



Working Papers

**Discontent with the World Bank's Excursion into Economic
Geography: Lions and Butterflies Once More?**

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ABSTRACT

The World Bank's 2009 *Reshaping Economic Geography* has had overwhelmingly negative reviews by geographers. Why are they so unhappy with the Bank's excursion into economic geography? One answer is that they have some valid criticisms. Another explanation, one that we think is important, is that some of their criticisms are not valid. We think that this problem arises because of cross disciplinary problems, filled with —venom and incomprehension. We attempt to put what we think are misunderstandings to the side to clarify the most effective criticisms. If this can be done, perhaps the Bank's foray into economic geography could be more usefully engaged by geographers and others

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I. Introduction.

The World Bank's 2009 *World Development Report: Reshaping Economic Geography (WDR)* has already had six reviews, four by geographers.¹ All of the latter reviews are overwhelmingly negative.² For instance, a review in *Urban Studies* by, Bryceson, Gough, Rigg and Agergaard (2009), hereafter BGRA, essentially savages the (*WDR*) as developing myths which help the Bank to continue to maintain a "lack of

¹ The World Bank's Chief Economist, Justin Lin, in a review of all the *WDRs*, says it... "is one of the ...most widely read vehicles for encapsulating the Bank's knowledge of and policy recommendations on key development issues. The ... earliest *WDRs* ... summarized the Bank's views on national and sectoral development priorities.... Since 1980, [they] have acquired a thematic focus and ... an overview of thinking on specific topics completed with a ... synthesis of practical experience..." in Yusuf et al (2009), p. ix. More than 50,000 executive summaries of the Report are produced in seven languages. Yusuf provides a review of all the *WDRs* seen through the evolving prism of development economics and historical events such as decolonization and the fall of the Soviet Union. His volume also provides commentary by Deaton, Dervis, Easterly, Ito, and Stiglitz.

² Other reviews by Biau (2009), and Montgomery (2009) were largely positive.

accountability to developing country populations...” They argue that the Report’s economic reductionism is irresponsible and often borders on the absurd. Similarly, in the *Journal of Economic Geography*, Scott (2009) calls the report “a signal failure.” Rigg et al (2009) are gentler, concluding that it is merely a “disappointment,” but they also find it to be “academically narrow and historically shallow,” p.130. Finally, Maringanti, Sheppard and Zhang (2009), hereafter MSZ, say it “effectively promotes a checkbox style to development.” In a word, to say the least, geographers do not like the report very much.

Why are geographers so unhappy with the World Bank’s excursion into economic geography? One answer is that they have some significant and valid criticisms.³ Another is that some of their valid criticisms are not that significant, or if significant are so only in a statistical or partial equilibrium sense. Still another explanation, one that we think is important, is that some of their significant criticisms are not valid. We think that this last problem arises because of cross disciplinary problems, many of them shrouded in the same rhetoric Duranton and Rodriguez-Pose (2005) refer to as “When Economists and Geographers Collide: or the Tale of the Lions and the Butterflies.” According to them, limited cross discipline interaction—filled with “venom and incomprehension”—accounts for the persistence of interaction between these disciplines which has been more bitter than satisfactory.

In short, we think that at least some of the reviews of the *WDR* seem to fall into a type of discourse in which selective “butterflies” are reproaching the more concentrated pride approach of economists.⁴ Using Duranton and Rodriguez Pose’s observations as a guide, we attempt to put what we think are rhetorical and empirical misunderstandings to the side, to clarify the most poignant analytical criticisms. If this can be done, perhaps the World Bank’s foray into economic geography – one made almost exclusively by economists -- could be more usefully engaged by geographers and others.

Our review of the geographers’ reviews focuses on four points:

- Significant but what we think are largely invalid criticisms with respect to the World Bank’s institutional intolerance for deviation from a “blind belief in *laissez-faire* markets” BGRA, p. 725 a belief that seeks “ideological hegemony” MSZ, p. 45. The related claim that this perspective is supported by an intentionally inadequate research

³ To a rough approximation, we use the word “significant” in the way one might distinguish between a coefficient with economic, as opposed to just statistical, significance, and “valid” criticisms as those having what we think are statistically but not economically significant effects.

⁴ The Duranton and Rodriguez-Pose article, written by an economist and geographer, respectively, an introduction to a Special Issue of *Environment and Planning A* (2005), discusses how the two disciplines might interact more productively. The metaphor of lions and butterflies refers to views about the way research is carried out in the two fields. Geographers, it suggests, move quickly from topic to topic whereas economists tend to concentrate their work, as might a pride of lions. As we show, the word “pride” can be applied to economists’ arguments in a very different way as well. The first issue of *The Journal of Economic Geography* (2001) also had a similar interchange between economists and geographers in a review of Fujita, Krugman and Venables (1999).

methodology which helps sustain the Bank's position, by ignoring a number of complicating factors thereby bring the Bank's hegemonic knowledge production into question;

- Valid but what we think are often insignificant criticisms of the *WDR*'s simplified approach, one that leaves out many significant issues, thereby eviscerating the advantages that geography offers to the study of development, Riggs et al, p.134-5, and Scott; and
- Valid and significant criticisms. We divide this category of criticisms into two: those that we argue take a limited perspective – one that is either a partial equilibrium perspective or one that might well be statistically but not economically significant -- and those which are more valid and significant criticisms. Both of these criticisms have to do with interpretations of the empirical results or the way they were presented, and they were made by MSZ and BGRA.

In the next sections we discuss each of these types of criticisms. We begin with evidence for the claim as to the World Bank's institutional intolerance. We argue that this criticism would be highly significant if it were valid. And, while we cannot rule out the criticism's validity directly we can show that the evidence adduced to make the criticism is invalid. Then, in section three we focus on the simplifications taken by the Report. These criticisms are valid, but whether or not they are significant depends upon the way the simplifications are used. The penultimate sector reviews two methodological criticisms, both of which seem valid, and one potentially significant. The other appears to be a decidedly partial perspective. A final section concludes.

II. The World Development Report: Does it Seek Ideological Hegemony?

We begin with the most strident criticism of the *WDR*, that it embodies a blind faith in laissez faire markets which causes it to bend its analytical arguments to support of a monolithic political perspective. In some respects, this argument is ironic given the length and emphasis the study gives to policy recommendation which support greater emphasis on market interventions. The Report provides considerable emphasis on spatial targeting of public assistance, infrastructure, and public institutions. These are topics that would generally be beyond the ambit of an aspatial economic perspective, and certainly not the views of those clinging blindly to laissez-faire markets.

MSZ take a more measured if also somewhat ironic criticism of the Bank's approach. In this case, however, the irony has to do with the style rather than the substance of the comment. They say "Increasingly, it appears that the inconsistencies and omissions in the Report are not incidental but effectively constitute a strategy to draw in academics and practitioners into ... a document written by economists who treat politics as an inconvenient reality, and yet it is a thoroughly political document." p. 45 In other words, unlike BGRA, they do not suggest that the problems with the Report are intentional tropes to maintain political hegemony. Rather, they suggest that through its

research capabilities the Bank has been able to replace structural adjustment conditionalities with intellectual conditionalities laid down by knowledge production.

As a result of the potential significance of these criticisms for the ability of the Bank to serve as a development agency we spend a bit more time than might be expected on this topic, examining the evidence adduced by the reviews on the factors that serve as the motivators of Bank behavior. Our point is not to engage in an argument about possible motivations, but rather to show that many of the touchstones for the claims of BGRA are empirically inaccurate. Perhaps by putting some of these criticisms to rest we can contribute to a deeper, and so far largely missing, cross discipline dialogue. Of course, as MSZ, and elsewhere Sheppard (2001) suggest, in the end the achievement of such harmony may not be desirable or achievable because it may just lead to intellectual hegemony replacing policy hegemony.

BGRA cite Angus Deaton and Joseph Stiglitz, to argue that: (1) even though the Bank's studies are sometimes repudiated by Nobel Prize winning former personnel it continues to persist with its latest orthodoxies; and (2) in the Bank's *WDR* efforts, advocacy takes precedence over balanced analysis. MSZ refer to a study by Broad (2006) to suggest that the Bank's incentive structure for researchers could well result in what might be called the "maintenance" of an ideologically motivated research "paradigm."

As evidence of their persistence with policy in the face of credible criticism by Nobel Laureate employees, BGRA refer to Stiglitz, who notoriously and very publically, disagreed with IMF/Bank policies during the crisis in East Asia, Stiglitz, (2003). While his criticisms, in this instance, were largely of specific macroeconomic recommendations, and again largely those of the IMF, they could lead to the charge leveled. It is of course always difficult to infer motives for a particular criticism. And to compound the difficulty, in this case, BGRA's interpretation of the Bank's behavior may be reasonable. See, for example, the discussion in Wade (2003). Nevertheless, since Stiglitz is highlighted as a clear example of a critic of the World Bank's political motivations in its *WDR* research it is worth considering his views in this regard. As he put it somewhat tongue-in-cheek:

[The World Bank's] predominate role in development research is so strong that were it involved in the production of an ordinary commodity, it might be accused of anti-trust violation, dominating an industry. (Stiglitz, 2007, p. 1)

In other words, according to Stiglitz, the Bank's research effort is so strong it effectively dominates the field of development economics. Is it likely that such strong analysts would be dominated by a monolithic perspective? Certainly ideas in development economics, like those in other fields, are periodically dominated by intellectual fads. However, BGRA argue that Stiglitz' criticism of the Bank's research stems from its blind support for particular policy positions rather than its simply being affected by passing fads or ideas. When one notes Stiglitz' effusive self-praise in describing how he focused the 1998-2002 *WDRs* on what he saw as "the" important development issues, one gets the sense that the perspectives taken by at least by the

WDRs under his supervision were based on intellectual rather than political whims. Kemal Dervis' remarks in Yusuf (2009) support this view. He says that the *WDR* is far too frequently in thrall to academic scribblers, too often attempting to target "big" ideas rather than political viewpoints. In this light, Stiglitz' criticism appears to be more a case of an old lion roaring about his views not being given enough attention rather than a concern with a systematic political manipulation.

Such a view does not of course assure that the Bank's research is always accountable to the Bank's clients, the population of developing countries. Nor does it assure that efforts to advocate and defend its policies will always rely upon objective evidence. The accounts of the three year suppression of Nobel Laureate Jan Tinbergen's work in the 1950s imply that such suppression of views can happen (Toye and Toye 2005). Similarly, Broad's (2006) analysis provides an account of how articulating a particular point of view can have highly beneficial consequences for researchers at the Bank.⁵ Nevertheless, in judging criticisms of the Bank's institutional blinders, it is important to make sure that the views of the critics cited are accurately portrayed. From what he says, Stiglitz' views were not.

Nor were the views of the other leading economist they cite, Deaton, accurately depicted. His criticism of Bank research is from a 2006 Bank-supported review he directed to see how its research efforts could be improved. Deaton was critical of the possibility of advocacy in Bank efforts, and made a number of recommendations to help insulate research from such pressures. But he did not, as BGRA claim, "single out annual *WDRs* as a medium through which advocacy of the World Bank's favoured policy recommendations sometimes take precedence over balanced analysis." p. 725. In fact, based on his review, the opposite criticism seems more apposite. He says, "There is much political correctness, including mindless cheerleading for cultural touchstones" and "[The *WDRs*] suffer from always trying to make everyone happy. (Deaton et al, 2006, p. 81-82)" Moreover, his report also notes that "all [*WDRs*] had deeply thoughtful discussions of topics that are not always or widely understood." p.79. In this kind of context, one almost cringes to read BGRA's retort "that the urban sector has been fortuitous to escape *WDR* analytical attention until the present (p. 726)."⁶

In short, the strongest criticisms of the *WDR* based on authoritative sources do not hold up. Nor do the claims about the Bank's attempted repression of sovereignty in sub-Saharan countries. BGRA, for example, say "sub-Saharan African economies have been

⁵ She discusses the career of Bank economist David Dollar whose work was consistent with what she calls the "paradigm maintenance" involved in supporting Bank policies such as those on the advantages of opening up and liberalizing economies. She does not mention that Dollar's work on this topic, while criticized on many grounds, was also published in some of the best economics journals which, one would assume, would be insulated from having to maintain the Bank's policy paradigm. Nor does she mention that Dollar is one of the world's most frequently cited economists having a higher ranking in this regard than a number of Nobel Prize winners. Finally, and perhaps most importantly, she does not discuss the role Dollar's work played in helping to maintain aid levels in an environment in which many donors wanted to cut it.

⁶ Besides being severe it is also inaccurate. *WDRs* in 1978 and 2000 gave considerable emphasis to urbanization issues.

subject to Western international financial institutions ... [with the result that] African sovereignty has yet to be regained.” p.732 This is a damning story and is fundamental to their conclusion that there has been a lack of accountability by the World Bank to “developing world populations.” There is certainly a large literature which indicates that the World Bank supported adjustment programs of the 1980s did not work in Africa, see Easterly for an extensive literature. But, have the flaws in this lending been denied or glossed over by the Bank? Has it attempted to deny the significance of this failing? And does this *WDR* contribute to this denial? Certainly World Bank adjustment lending did not help generate growth, but the Bank has not been shy about discussing mistakes, see World Bank (1993) and Yusuf (2009). In fact, it often highlighted discussions of the failings. Hence, the argument that the Bank has been unaccountable in analyzing and discussing these events is also inaccurate.

Ultimately, it is perhaps not surprising that the Bank’s use of *WDRs* to propagate perspectives on “what works” in a field that has had so many disappointing results, and such weak data, that changing understandings about the development process have frequently arisen.⁷ Certainly, in such an ambitious undertaking, over such a tumultuous time period, there have been criticisms which have had a good deal of validity. However, the specific criticisms as to motivation made with respect to this *WDR* are particularly weak. Perhaps the strongest criticism made against the Report in this regard is that it adopts a seemingly politically agnostic position with regards to spatial decisions when these decisions are often inherently political and based upon fundamentally important distributional issues or matters of justice, Scott (2009) p.3.

Two answers to this criticism are evident: First, and somewhat disingenuously, “the Bank’s charter forbids commentary on the politics of a country,” Yusuf (2009), p. 103. So, Bank reports, such as this one, are compelled to ignore the elephant in the room, by refraining from an explicit discussion of the wide range of extremely important considerations even if those issues are the most basic determinants of why spatial considerations are of such significance to the policy discussion. Second, and more clearly related to the differences between economists and geographers, is a description of the method used by Krugman to develop economic geography.

“He spots an important economic issue... before anyone else. Then he constructs a little model [which] is wonderfully simple. But it leaves out so much, and relies on so many special assumptions ... that [many] don't think it could possibly do justice to the complexity of the issue. Armies of well-trained economists [then] go to work on it, and generalize it.” Dixit (1993) p.173.

⁷ With regard to data development, this *WDR* gave emphasis to developing a new measure of urbanization across countries. Such measurement has long plagued empirical analyses of urbanization because countries have so many definitions of how cities are defined and urbanization measured. This sort of measurement development by the *WDRs* has in many other cases lead to the widespread use of new statistics -- such as the dollar a day measure of poverty, presented in the 1990 *WDR* and the Disability Adjusted Life Years, DALYs, which was presented in the 1993 *WDR*.

In other words, as long as the objective is the modest one of generating much-needed policy discussion, the approach is one that many, perhaps even geographers, might find to be reasonable. However, as the next section suggests, its objectives appear to have gone well beyond this point.

III. Checkbox Development: Simplifications and Elisions.

The criticism that the Report leads to a checkbox form of development stems from two aspects of the work. First, the Report's linking of targets and instruments produces a policy categorization scheme "that fails to provide thoughtful and relevant policies." MSZ, p. 51 which "if taken too far, become absurd abstractions or even dangerous ones if translated into policy." Rigg et al. p. 131. Second, it elides over most of the interesting debates in development geography, as discussed at length in Rigg et al, creating a "straw figure by misrepresenting the position of most policy-makers" by arguing that "policy-makers have long recognized the impossibility of spatially balanced growth." Rigg et al, p. 133.

The criticism of the simplistic discussion of targets and instruments is in many ways valid. Mechanical frameworks, such as the one proposed by the Report, have obvious weaknesses and inconsistencies. Any such reductionist, highly partial scheme will certainly have problems such as those indicated above. Nevertheless, even if the presentation is less than compelling, the point that there are gains from coordinating targets and instruments also has validity. Attempting to achieve more than one goal with each policy instrument has often been followed by policy-makers even if such methods have long been the *bête noire* of economists. Quite simply, economists worry about the reductions in effectiveness associated with a policy that tries to do too many things at once. In other words, both the economic and statistical impacts of a policy instrument that addresses more than one target will almost always be unambiguously weaker.

A central point of the Report is that the implications of spatial economic decision-making matter; it is not just social concerns or the fundamental injustices which may have created spatial disparities that should be considered. Of course these other issues are often paramount, but too often have spatial aspects of economic choices been ignored by economists who, prior to the emergence of economic geography, had nothing tangible to say about decision making with respect to space. At the end of the day, while the categorization scheme is undoubtedly extremely naïve it can also be useful. To take an example, consider policy-makers in a very poor, land-locked African country riddled by ethnic tensions which have dictated a particular allocation of people and resources across space. Suppose these policy-makers are attempting to sort through their policy priorities. The framework presented can at least help them identify which sort of spatial policies are likely to have the greatest effect on economic growth and welfare. It cannot permit them to discern how such spatial policies will affect, for instance, the various injustices generated by ethnic tensions, but it can give a sense of which spatial policies are likely to matter. That is all it can do, and recommendations beyond that can indeed become dangerous as MSZ say.

Having said that economists' views about policy effectiveness have often been ignored, an obvious question is whether the Report creates straw figures of policy-makers as Riggs et al say it does. Have policy-makers in fact long recognized the impossibility of spatially-balanced growth, as Riggs et al say? The most direct way of answering this question is to review the statements of policy-makers themselves. Spence (2009) p. xii, presents evidence from a UN survey which shows that the vast majority of developing country policy-makers would resist rather than welcome urbanization, a view that implies that these policy-makers believe that individual spatial decisions are non-optimal. Hence, if those surveyed by the UN are at all representative, the straw figures do indeed seem to exist.

Finally, MSZ suggest that the Report overlooks the fact that in many places in developing countries decision making is not made in a capitalistic framework, p. 49. Scott carries this view a step further saying that many of the important localized decisions which have significant implications for spatial decisions require collective action or the actions of actors engaged in strategic behavior. It is of course true that many such decisions are not those of decision-makers who would easily be referred to as capitalists. But, is this criticism really about capitalism or the underlying models of decision-making?

The maintained hypothesis of economics is that individuals and households attempt to get the most out of their opportunities. It is not one that subscribes to capitalism or alternative systems. Rather, it is an assumption that in addition to budget constraints economic actors may also be constrained by limited information, by behavioral patterns that are dictated by traditions or by strategic behavior due, for instance, to property rights ambiguities. But these complications have nothing to do with whether the behavior modeled is within a capitalist system. These concerns are clearly very difficult to capture in simple models – indeed, it may be impossible to capture some of them -- but these modeling difficulties have little to do with the behavior of the economic system. It is, in other words, the models of behavior rather than the models of the economic systems that should be the subject of criticism.

In sum, within narrow confines the targets and instruments approach taken by the *WDR* will have a recognizable resonance among economists and their concerns with minimizing the side-effects of interventions into decision-making processes. Outside of those confines, however, it is, as the geographers claim, all too easy to fall into a checkbox development perspective.

IV. Methodological Issues.

We focus on two of the methodological criticisms raised by the reviews. First, African urbanization trends are ignored because the results would contradict the models of the new economic geography. And second, that the experiences of India and China, according to MSZ, belie the arguments made in the Report about the beneficial effects of Special Economic Zones, and overlook the role of “forced,” cheap labour in the development process.

Africa. With regard to Africa's urbanization BGRA note that it has been driven by the "push" from rural areas rather than the "pull" of cities. They call this "African exceptionalism" a view not shared by the *WDR*, see p. 59. This exceptionalism has recently been shown to be the case empirically by Barrios et al (2006), and has long been noted by geographers and historians, such as Davidson (1992) and those cited by him. The *WDR* does not comment on Africa's exceptionalism, preferring to rely on a very limited number of country observations to, as BRGA suggest, argue that Africa's urbanization trends appear to follow typical urbanization patterns. They claim that the *WDR*'s claim of data limitations is simply an excuse that allows it to avoid discussing empirical trends that would contradict the new economic geography.

One may argue about the data weaknesses pointed to by the *WDR*, but one cannot argue that the data were not used because they would contradict the model. Duranton (2009), pps. 79 and 84, has shown that the African trends discussed by BGRA are perfectly consistent with the sort of models typically used in the new economic geography. Thus, the rationale that BGRA impute to the *WDR* motivations is incorrect, even if the point itself raises a potentially fundamental question about urbanization in Africa. As BGRA say, the *WDR*'s focusing on only ten country observations to discuss urbanization trends in Africa is not an appropriate methodology to test the statistical validity of African patterns.

This underlying African exceptionalism would arise, in the language of the new economic geography, because urbanization generates so many spill-over and agglomeration economies and diseconomies which, in turn, have broader welfare effects. When the decision to migrate to a city is motivated by climatic austerity in the countryside, rather than the opportunities presented in the city, agglomeration diseconomies rather than economies are likely to prevail, particularly in countries experiencing negative economic growth. The result, as BGRA suggest, is likely to be concentrations of poverty in the cities, and long term deterioration of basic services in African cities as shown by Annez and Buckley (2009), p. 8.

As BGRA say, a more nuanced policy stance is appropriate in such a distinct environment. The *WDR*'s calls for a new contract for Africa, p. 292 -- one which focuses greater attention on encouraging more regional integration and more porous borders -- to help offset the secular decline in cross-border African migration. This new approach could be an important way to help African cities reach their full potential as drivers of growth. It is also likely to lead to some considerably larger African cities. With a greater emphasis on a regional rather than national perspective there is certainly room for greater attention to be given to the factors -- and particularly the second nature geographical factors -- that motivate this migration, be it within across or within countries.

India and China. MSZ present the Indian and Chinese experiences as demonstrations of two kinds of weaknesses. First, the *WDR* has erased politics from the analysis, and second, the *WDR*'s arguments for the desirability of increased labour mobility and the high costs generated by barriers to allowing the spontaneous patterns of location, p. 45

are dubious. As to the erasure of politics we agree that this was done. However, as to the desirability of labour mobility we believe the criticisms to be partial at best. For example, with regard to India, MSZ bemoan the “commodified” nature of urban land markets and describe the hostility to migrants in cities while conditions in the countryside have progressively deteriorated p. 48. While for China, it argues that “increased labour mobility... is primarily for powerful local politicians and investors.” p. 49. From this discussion one might reasonably expect that these are countries in economic decline rather than two of the most rapidly growing economies that have ever been observed.⁸

Of course, China’s internal passport system results, as they say, in “incomplete urbanization,” but that is a much less restrictive policy than the prior one of central government control over all migration. Similarly, nowhere is it mentioned that in the early 1990s India eliminated the “license raj” which dictated where industries could locate, with a clear mandate to balance growth more equitably. Certainly the large scale movements of people in these two countries have created enormous social disturbances and exacerbated tensions among those affected by such change. Just as certainly, however, it is hardly a coincidence that the much higher growth rates achieved in both of these countries occurred when policy-makers allowed much more freedom in spatial decision making and reduced some of the significant barriers to the mobility of resources across space.

In sum, India and China have not undergone costless migrations of hundreds of millions of people moving from the countryside to the city. That would not be possible. However, when one takes note of the views of the Chinese Central Bank Governor or the Indian Reserve Bank Deputy Governor, in Peirce and Johnson (2008), pps 43-45, it is clear that they think that cumulatively these costs were outweighed by large gains in social welfare brought by urbanization. In other words, these costs presented by MSZ are valid but arguably partial equilibrium results that do not take into account the broader effects unlocked by less controlled urbanization patterns.

IV. Conclusion.

The new economic geography is, as the name implies, a new field with seminal works less than 20 years old. It is a field that allows economists to discuss why a port, for example, might be a desirable investment at a particular place, a topic that could not be analyzed within the spatial maximizing frameworks of neoclassical economics.⁹ Such an approach, in principle, represents a major advance on the sorts of techniques used to explain the urbanization process. It is a field that allows for multiple equilibria, path-dependency, and empirical testing of ideas that were until recent years within the purview solely of geography. In other words, it is a complicated new field that is just beginning to test some of the implications of its models.

⁸ Per capita income in China has grown by 8 percent per year for the past 30 years. India’s has been lower but still remarkable. These long term trends have major implications for almost all social welfare measures, leading to improvements at far higher rates than occurred in earlier periods.

⁹ Sunley (2001) provides an amusing discussion of the port example in his review of Fujita et al.

Moreover, these tests usually involve working in fields that have long been tilled by others who have followed very different methodological approaches. In some ways, then, it is not surprising that there is conflict between these disciplines. Moreover, when one takes note of the perhaps unintentional triumphal titles of some of the economic work in this field – such as Rodrik et al’s (2004) article “Institutions Rule: The Primacy of Institutions Over Geography ...in Economic Development,” or MSZ’s finding that only one citation in the WDR’s 25 page bibliography was written by a geographer, and that one was 40 years old, p. 47, one can see why geographers do not, in the words of BGRA, always share in “euphoria of the disciplinary breakthrough in economics.” p. 724 that is represented by the new economic geography.

Nevertheless, when displeasure is put aside, it is important that the cross discipline criticisms be made on refutable grounds even if at the end of the day it may, as Sheppard (2001) suggests, be impossible for a common ground to emerge on which the two disciplines can interact. When the charges of irresponsibility, lack of accountability, and advocacy are either without substance or matters of opinion, there are venomous, and not likely to lead to conversations across the disciplines. Similarly, many of the criticisms raised by the reviews – the lack of attention to place versus space, the inadequate treatment of Africa’s urbanization, the extreme simplicity of the policy framework, and the many overlooked analyses – are also important to the establishment of a boundary crossing dialogue. Perhaps there is room for compromise on both parts?

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