

Review of the World Bank's Research on Urban Issues

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I. Overview

The central problem with urban research at the World Bank is that urban studies remains an intellectually challenged field, yet despite the weaknesses of the field, the Bank must remain committed to the area. After all, the developing world is increasingly urban and development is often so closely correlated with urbanization that it is impossible to think about growth without also confronting cities. Moreover, the Bank's work on infrastructure often brings it directly in contact with urban policy. Cities usually need more transportation, sewage and water infrastructure than rural areas. This makes the intellectual difficulties of my own field all the more costly for the Bank.

The studies that I review in this are neither unusually good nor unusually bad relative to the standards of urban economics. The three projects that I review span the intellectual space of the field. "Urban Development and Poverty" represents traditional, urban studies type research. "Connecting Cities with Macroeconomic Concerns" borrows more from recent work in urban economics, but then relies on a simple survey that is only partially suited to answering policy questions. "Metamorphosis of Marginality" represents traditional urban ethnography using interviews with *favela* residents. All of these projects are reasonable, but none of them represents large improvements to our understanding of these topics. Even more problematically from the Bank's perspective, none of them really changes our view of appropriate urban policy. This does not suggest that the Bank's urban research is unusually bad, but rather in line with the bulk of urban research worldwide.

The Flagship projects are generally stronger. "Connecting East Asia" is the weakest of this trio. It really is little more than a collection of facts. The other two reports combine facts with new research findings. I was more impressed with the report on infrastructure,

which presents a balanced view of the costs and benefits of privatization. The Flagship project on rural development has plenty of new research, but it then substantially overreaches in its attempt to justify an agriculturally oriented development policy. This may be the right conclusion, but it really isn't justified by the regressions given in the report.

I would like to be able to suggest a magic bullet akin to the randomized trials that have done so much to improve the quality of development research in many other areas. Ten years ago, many outsiders would have had the same relatively negative views of traditional development economics that I have just written about traditional urban research. Development has, however, undergone a massive shift with the rise of randomized experiments. As a result, development has become one of the hottest fields for young graduate students and development research has come to fill the top journals. This is not to belittle the many superb development economists who didn't focus on randomized trials, but this older research style requires more skill and was and is hard to reproduce in a large shop like the World Bank.

A conclusion that might be drawn from this is that urban research needs to focus more on randomized experiments and surely this is true. We would learn much about crime prevention in the *favelas* if at least one of the two competing branches of Rio police could be induced to randomly start community policing, or some other innovation, in particular areas of the city. We would learn more about civic participation and tenure if research was based on the natural experiments created by partial land title programs. Certainly, this is one step forward.

However, it would also be foolish to suggest that urban research is as amenable to simple experiments as work on public health or even education. First, many forms of urban intervention are so large that it is hard to imagine a real experiment being done with them. Second, and even more problematically, spatial equilibrium effects are the core insights of urban economics. This makes program evaluation extraordinarily difficult. If we think that free migration of firms ensures constant profits across space, how are we

going to use a difference-in-difference approach to estimate the impact of a spatially concentrated piece of infrastructure?

As an example, consider the impact of a highway system. This system will surely raise productivity in some places more than others. However, it would be a huge mistake to simply look at wages and infer the effects of this infrastructure. Firms will migrate to the infrastructure so the difference includes the loss elsewhere as well as the gain in the specific area. This will cause the estimated impact of the project to be too high. Workers will migrate and this will cause the estimated impact of the project on wages to be too low. The analysis of natural experiments is not impossible, but it requires much more economic analysis than looking at the reading scores for randomized students who have been given test scores.

Because these equilibrium effects are the heart of urban analysis, there is no simple fix for the Bank's troubled urban research program. Policy evaluation is extremely difficult because it requires both exogenous sources of variation and the lens of economic theory. Moreover, the major universities have generally stopped running serious programs in urban and regional economics. Even those Ph.D. programs that have urban economics generally don't offer course sequences that would train researchers to work in this area. The cream of the urban economics research community is generally more interested in working in the developed world, especially Europe and the United States, and the enormous demand of real estate programs means that promising young researchers with interests in this area are lured into working on housing prices in America instead of urban infrastructure in Brazil.

As a result, I think that the only way forward is for the Bank to renew its commitment to really basic methodological research in this field. Some combination of top researchers in development who are committed to modern experimental techniques must be brought together with the better people in urban economics and representatives of the Bank who can steer this toward a better methodology for evaluating the Bank's core topics.

Improvements in methodology require both the new empirical techniques and urban

theory. Today, there is little dialogue between the researchers who specialize in these areas. Without this dialogue, the Bank's researchers will not have the skills to make real progress. There is no easy fix for the problems of urban research at the Bank. The only way forward is to radically improve the tools available to researchers and this is only likely to happen with a large investment by researchers outside the Bank.

II. Project # 1: Urban Development and Poverty

This project contains a set of papers that are connected only by the fact that they concern development in urbanized areas. The most useful papers were the simplest. For example, the essay on feedback on service provision correctly makes the point that feedback reflects many things (like expectations) other than service quality. Perhaps this is obvious, but it certainly is true. The essay on tenure and community action is interesting, mainly because of the novel measures of community intervention. On the other hand, the papers that try to make more use of fancy techniques are much more problematic. The papers on transport in Indonesia and the impacts of policy intervention on the urban poor are deeply troubled in ways that I will discuss later. It will be an improvement if this group can be oriented around new data collection and away from methodology.

The most clearly useful paper in this selection is the one on community feedback and service quality. Feedback responds to quality but it also responds to neighbors, expectations, etc. This is not surprising, but it is clearly true from the data and a useful input into thinking about how to design community feedback mechanisms. I also thought that there was much that was useful in the paper on the economy of southern Mexico. There is much in that paper that is descriptive and useful. The reader really learns something about the region. The weakness lies in the final regressions, which include a host of endogenous regressors, making the regressions almost impossible to interpret. This problem will return in my discussion of later papers.

I also enjoyed the paper on tenure choice and community participation. My own work in the United States has looked at homeownership and civic participation and found a correlation. Real progress in this area involves moving from correlations to exogenous variation and some work of this nature is being done. This paper does a more standard correlation, but still the results are interesting. I was particularly struck by the use of a hypothetical question rather than questions about ex post participation. I was intrigued by this question, but the paper would have benefited from more analysis of whether there is anything real to this.

I was quite unhappy with both of the more technical papers on transport infrastructure in Indonesia and location and welfare in cities. The transport infrastructure paper does the worst things that we seen in certain urban economics regressions, especially regressing current levels on lagged levels and calling this some sort of evidence for agglomerative linkages. This is silliness on two accounts. First, omitted variables are at least as likely to explain the correlation. Second, once you have controlled for a recent lag, the coefficients on the other parameter need to be interpreted as coefficients in a short-term growth regression, not in a basic location decision. There are interesting things to do on this topic, but this paper suffers from really problematic methods.

The paper on location and welfare in cities is also difficult. There are interesting facts here that could be studied, but this sort of neighborhood choice model is ultimately a little embarrassing. Regressing ethnic composition of neighborhood on ethnicity of resident couldn't be sillier. Again, this team is at its best when it is documenting unvarnished facts. Attempts to be fancy clearly backfire in this case.

As a final note, I quite liked "Economic Geography: Real or Hype," perhaps because I have been making similar points for over a decade. The key here is that differences in productivity over space do reflect selection as much as treatment, and if there is treatment, free migration means that there must be some sort of offsetting compensating differential. For these reasons, I have always thought that changes in quantities of firms

(employees, etc.) are safer measures of urban success than changes in wages alone. Better still, the measures are used together.

III. Project # 2: Connecting Cities with Macroeconomic Concerns

This project has both significant strengths and weaknesses. The core thesis of the project is that cities have a great role to play in macroeconomic development, but that this is hampered when they fail at delivering basic services. The project then attempts to evaluate service provision in five studies. These city-specific surveys are the main contribution of the volume and they are certainly useful to know what annoys small business managers about local service provision. Still, it is hard to know what actual policy question is being answered with this survey. I couldn't figure out if this helped us to direct resources to crime vs. infrastructure, or whether the project was really delivering the information that would be needed to decide whether to spend more on these services. At the end of the day, the project occupies an awkward piece of intellectual real estate. It is neither applied enough to really base a concrete policy, nor scientific enough to be primary research. Perhaps the attempt of the study to focus on so many aspects of urban service provision was an error.

The introductory part of this project does an admirable job surveying recent trends in urban economics. It concludes from this literature that cities are really important to development, but only if they don't screw up basic services. I am sure that this is correct, but it is not obvious that this chapter (or the literature) has really made the case. The evidence that bad urban services derail growth is not made by this survey and it is not obvious that this case is necessary to actually study urban service provision. Is it crucial that clean water contributes to economic growth? Isn't it enough that it saves lives? I think that it would be better served without trying to connect the important topic of urban service provision with larger macro issues.

The actual meat of the essay is a set of surveys in five different cities where they question small business owners about their operations. Some of these questions feel like a

standard victimization survey (how often have you been robbed over the last month), and some ask the business owners to comment on the extent that different factors impact their business. I always think that these surveys are useful—asking people what is actually troubling them surely makes some sense. But I couldn't possibly figure out how to actually infer from this the social costs of different urban service failures. Even the attempts at hard questions (i.e., how much does this add to your costs) were difficult to interpret and accept.

I think in the future that this type of survey should be used at a very preliminary stage to help focus Bank attention on particular aspects of the urban environment. It probably should be tied more tightly to other indicators and then used to direct attention at particular problems like traffic congestion or crime. To actually say anything meaningful about those topics, they can't be embedded in something as broad as this and the research needs to combine information on both costs and benefits of related policies.

IV. Project # 3: Metamorphosis of Marginality

I thought that this was the weakest of the three research projects. Janice Perlman did path-breaking ethnographic research in the 1970s on the *favelas*. The heart of this project is a resurvey of the researcher's original interviewees. While this is an attractive design, there is actually little here that really advances our knowledge of the *favelas*. The main conclusions are that the residents have dispersed somewhat, although there is little attempt to benchmark their mobility. They have become more affluent, although again relative to what? They are afraid of crime, which is unsurprising given how much we know about crime in these areas and there is more inequality. Somewhat sadly, despite my great personal interest in these areas, I didn't feel like I learned very much from this exercise.

I remain a big fan of ethnographic research in the *favelas*. In principle, this type of research could help inform on social dynamics within these areas and give a sense of what is going wrong (and right). However, this research had the feeling of a rock star

who rerecords a former hit without breathing much new life into it. My guess is that the key would have been to have a few very key research questions of obvious policy relevance. Why is crime so high in these areas? Why is education acquisition so low? What is happening in terms of the informal economy and small enterprise start-up? Then the research could have been geared toward answering the questions of interest and the researcher could have engaged in a meaningful academic dialogue with the Bank's more statistical researchers who work on these topics. As it is, it is very hard to see what one learns from this project.

Of these essays, my favorite was the description of what has happened to the poor in the last 30 years. As mentioned above, this has the disadvantage of failure to benchmark, but still there are real facts here that are interesting, if not surprising. I was much less impressed by the other two essays. From myth to reality spends an awful lot of time on personal reminiscences and experiences that seem neither particularly scholarly nor particularly policy-relevant. The myth of marginality revisited seems rooted in the past and again not particularly oriented toward answering policy questions.

I enjoyed reading some of this work, and I remain committed to ethnographic work of this type, but ultimately, I think that the researcher has not succeeded in producing research that furthers either the academic or policy objectives of the Bank.

V. Flagship Project # 1: Connecting East Asia

This flagship project is a high-quality overview of the state of infrastructure development in East Asia. It also includes a number of utterly unobjectionable policy recommendations (e.g., don't be corrupt, promote competition). I suspect that it would have been hard to do better given the extremely wide scope of this project. As an overview this is quite useful, but it is unclear whether this actually teaches us all that much about good policy. The conclusions seem drawn more from sensible economic theory than from anything empirical. I like this approach, and I think that it makes sense

for the Bank to pursue some projects of this nature. However, this shouldn't be confused with research that really can change the way that we pursue development policy.

My favorite parts of this project are the summary statistics about Asian infrastructure. In a sense, this resembles a World Development Report with a focus on East Asian infrastructure. I liked seeing the trends in roads and car ownership. I was less enthusiastic about paeans to Asian inclusivity, but I guess that comes with the territory. My own experience with China suggests less of a peasant's paradise than the authors suggest.

I also liked the attempt to fuse economic, political and social analysis. Without going into too much detail, the authors do a good job of illustrating the many forces that come together to shape infrastructure development. The in-depth analysis of Indonesia is particularly useful and more case studies of this form would have been helpful. Moreover, the lessons drawn from this analysis are quite sensible and accord with at least my prior beliefs.

My only major criticism is that this type of thing should not be seen as a substitute for real research. Most of the conclusions are not tightly tied to any form of evidence. For example, certain types of decentralization are touted. I support this view, but it is not really based on sound empirical work and the claims are unlikely to sway those who don't begin as true believers. This is useful, but it isn't enough to form the basis for infrastructure development policy.

VI. Flagship Project # 2: Reforming Infrastructure

This project is the strongest one that I reviewed. It provides an overview about trends in providing infrastructure, but it actually goes beyond that and tries to really evaluate a large global shift in infrastructure provision. The world has moved from state-owned, vertically integrated infrastructure provision to a more private and decentralized model. Increasingly, infrastructure is provided through a mix of public and private entities. The

authors document this shift, but also do their best to evaluate these changes using reasonable empirical methods. They cite the appropriate academic research. As such, the reader not only learns basic facts, but also is persuaded that we have made progress in the government approach to infrastructure.

At its simplest, the new wave in infrastructure provision is privatization. The authors look at the impacts of privatizing in both the developed and less developed world. They focus on simple comparisons of service quality before and after to understand the impacts of privatization and lead the readers to academic papers that have done more. This work is imperfect but it represents a real attempt to use facts to inform our policy decisions. I felt encouraged by the successes of privatization but also appropriately warned about its shortcomings.

There is also a good section on infrastructure pricing. The authors show the extent that pricing fails to cover costs in many areas and also the extent that subsidies tend to be fairly inequitable. The people who receive the biggest subsidies are not the poorest members of society. This continues to challenge appropriate pricing policy in the developing world and pushes us to think of better solutions (infrastructure vouchers?).

The analysis of regulation is interesting but more theoretical in nature. The authors have drawn deeply from the best academic sources from Stigler to Laffont and they draw quite sensible conclusions. Unfortunately, apart from their cross-national indices of infrastructure regulation, they are able to do less empirically in this area. We have good insights about the impacts of regulation and its design, but much less on what actually happens with different regulations.

The chapter on electricity is particularly informative. This type of detailed industry study offers a great deal and there is much to be said for going deep in one particular area. I found the case studies of California, Poland, Kenya and Zimbabwe modest but quite interesting and informative. This is a complicated commodity that this report teaches us much about.

The section on water supply was also extremely good. This is clearly an ongoing issue with huge scope for government involvement. Lives depend on it. The section on transportation was the weakest in the report. It was never all that clear to me why large-scale government involvement was necessary to begin with and I had trouble squaring the report with what I thought that I knew about this sector. Still, despite the weaknesses, this report was generally quite strong.

VII. Flagship Project # 3: Beyond the City

Like the flagship project discussed immediately above, this report combines basic facts about rural development in the Western Hemisphere with original research. The research is generally at a reasonably high level, but the report suffers from a remarkable tendency to overreach. The conclusion that there should be a pro-rural bias in public policy strikes me as both wrong and certainly not justified by the regressions shown. The report reads as if the authors saw themselves as cheerleaders for rural development who were determined to find some support for this view.

The key justification for their pro-agriculture conclusions is Granger causality tests where improvements in agricultural productivity precede improvements in GDP. There are two reasons to be skeptical of this evidence. First, Granger causality tests are notably imperfect tests of actual causality. Just because agricultural improvements appear to slightly precede, there is little real evidence on what is causing what. One could easily believe that improvements in government policy cause both agricultural and non-agricultural productivity to increase, but agriculture is simpler and responds more quickly. This is not the same thing as a real natural experiment that could enable us to draw these conclusions. Second, there are many other reasons to be skeptical of the view that agriculture is the future of the developing world. After all, many poor places are poor because they lack agricultural endowments.

As such, I would have liked to see this enthusiastic report tempered with a more serious industry analysis that asks whether there are sectors in these countries that have an international comparative advantage at agriculture. Taking into account transportation costs, how much potential is there really there for growth? Certainly, improvements can be made, but outside of Argentina and possibly Brazil and Chile, I am skeptical that any Latin American country is likely to earn enough from agricultural exports to fund a transition to wealth. It would have been better if this study were more firmly grounded in the realities of this industry.

I was also struck by the strong negative correlations between the size of the agricultural sector and ex post growth. While I generally wouldn't draw too much from these regressions, they seem as valid as the Granger causality experiments and yield a diametrically opposite conclusion: too much concentration on agriculture is likely to impede growth. My own guess is that sectoral and spatial neutrality is probably the soundest basis for policy and little in this report compellingly refutes that view.

I found some of the environmental results to be interesting, but these results are focused on only one side of the environmental Kuznets curve. At low levels, industrialization leads to environmental degradation. At high levels, more development improves the environment. In fact, in many developing countries, like India, we have already seen an increase in forestation associated with further development. Over time, keeping countries rural may hurt rather than help the environment.

My favorite part of the report was the analysis of education. Certainly, the authors are right to follow Griliches and suggest the importance of education in productivity in both the rural and urban sectors. They should worry a little bit more about interpreting the connection between education and jobs growth. Much of this is surely migration to high education areas, which is a shift in productivity over space, not necessarily an overall increase in productivity. Still, I thought that they were right to highlight schooling.

Finally, many of their policy recommendations (apart from advocating a pro-rural bias) seemed quite sound. It is hard not to be enthusiastic about improvements in property rights definitions. The analysis of tariffs seemed sensible. There is much to like in the end section of this report.

Sheet 1: Urban Development and Poverty

S	Superior
AA	Above Average
A	Average
BA	Below Average
U	Unacceptable

	S	AA	A	BA	U
<i>Please rate the following aspects of the project outcomes (or in the case of ongoing projects, design and intermediate outputs)</i>					
Topics					
Importance of the issues addressed	X				
Clarity of the project focus and stated objectives			X		
Analysis					
Theoretical/conceptual framework			X		
Empirical application			X		
Statistical and econometric methods		X			
Use of existing knowledge and resources		X			
Data					
Awareness of other data sources		X			
Data compilation, cleaning and cataloging		X			
Survey design and sampling		X			
Output Quality					
Writing quality (clarity, organization, etc.)		X			
Clarity of conclusions and recommendations			X		
Extent to which conclusions are based on analytic evidence		X			
Appropriateness of the recommendations		X			

Appropriateness of output form (working paper, book, database, etc.) for intended audiences		X			
Availability of translated outputs where appropriate		X			
Extent to which research:					
Increases knowledge and understanding of the issues			X		
Provides a sound basis for policy		X			
Actual or likely impact of research on:					
Government policy		X			
Future analysis		X			
The development community in general		X			
Overall Quality of Research		X			

Sheet 2: “Connecting Cities with Macroeconomic Concerns”

S	Superior
AA	Above Average
A	Average
BA	Below Average
U	Unacceptable

	S	AA	A	BA	U
<i>Please rate the following aspects of the project outcomes (or in the case of ongoing projects, design and intermediate outputs)</i>					
Topics					
Importance of the issues addressed	X				
Clarity of the project focus and stated objectives			X		
Analysis					
Theoretical/conceptual framework		X			
Empirical application			X		
Statistical and econometric methods			X		
Use of existing knowledge and resources		X			
Data					
Awareness of other data sources		X			
Data compilation, cleaning and cataloging		X			
Survey design and sampling		X			
Output Quality					
Writing quality (clarity, organization, etc.)		X			

Clarity of conclusions and recommendations			X		
Extent to which conclusions are based on analytic evidence			X		
Appropriateness of the recommendations		X			
Appropriateness of output form (working paper, book, database, etc.) for intended audiences		X			
Availability of translated outputs where appropriate		X			
Extent to which research:					
Increases knowledge and understanding of the issues		X			
Provides a sound basis for policy		X			
Actual or likely impact of research on:					
Government policy		X			
Future analysis			X		
The development community in general		X			
Overall Quality of Research		X			

Sheet 3: Metamorphosis of Marginality

S	Superior
AA	Above Average
A	Average
BA	Below Average
U	Unacceptable

	S	AA	A	BA	U
<i>Please rate the following aspects of the project outcomes (or in the case of ongoing projects, design and intermediate outputs)</i>					
Topics					
Importance of the issues addressed		X			
Clarity of the project focus and stated objectives				X	
Analysis					
Theoretical/conceptual framework				X	
Empirical application			X		
Statistical and econometric methods				X	
Use of existing knowledge and resources			X		
Data					
Awareness of other data sources			X		
Data compilation, cleaning and cataloging			X		
Survey design and sampling		X			

Output Quality					
Writing quality (clarity, organization, etc.)		X			
Clarity of conclusions and recommendations			X		
Extent to which conclusions are based on analytic evidence			X		
Appropriateness of the recommendations			X		
Appropriateness of output form (working paper, book, database, etc.) for intended audiences			X		
Availability of translated outputs where appropriate			X		
Extent to which research:					
Increases knowledge and understanding of the issues				X	
Provides a sound basis for policy				X	
Actual or likely impact of research on:					
Government policy				X	
Future analysis			X		
The development community in general				X	
Overall Quality of Research			X		

Sheet 4 (Flagship): Connecting East Asia

S	Superior
AA	Above Average
A	Average
BA	Below Average
U	Unacceptable

	S	AA	A	BA	U
<i>Please rate the following aspects of the project outcomes (or in the case of ongoing projects, design and intermediate outputs)</i>					
Topics					
Importance of the issues addressed	X				
Output Quality					
Writing quality (clarity, organization, etc.)	X				
Clarity of conclusions and recommendations	X				
Extent to which conclusions are based on analytic evidence			X		
Appropriateness of the recommendations		X			
Appropriateness of output form (working paper, book, database, etc.) for intended audiences		X			

Extent to which research:					
Increases knowledge and understanding of the issues		X			
Provides a sound basis for policy		X			
Actual or likely impact of research on:					
Government policy		X			
Future analysis			X		
The development community in general			X		
Overall Quality of Research		X			

Sheet 4 (Flagship): Reforming Infrastructure

S	Superior
AA	Above Average
A	Average
BA	Below Average
U	Unacceptable

	S	AA	A	BA	U
<i>Please rate the following aspects of the project outcomes (or in the case of ongoing projects, design and intermediate outputs)</i>					
Topics					
Importance of the issues addressed	X				
Output Quality					
Writing quality (clarity, organization, etc.)		X			
Clarity of conclusions and recommendations	X				
Extent to which conclusions are based on analytic evidence	X				
Appropriateness of the recommendations	X				
Appropriateness of output form (working paper, book, database, etc.) for intended audiences		X			
Extent to which research:					
Increases knowledge and understanding of the issues	X				
Provides a sound basis for policy	X				
Actual or likely impact of research on:					
Government policy	X				
Future analysis		X			
The development community in general		X			

Overall Quality of Research	X				
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Annex 3: Flagship Research Quality Indicator Form

S	Superior
AA	Above Average
A	Average
BA	Below Average
U	Unacceptable

	S	AA	A	BA	U
<i>Please rate the following aspects of the project outcomes (or in the case of ongoing projects, design and intermediate outputs)</i>					
Topics					
Importance of the issues addressed	X				
Output Quality					
Writing quality (clarity, organization, etc.)	X				
Clarity of conclusions and recommendations	X				
Extent to which conclusions are based on analytic evidence		X			
Appropriateness of the recommendations			X		
Appropriateness of output form (working paper, book, database, etc.) for intended audiences		X			
Extent to which research:					
Increases knowledge and understanding of the issues		X			
Provides a sound basis for policy		X			
Actual or likely impact of research on:					
Government policy			X		
Future analysis			X		
The development community in general			X		
Overall Quality of Research		X			