raised issues with the policies and the indexes used by those practitioners in any serious way. This is certainly not because, being isolated in the ivory tower, they could not make their voices heard by the relevant practitioners. Many of the commentators in the issue have had excellent access to, especially, the World Bank. For example, Clague has written with Keefer, one of the other commentators in this issue, who is working for the World Bank. Shirley has also worked for the World Bank, while Wallis has contributed to a major report for the World Bank, co-authored with North and Weingast. Thus, when many of them have had close connections with (and sometimes indeed have been) the practitioners they criticize for vulgarizing their arguments, it is unacceptable that the above-mentioned commentators are trying to save the credibility of orthodox institutional economics by disowning 'simple' and 'overly zealous' practitioners.

#### **4. Ad hockery?: The alleged methodological weakness of my empirical criticisms**

Many of the commentators in the issue argue that my empirical criticisms of the dominant discourse on institutions and development are deeply flawed.

As I say in my target article and as I repeat in section 2 (where I criticize those commentators who mistakenly claim that I reject the value of cross-section econometric studies), the evidence used in the orthodox institutional literature is almost exclusively based on cross-section econometric studies – Ros (2011) agrees. We need other types of empirical evidence – time-series econometrics (where appropriate), historical case studies or comparative historical studies.

But many commentators do not consider it a significant problem, especially when, according to them, the econometric techniques have been advancing (for example, Clague, 2011; Nugent, 2011). Indeed, even Maseland, who is also critical of the excessive reliance on cross-section econometric data, is sceptical about the utility of other types of evidence and asks 'were we to add time-series and better measures, do we have reason to believe insights would change?' (Maseland, 2011: 3)

However, when so much of the evidence that is not based on cross-section econometrics contradicts the results from the cross-section econometric analyses, such evidence at least needs to be examined carefully, before being dismissed as being unlikely to change the empirical conclusions.

More serious is the criticism that I do not provide systematic evidence and make my arguments on the basis of 'selective use of particular cases' (Keefer, 2011: 2), which 'is quite *ad hoc*' (De Jong, 2011: 2).

The first thing I would like to say against this criticism is that my target article is a broad-brushed review article, where there is no scope for providing

detailed evidence, especially what would count as ‘systematic evidence’ for those commentators steeped in orthodox methodology – that is, sophisticated econometrics or randomized controlled experiment.

More importantly, this criticism is based on a fundamental misunderstanding of the utility of using ‘particular cases’. In referring to particular cases, I am *not* trying to ‘prove’ anything in the active sense. We all know that one, or a few, cases, however striking they may be, cannot prove anything in the active sense – especially when there are many factors affecting the outcome (whatever that is), whose effects may (or, rather, are likely to) change over time. However, appropriate examples *can* prove something in the passive sense. One black swan does not prove that 30% of swans are black (of course, I do *not* make that kind of a claim in my target article), but it *does* prove that not all swans are white. So, while the examples of successful SOEs in several countries that I mention in my target article do not prove that SOEs are necessarily better than private enterprises (a claim that I do *not* make), they *do* prove that SOEs can perform well (which *is* what I am trying to show). When there are certain examples that clearly do not fit into the received theory, the defenders of the theory being criticized have a duty to modify their theory, rather than dismissing the critic for failing to provide conclusive proofs for (what they think is) the case that he/she is making.

The typical reaction to the kind of awkward examples that I use in my target article (and indeed elsewhere in my work) is to dismiss them as exceptions that do not disprove the received theory – a practice akin to the drawing of epicycles by Ptolemaic astronomers when confronted with observations that go against their predictions of planetary movements. So, when they are told that the USA was the fastest growing economy in the world despite being the most protectionist throughout most of the 19th and the early 20th centuries, free-market economists typically respond that the USA grew fast despite, not because of, protectionism because it had a huge internal market, abundant natural resources and high-quality immigrants. That may be, but then how do we explain the success of protectionist policies in countries like Japan, which had virtually no natural resources and lots of emigration, or countries like Sweden, Finland, South Korea or Taiwan, which had none of those conditions that are supposed to have countered the ill-effects of protectionism in the USA? Indeed, when the exceptions number over two dozen countries with very different conditions – most of today’s rich countries, including Britain, the supposed home of free trade, used protectionism in their earlier stages of economic development (Chang, 2002b, chapter 2) – this ‘epicycle’ defence simply does not work. The theory has to be modified – in the same way that geo-centrism was replaced by helio-centrism in astronomy.

So, when it comes to empirical evidence, the ball’s in my opponents’ court, so to speak. I know that I cannot prove anything conclusively with the examples I use, but I think they are powerful enough to show that the

orthodox interpretation of the empirical evidence is seriously questionable, if not downright wrong. None of the commentators in the issue has provided a convincing defence in this regard.

## 5. Some substantive issues

Many of the commentators have spent their entire comments making negative cases against me – I make extreme claims, I misrepresent the orthodoxy, my criticisms only affect marginal elements in the orthodoxy (be they the *avant garde* work of La Porta *et al.* or the vulgarized version promoted by practitioners), I denounce econometrics, I do not provide systematic evidence, and so on. As a result, relatively few substantive points have been raised. Let me discuss those few substantive points that have been raised.

### *Is there such a thing as a free market?*

Choi criticizes me for mixing up a free market with ‘a libertine state in which anyone can do anything without any restriction whatsoever. Since such a state does not and cannot exist on a sustained basis and all operational free markets have an assortment of legal restrictions . . . which differ from one country to another, there cannot be a definition of a free market that everyone can agree on. Therefore, Chang triumphantly claims that a free market is impossible’ (Choi, 2011: 2).

This criticism is off the mark. I was not into a petty scoring through showing that my opponent’s ideal (that is, the free market) is unattainable in reality. No theoretical concept, regardless of its theoretical origins, is attainable in its pure state in reality. The point I was trying to make is a different, and in my view a more fundamental, one.

My point is that there is no one correct definition of the free market even in the same society at the same time and therefore that deciding how ‘free’ a particular market is is ultimately a matter of political judgment. Given this, when they say that the freest market is the best for economic development, the orthodox institutionalist economists are, without realizing it, making a political statement. They are, in effect, saying that what *they* believe to be the legitimate boundary of the market is the only correct one, even when there is no apolitical, ‘scientific’ way to judge which is the correct one.

### *Property rights*

In my target article, I raised quite a few substantive issues with the orthodox theories of property rights, and of their role in economic development. Unfortunately, the orthodox commentators have failed to respond to any of them in a substantial way. Many of them have simply chosen to re-assert the importance of property rights – to the extent of claiming that they are the only institutions that deserve to be called institutions (see section 3). This is

quite disappointing, given the centrality that property rights are accorded to in orthodox institutional economics.

To begin with, none of the orthodox commentators in the issue directly acknowledges that there are serious problems with the definition of property rights in the dominant discourse – what is it exactly and what are its components? Insofar as they indirectly engage with this subject, some of the commentators actually make things worse by rejecting even things like the legal system, corporate governance, and financial regulatory regimes and by going for very abstract definitions almost devoid of any content (Boettke and Fink, Keefer, John and Storr, as discussed in section 3). As Dutt remarks, ‘moving from theory to policy regarding institutions, it is not clear what real-world institutions are required. Even if we confine our attention to formal laws, there are many dimensions to property rights’ (Dutt, 2011: 3). And indeed in my target article, I list as many as ten possible components of the property rights system,<sup>3</sup> without pretending to be exhaustive. Despite asserting that the property rights system is so central, the dominant institutional discourse is not even able to tell us what it is made up of.

Then there is the problem of aggregation – how do you add up the components of the property rights system and measure its overall quality? None of the orthodox commentators in the issue seriously engages in this question – except for Kimenyi, who points out, rightly, that ‘micro-institutions cannot just be merely added together to come up with a macro-measure of the character of institutions’ (Kimenyi, 2011: 4). This limitation, however, has not kept most orthodox institutional economists from using quantitative indexes of institutional quality. While I do not argue that we should stop quantifying things until we have perfect theory behind them, as this will make any empirical study impossible, we need at least a degree of humility and acknowledge that conceptualization and the measurement of aggregate concepts, like the property rights system or (even more problematically) institutions, have rather shaky theoretical foundations.

Then there is the issue of different forms of property rights. Against my criticisms that the orthodox discourse has failed to adequately consider all forms of property rights, some commentators argue that the discourse has in fact always dealt with issues like communal property, citing Elinor Ostrom (Nugent, 2011). However, it is news to me that Ostrom has ever belonged to the orthodox institutionalist circle. She is a political scientist who has mostly propagated her ideas through books – that low-grade activity that orthodox economists tend to despise. Most of her journal publications are, naturally, in political science journals and most of the economics journals she has published in were heterodox ones, that is, until she got the Nobel prize. The reactions shown by young

<sup>3</sup> They are land law, urban planning law, zoning law, tax law, inheritance law, contract law, company law, bankruptcy law, intellectual property laws, and customs regarding common property.

USA-based economists in the American Economic Association's job search website, [www.econjobrumors.com](http://www.econjobrumors.com), in the days after the announcement for her Nobel prize are a very good, if absolutely shocking, testament to the contempt in which she is held by most mainstream economists.

Then there is my criticism that orthodox institutional economics unjustifiably privilege private property. Some commentators have tried to evade this issue by arguing that it is the security, rather than the form, of the property rights system that matters (Boettke and Fink, 2011; Keefer, 2011). But, as I have discussed in section 3, this is a doomed attempt because such an 'escape route' makes it impossible for them to deal with my other criticism that orthodox institutional economics mistakenly believes that the maximum possible protection of all existing (private) property rights is the best for economic development.

Finally, unless all of the above-mentioned issues are satisfactorily addressed (if not completely resolved), it is unconvincing to claim that some of my criticisms about the measurement issues are unjustified because 'measures of institutions have been accumulating and improving' (Clague, 2011: 4). If we do not know exactly what we are measuring and if we do not know which forms of what we (think we) are measuring are better, how can we say that the quality of measurement has been improving?

### *Institutional changes*

In my target article, I criticize the dominant institutional discourse for taking 'corner solutions' in its explanation of institutional changes – the boundless optimism of the GSI approach and the fatalism of the climate-culture approach. Not many of the commentators have dealt with these issues at any length. However, insofar as they do, their responses are rather problematic.

My criticism of the GSI discourse – apart from the criticism of its unwarranted institutional isomorphism (see Andrews, 2008 and 2010, for further discussion of this issue) – was that it fails to recognize the costs of institutional changes.

In response, Nugent asserts that the dominant discourse has always 'recognize[d] that institutional change is far from costless, and sometimes feasible only in rather special circumstances' (Nugent, 2011: 4). Clague is more nuanced and admits that there is indeed a problem in this regard with 'the pronouncements of international organizations, the media, business groups, and activists promoting one or another cause' (Clague, 2011: 5) but he also asserts that this does not apply to 'the academic literature, which is generally highly cognizant of the forces causing institutions to persist' (Ibid.).

However, if those academic economists working in the tradition of orthodox institutional economics have been so aware of such costs, why have they so enthusiastically endorsed, or at least implicitly condoned, all those attempts at radical, costly institutional reforms imposed on the former socialist economies and many developing countries in the last two, three decades? Were their endorsements really based on careful cost-benefit analyses of the proposed



institutional changes in the context of the particular country in question? Hand on heart, can they deny that they went along with those reforms because those were trying to implement institutions that they like?

It is a similar story with my criticisms of the fatalism of what I call the climate-culture school, which I criticize for believing that institutions are basically shaped by basically immutable factors, like climate, geography and culture.

Those few commentators who deal with this issue claim that orthodox institutional economists have always been aware of this problem. For example, Clague claims that even the supposed supporters of this view, like Acemoglu *et al.* or Stanley Engerman and Kenneth Sokoloff, ‘do not deny that institutions have changed and are changeable through the actions of political leaders’ (Clague, 2011: 4).

However, once again, words are cheap. Unless orthodox institutional economists provide credible theories that are able to explain institutional changes on the basis of sophisticated understandings of the complex interactions between material conditions, institutions and individuals (both as the carriers of ‘cultural memes’ embodied in institutions and as active agents with ‘free will’), the assertion that they know that ‘history or geography are not destiny’ fundamentally remains no more than a lip service.

## 6. Concluding remarks

As I have shown in this reply, so many of the comments on my target article in this issue are the results of ideology-influenced misunderstandings and attempts to sidestep my criticisms by claiming that they only apply to the peripheral, but not the core, elements of their theories. As a result, much of the debate has become rather stilted, but the debate in this special issue has taught me a lot of things – sometimes by forcing me to sharpen my argument and sometimes opening my eyes to different dimensions of issues that I had thought I understood fully. This would not have been possible without the commentators engaging with me, even when many of them seem to have found it uncomfortable to debate with someone who does not share many of their foundational beliefs. I thank them for that.

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