Governments and donors have learned many key lessons about infrastructure and development over the past 15 years. Published and unpublished research over this period yields a snapshot of the main dimensions of the sector and examines the linkages between infrastructure and growth, the relevance of infrastructure reform for the poor, the fiscal cost of the sector, the potential for a private sector role, and corruption.

Infrastructure seems to be returning to the agenda of development economists. This follows the recent return of infrastructure to the top of the agenda for many governments and donors. After roughly 10 years of a relatively modest profile, this “born again” policy and academic interest in infrastructure should not really be a surprise. Access to affordable infrastructure services continues to be rationed for a large share of the poorest populations of the world. In Africa, for instance, increases in water and electricity services barely managed to catch up with population growth during the 1990s; coverage is still the lowest in the world, particularly for the poorest income classes.

Growth is so obviously constrained by infrastructure bottlenecks in a wide range of developing countries that governments are not really surprised when investment climate surveys tell them that they should rank infrastructure as a top priority. According to the World Bank, the poorest countries need to spend about 9 percent of their GDP on operation, maintenance, and expansion of their infrastructure if they are to reach the Millennium Development Goals (MDGs). They are now probably spending about half that, although it is difficult to know, as no one is measuring the allocation of public resources to the various sectors properly.
While governments and donors are uncertain about specific investment needs, they have learned many lessons over the past 15 years about what matters for the operation, regulation, financing, and political management of infrastructure services. In addition to the specific lessons discussed below, the most dramatic lesson the international infrastructure community may have learned is humility. This humility reflects the limits to its knowledge on a wide variety of issues relevant to policy making in infrastructure.

There is, for instance, still great uncertainty over how, and how much, infrastructure affects growth. Does it work in the same way at all stages of development, for all regions in the same country, for rural and urban areas? There is probably even more uncertainty on many fiscal dimensions relevant to the sector. How and how much did reforms change the fiscal cost of infrastructure in the aggregate and across government levels? Are standard fiscal rules used to assess debt sustainability penalizing the sector unnecessarily? Can the public sector deliver services without the private sector? There are also many issues related to the weak understanding of the relevance of institutional reforms. For instance, are regulatory agencies really needed? If so, do they need to be independent? Can reform be built on institutional models mixing Anglo-Saxon and other legal traditions? What kind of institutional reforms are needed to improve accountability and eradicate corruption in this sector? Finally, there is still much that is not known about the infrastructure-poverty nexus, because good baselines are lacking on how much the poor actually spend and consume on infrastructure services.

This article sums up the key lessons learned on these issues in the past 15 years. It relies largely on recently published research but also on some current unpublished research that is already making significant contributions to the way policy makers are dealing with infrastructure. It provides a rough blueprint for a research agenda on infrastructure, as one of the main overarching lessons is that the knowledge gap is great.

The article is organized as follows. The next section provides a snapshot of the main dimensions of the sector. The second section examines the linkages between infrastructure and growth. The third section reviews the evidence on the relevance of infrastructure reform for the poor. The fourth section summarizes the main debates on the fiscal cost of the sector and on the potential for a private sector role in the sector. The fifth section examines corruption. Some concluding comments are presented in the last section.

The State of the Infrastructure Sectors

A large proportion of infrastructure services are still delivered mainly by the state in many countries. The relative importance of the private sector has increased significantly in some sectors, but the public sector continues to finance and often also deliver many services. Many governments faced with fiscal constraints did try to cut their financing role in the sector during much of the 1990s, but not many really succeeded in attracting private capital; where they did succeed, the record has been mixed.
A recent survey reveals the extent to which the large-scale private sector is making a significant contribution to the financing of various infrastructure subsectors in developing countries (table 1). As expected, the involvement of the private sector is greatest in the highest-income countries. Less expected is the fact that the presence of the private sector is much less widespread than sometimes argued. Only about a third of developing countries can count on private sector operators for the delivery of electricity, water, or railway services. The largest private sector presence is in the fixed line telecommunication sector, where about 60 percent of countries rely on private operators. Overall, the private sector has provided roughly 20–25 percent of the investment realized in developing countries on average over the past 15 years or so. In Africa it has probably contributed less than 10 percent of the needs.

To many countries, in particular the poorest countries, this low participation by the private sector in the operation and financing of key infrastructure investments has been a significant disappointment. Many of these countries followed the prescriptions of “specialists” in order to attract the private sector. They unbundled their services, introduced competition where they could (in and for the market), and created independent regulatory agencies (table 2).

But, as table 2 shows, having an independent regulatory agency, one of the main policy recommendations in infrastructure of the past 10–15 years, does not guarantee private participation. Indeed, there are more countries with such agencies than countries with private participation in electricity distribution. Conversely, a country does not need an agency to attract the private sector: there are more countries with

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**Table 1. Percentage of Countries with Large-Scale Private Investment in Infrastructure, by Sector, 2004**

<table>
<thead>
<tr>
<th>Country income level</th>
<th>Electricity generation</th>
<th>Electricity distribution</th>
<th>Water and sanitation</th>
<th>Railways</th>
<th>Fixed-line telecommunications</th>
</tr>
</thead>
<tbody>
<tr>
<td>Low</td>
<td>41</td>
<td>29</td>
<td>18</td>
<td>34</td>
<td>50</td>
</tr>
<tr>
<td>Lower-middle</td>
<td>48</td>
<td>37</td>
<td>50</td>
<td>26</td>
<td>62</td>
</tr>
<tr>
<td>Upper-middle</td>
<td>58</td>
<td>48</td>
<td>47</td>
<td>60</td>
<td>72</td>
</tr>
<tr>
<td>Developing</td>
<td>47</td>
<td>36</td>
<td>35</td>
<td>36</td>
<td>59</td>
</tr>
</tbody>
</table>

Note: Data for railways are from 2002.

**Table 2. Percentage of Countries with Independent Regulatory Agencies, by Sector, 2004**

<table>
<thead>
<tr>
<th>Country income level</th>
<th>Electricity sanitation</th>
<th>Water and sanitation</th>
<th>Railways</th>
<th>Fixed-line telecommunications</th>
</tr>
</thead>
<tbody>
<tr>
<td>Low</td>
<td>38</td>
<td>13</td>
<td>2</td>
<td>69</td>
</tr>
<tr>
<td>Lower-middle</td>
<td>63</td>
<td>32</td>
<td>8</td>
<td>60</td>
</tr>
<tr>
<td>Upper-middle</td>
<td>63</td>
<td>28</td>
<td>19</td>
<td>71</td>
</tr>
<tr>
<td>Developing</td>
<td>51</td>
<td>22</td>
<td>8</td>
<td>66</td>
</tr>
</tbody>
</table>

Note: Data for railways are from 2002.
private participation in water or rail than countries with independent regulators in those sectors.

The apparent paradox between tables 1 and 2 is not real. The participation of the private sector depends on many more dimensions than the risks associated with not having an independent regulator or having a bad regulator. Exchange rate risks, commercial risks, and political instability can just as damaging. These risks are typically accounted for in estimates of the minimum rate of return that private operators want from a deal in a given country. Ignoring for now the strategic motivations that may lead an operator to enter a country even if the returns on a specific transaction are not high, it is usually believed that estimates of the cost of capital associated with a transaction can be a good approximation of the expected minimum return.

Several recent articles estimate this cost of capital. They show why there is less private capital in lower-income countries. Indeed, these estimates suggest that the returns required to start a project in lower-income countries have to be at least 2–3 percentage points higher than in richer developing countries and more than twice what is generally expected in developed countries in infrastructure activities. The average rate of return has actually often been below this cost of capital, in particular in Eastern Europe and in Latin America, where the public sector is coming back strongly to operate utilities. These numbers imply that, all nonfinancial conditions being equal, the average tariff necessary to generate the minimum required rate of return in the poorest developing countries has to be higher than elsewhere, because it needs to cover a higher cost of capital. This is politically a very difficult position to hold, and fewer and fewer private operators are willing to do so, in particular in politically sensitive sectors, such as water and passenger transport.

Reform experiences have provided insights into what needs to be better analyzed by researchers and where policy makers need to focus their attention. The lessons are, however, quite diverse across regions. Latin America has probably been the most effective in showing that infrastructure matters to growth, with much evidence on the cost to growth of the slowdown of investment in infrastructure in the region (see Easterly and Serven 2003, for instance). There is also a growing body of evidence on infrastructure as an explanation of the gap between rich and poor regions within large countries. Latin America’s recent experience has shown the need to study the social and political risks better, because they matter to the effectiveness of reforms and hence their sustainability. Reforming by decree without an effort to build up support is no longer an option. In this region, the poor have very clearly voiced their view on what kind of infrastructure services they want. Very often this implies that policy makers have to understand how to better balance the concern for equity with the need for incentives to invest that has dominated the past 15 years of reforms. The experience also shows that it is worth looking more carefully into the options to ensure the commitment of government and operators to increased accountability to users and taxpayers. This can be achieved by adopting regulatory models that allow transparent documentation of efficiency, equity, and fiscal considerations.

The experience of the past 15 years has also shown that the international community does not yet know how to address risk effectively. East Asia may have been the most effective in revealing that foreign exchange risk matters to infrastructure
financing. The first generation of public-private partnerships in East Asia was hit hard by the 1997 crisis. Ten years later, these partnerships have not yet fully recovered, except in China. Experience in Eastern Europe and Africa has shown that there is still a long way to go to understand how institutional reforms work in this sector. It may be that reforms have to be introduced slowly. Better documentation is needed of just how counterproductive trying to force institutional changes without taking the time to build the institutional capacity consistent with the desired reforms can be. In francophone Africa, for example, it has been difficult to implement concession contracts, which are derived from the Anglo-Saxon legal tradition. The importance of this risk in Africa has been less well studied than the intensity and the drivers of renegotiation (see Guasch 2004 for an overview of the issues in Latin America).

Experience also shows that politics matter. Anecdotal evidence from Asia, Eastern Europe, and Latin America suggests that politicians are unlikely to give up control of a sector that buys votes in democratic societies. Moreover, in societies in which corruption is rampant, they will not give up control of a sector involving large amounts of money and in which contract award processes often provide opportunities for unchecked transactions.

Finally, there is a widespread sense among specialists that economic regulation and regulatory processes need to be taken much more seriously by actors concerned with corruption. There is, however, little evidence of the direct link between corruption and lax regulation. As discussed below, evidence is starting to emerge, but it is still too modest to validate the intuition of regulatory experts. Regulation will probably never really be independent, but much more transparency and accountability is achievable. It is worth testing more formally whether doing so is worth the effort and time demanded from governments busy with many other components of the reform agenda.

Improving accountability of regulators and operators will have to start by improving the way the various dimensions of the sector are measured. The international community, for instance, has very little knowledge of access rates, affordability, quality, or fiscal cost of the sector. Most of the information necessary to ensure a minimum level of accountability from government, donors, and operators is either estimated very roughly and very occasionally or never collected at all.

Before turning to what is known and what needs to be known on each issue, it may be useful to conclude this section with a quantitative snapshot of the sector (table 3). Consider this snapshot to be a baseline from which progress from reform is measured. The most fundamental performance baseline may be the share of the population with access to basic infrastructure services. The table points to a major gap between the lowest-income countries and the middle-income countries and suggests that there is a long way to go for the development community.

Table 3 hides one of the most unfortunate dimensions of coverage gaps—the extent of hardship endured by the poorest people. Briceño and Klytchnikova (2006) show that across country groups, the poorest 20 percent are significantly poorer than the richest 20 percent, and the gap is largest for the lowest income level (table 4).
Commitments to improve access rates in water and to some extent in telecommunication have been picked by the MDGs. The commitments to electrification have been added as part of the Johannesburg Declaration. There is no similar commitment for the transport sector, and very little information is available on what could be a reasonable baseline to assess the performance of the sector. Road density in the poorest developing countries is about a third what it is in the richest developing countries and about a sixth what it is in developed countries (Estache and Goicoechea 2005b). Most of the evidence on the relevance of the transport sector stems from macroeconomic work looking into the engine of growth, discussed in the next section.

An ideal baseline to track progress quantitatively would include information on affordability and the quality of services. This information is not available for a large majority of developing countries. Most of the related information published is anecdotal, and cross-country comparisons are often not meaningful, because definitions for quality standards and service pricing practices vary significantly across countries.

An ideal baseline would also include information on the cost of the sector. This is particularly important in view of the size of the projects and the importance of the associated financial transactions. Comparable cost data in infrastructure are largely unknown in this sector in developing countries, despite a standard policy message that aims at telling policy makers to improve cost recovery because it costs too much to the taxpayers. In fact, the cost of this sector to taxpayers is not known.

### Table 3. Access to Utilities Services, by Sector

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Low</td>
<td>31</td>
<td>76</td>
<td>65</td>
<td>41</td>
</tr>
<tr>
<td>Lower-middle</td>
<td>82</td>
<td>319</td>
<td>85</td>
<td>72</td>
</tr>
<tr>
<td>Upper-middle</td>
<td>87</td>
<td>672</td>
<td>93</td>
<td>86</td>
</tr>
<tr>
<td>Developing</td>
<td>58</td>
<td>290</td>
<td>77</td>
<td>59</td>
</tr>
</tbody>
</table>


### Table 4. Access to Basic Infrastructure Services by Richest and Poorest 20 Percent of the Population

(percentage of population receiving services)

<table>
<thead>
<tr>
<th>Country income level</th>
<th>Electricity</th>
<th>Water</th>
<th>Sanitation</th>
<th>Telephone</th>
</tr>
</thead>
<tbody>
<tr>
<td>Low</td>
<td>9.7</td>
<td>68.7</td>
<td>41.1</td>
<td>78.5</td>
</tr>
<tr>
<td>Lower-middle</td>
<td>79.5</td>
<td>99.3</td>
<td>64.5</td>
<td>86.6</td>
</tr>
<tr>
<td>Upper-middle</td>
<td>81.4</td>
<td>99.5</td>
<td>76.7</td>
<td>95</td>
</tr>
</tbody>
</table>


Note: Data are the most recent available for 2000-04.
The IMF’s Government Financial Statistics, the standard reference on public finance data, report very little information relevant to assessing the performance of the infrastructure sector.\(^\text{14}\)

The experience of the past 15 years has shown that much work remains to be done to ensure that the lessons of experience are properly internalized in the advice given to policy makers in infrastructure. This experience has also shown that the international community needs to assess performance better, in order to both measure the degree of effectiveness of reform and to ensure improved accountability by all actors—governments, operators, bankers and other financial actors, users, and donors.

**What Effect Does Infrastructure Have on Growth?**

Since the late 1980s, more than 200 published articles in English, French, and Spanish—and at least as many unpublished ones—have analyzed the macroeconomic effects of infrastructure. This is probably still the most widely covered theme in the economic literature on infrastructure, as well as the best known outside the infrastructure community. This literature boom has been the result mainly of conceptual and technical developments associated with new growth theory and associated discussions of regional policies (see de la Fuente \([2002]\) for a survey that includes a discussion of infrastructure).

Among the most useful outcomes of this literature has been a debate on the importance of infrastructure spending at different stages of development. The main message from this literature seems to be that how much infrastructure spending matters is an empirical matter and that infrastructure matters more in low-income countries or regions than in richer countries or regions.

The most common way of quantitatively assessing the relevance of infrastructure spending is to estimate social economic rates of return of past and new investments using a production function. Most of the academic literature estimates these returns using macroeconomic growth regressions. These are usually calculated using data for a specific country or group of countries over several years. In recent years these methods have suggested economic returns on investment projects averaging 30–40 percent for telecommunications, more than 40 percent for electricity generation, and more than 200 percent for roads (when the outliers are excluded, the average is about 80 percent for roads). Returns tend to be higher in low-income than in middle-income countries (Canning and Bennathan \([2000]\); Briceño Estache, and Shafik \([2004]\)).

New growth theory has also analyzed factors leading to the convergence—and disparities—of growth rates between poor and rich regions within and across countries. This research has generated comparative rankings of sectors across regions in the same country, showing that one size does not fit all when it comes to assessing a country’s public investment needs.\(^\text{15}\)

Many insights emerged from the new economic geography theory, which boomed following the seminal contribution by Krugman (1991). Its basic concern is how firms decide on the locations for their production. The main tradeoff—between market
proximity and production concentration—is driven by transportation costs and economies of scale in production. In connection with this theory, there is also a (re)-emergence of the concept of territorial planning, with a view to direct expenditures on the basis of territorial priorities instead of sectoral priorities. But many other factors are also relevant, causing this literature to develop at a very rapid pace. The upshot at this stage may be that while theory points to a significant potential role for infrastructure, much more empirical evidence is needed.

Much of the literature on the importance of growth discusses whether priority should be given to rural or urban development. This may be the most important area of research in the future. Because rural poor people live in relatively low-density areas and rely heavily on natural resource–based production, their demands for infrastructure are different from those of the urban poor. Both groups tend to have limited access to public infrastructure and services, but the constraints on physical access to job and product markets are greater concerns for the rural poor (see the case studies by Fan and his various coauthors on China, Thailand, and Uganda; see van de Walle and Cratty [2004] for recent detailed studies). Recent household surveys seem to indicate that the urban poor are often unable to afford the infrastructure services provided by utilities. Access by the rural poor to network utilities is less important, because many prefer more cost-effective local solutions for their needs, such as solar energy, water pumps, and satellite-based telecommunications. In the context of urbanization, the growth of large cities is fast becoming a major source of demand for additional infrastructure, creating a sense of urgency in some policy circles. But this sense of urgency is criticized by some academics as favoring an excessive urban concentration (Henderson 2002). There is indeed a hot debate as to whether new infrastructure, particularly in interregional transport, also creates additional incentives for rural-urban migration. The debate is as hot as the evidence provided by both sides of the debate is scarce, with most of the discussion based on descriptive case studies rather than technical analysis.

How Has Infrastructure Reform Affected the Poor?

In just the past four years, international organizations, bilateral agencies, and think tanks have produced seven major books on how infrastructure reforms affect poor people (Estache, Foster, and Wodon [2002] on Latin America for the World Bank; Ugaz and Waddams-Price [2003] on Latin American and Europe for the United Nations; Brook and Irwin [2003] on the world experience for the Department for International Development and the World Bank; Kessides [2004] for the World Bank; Nellis and Birdsell [2005] on the world experience for the Center for Global Development; Alam and others [2005] on Eastern Europe for the World Bank; and Estache and Wodon [2006] on Africa for the World Bank). The main message of this research is that the reforms of the 1990s have generally increased the efficiency of infrastructure sectors but that these efficiency gains have not always been shared with users, particularly the poor. The six main reasons why the poor have not always benefited can be summarized as follows: (a) when tariffs were redesigned to
be more efficient (when countries eliminated cross-subsidies, for example), they sometimes became less progressive or more regressive; (b) major increases in indirect tax rates—which tend to be more regressive than other taxes—were applied to reformed infrastructure sectors to allow the state to capture part of the rent generated by efficiency gains; (c) operators increased enforcement of revenue collections; (d) increases in quality made services unaffordable for some users; (e) cream-skimming in the design of restructuring eliminated cross-regional subsidies, slowing investment programs in the poorest regions when governments could not compensate through increased subsidies; and (f) failures to alleviate credit rationing added to the difficulties of financing poor users’ expansion needs.

All this implies that poverty was not addressed carefully in the regulatory and other reform packages implemented during the 1990s. Recent studies on Eastern Europe show that affordability may be just as important a challenge as access. In most cases, the negative poverty effects result from mistargeted subsidies.

Infrastructure policy makers tend to target access and affordability separately, relying on different instruments to achieve these goals. For access there are three basic types of instruments: (a) instrument requiring operators to provide access (a service obligation to avoid unilateral exclusion by the provider); (b) instruments reducing connection costs (through cross-subsidies or direct subsidies built into the tariff design or through credit or discriminatory payment plans in favor of the poor); and (c) instruments increasing the range of suppliers (to give users choice, including the option of cutting costs by choosing lower-quality service providers).

For affordability, broadly speaking, all instruments work in at least one of three ways: (a) by reducing bills for poor households (through lifelines or means-tested subsidies based on socioeconomic characteristics or the characteristics of the connection, financed through cross-subsidies or direct subsidies built into the tariff design); (b) by reducing the cost of services (by avoiding granting a monopoly right when it is not necessary or by providing an incentive for operators to reduce costs and pass on the cost reductions to users); and (c) by facilitating the payment of bills (by allowing discriminatory administrative arrangements in favor of the permanently or temporarily poor) (Estache, Foster, and Wodon 2002).

For a long time, analysis of these instruments focused on efficiency and was conducted at a fairly theoretical level by public finance specialists. Enormous methodological progress over the past 10–15 years has allowed the academic world to make much better assessments of the performance of these instruments, not only in terms of efficiency but also in terms of their effects on the behavior of poor people and service providers. Methodological developments have occurred in three fields: microeconomics (especially developments in the econometrics of panel data), evaluation techniques, and incentive theory applied to the theory of regulation. Bourguignon and Pereira da Silva (2003) provide an exhaustive overview of the status of evaluation techniques. Although not yet widespread, much of this work builds on the incentive theory presented in Laffont and Tirole (1993) in general and in Laffont (2005) for developing countries.

A look at the associated empirical evidence is somewhat disappointing for infrastructure policy makers. The leading academic economic journals contain relatively
few articles on targeting, affordability, or regulation for infrastructure services in developing countries (even for health and education services, where there is much more data and the data quality lends itself better to academic publications).23

While much more empirical evidence needs to be generated, the partial results available so far are surprising. Despite their popularity in policy circles, particularly for infrastructure, targeted subsidies (as well as safety nets) have long been held in low regard by academics for their alleged ineffectiveness (in terms of economic efficiency and incentive costs). But new data suggest that in many cases these costs are modest: direct subsidies and cross-subsidies are not always as bad as they are thought to be. These results seem to hold for both temporary and chronic poverty.24 This finding confirms the intuition of many infrastructure practitioners.25

The evidence suggests that the poor can be deprived of infrastructure services in many ways. They often need a connection subsidy, as often mentioned by analysts of the access problem, but they also often need a subsidy to be able to afford a minimum level of consumption.26 Providing access when consumption is unaffordable is useless.

The evidence also suggests that because of the limited fiscal ability of many governments to generate enough revenue to finance targeted subsidies from general revenue, cross-subsidies are often the only realistic solution. Such subsidies help finance the needs of the poorest through redistribution within a sector. For every documented mistargeted cross-subsidy, there is a documented success story of an effective cross-subsidy, suggesting that they are an option to consider. But it is also important to recognize that well-intended targeting mechanisms have also been regressive and that this regressiveness may come from a failure to target access, consumption, or both.27

Poverty is also very often a distributional issue. Evaluation techniques now allow for very systematic assessments of the distributional implications of reforms. A reform can help poor people and be regressive, but it can also be regressive without helping the poor. These issues can now be reliably handled quantitatively. New techniques allow identification and monitoring of the most vulnerable groups with respect to reforms, as well as good evaluations of the relative and nominal impacts of reforms. But this literature provides much more than ex post assessments of policy reforms. It makes the case for systematic monitoring of the effects of new reforms and projects to ensure that they internalize lessons of the past. This can be done at the project level (see Baker [2003] and Duflo [2003] for surveys), at the sector level (see Torero and von Braun 2006 for a large set of country-specific cases studies in telecommunications), or at the macroeconomic level. Systematic monitoring can be important when microhousehold data are weak or interactions with other sectors need to be assessed.

Infrastructure reforms do not occur in a vacuum; they have an impact on the poor through their impact on other markets (such as the labor market and investment savings market) that matter to the poor. These feedback effects are potentially significant for poverty alleviation; an economywide analysis is therefore needed. This usually calls for a multiagent, multicommodity model. Computable general equilibrium (CGE) models are increasingly becoming a useful analytical response to these needs.
CGE models simulate the economic and social impacts of reforms. They are based on the socioeconomic structure of a social accounting matrix (SAM), with its multisectoral disaggregation. The basic idea behind a SAM is to identify the linkages in an economic system. The basic elements when constructing a SAM are input-output tables, combined with government accounts and household surveys. The household surveys are crucial for performing impact analysis on welfare and poverty. How deep the analysis can go depends on data availability. The CGE literature on the effects of public infrastructure service reform is rather modest. Their main contribution is to show the importance of infrastructure for achieving the MDGs and, perhaps most important, to show that good regulation is redistributive and progressive.

These techniques have not yet helped address one old battle in infrastructure—the debate on the need to address urban and rural concerns differently. There is wide agreement that infrastructure in rural areas can improve agricultural productivity and reduce rural poverty. But there is also some ongoing research on the impact on the rural-urban gap that can be credited or blamed on reforms. Boccanfuso, Estache, and Savard (2006) show that water reforms in Senegal have had a very different initial impact in the capital city, secondary cities, and rural areas. Unless interregional cross-subsidies are an option, most common cost-recovery financing policies hurt the poor differently in each region when the fact that each region is dominated by a different provider type (that is, large public, large private, or small private) is accounted for.

Adam and Bevan (2005) find that infrastructure investments in Uganda that support tradables have different impacts on the distribution of poverty between rural and urban areas as well as on the real exchange rate and other macroeconomic variables. When infrastructure investment is biased toward sectors that favor tradables (that is, telecommunications or energy, which tend to enjoy a much stronger demand from manufacturing and services than transport), the real exchange appreciation is strongest. When it is biased toward nontradables (for example, rural and urban roads), there is hardly any change in the real exchange rate. The main difference between the two scenarios is a distributional one. Support to tradables helps all income classes; support to nontradables helps the urban poor and, somewhat counterintuitively, hurts the rural poor, if population migration is ignored. The rural poor gain from more access to food, but they lose from the lower income they receive from food production. This loss is greater the more the infrastructure aid is biased toward nontradable goods.

Adam and Bevan (2005) provide one of the rare quantitative illustrations of the current policy relevance of the old debate on the importance of rural versus urban needs in infrastructure. There are many less quantitative debates. One is the extent to which there may be a bias in favor of alleviating rural rather than urban poverty. Because there is little research on infrastructure on this topic, policy makers must rely on anecdotal evidence. A related concern not addressed by researchers includes the possibility of a difference on this front across sectors (that is, water versus energy versus telecommunications versus transport).

All this assumes, of course, that the evolution of poverty is understood. There is plenty of evidence showing that poor people urbanize more rapidly than the
population as a whole, which implies that anticipating their needs will require a stronger focus on urban issues. Yet in many regions, the sheer numbers argue otherwise. In Africa, for instance, more than two-thirds of the population remains rural today, and it will likely be a while before the urban population becomes the majority. More generally, experiences across countries suggest that a majority of poor people will still live in rural areas long after most people in the developing world live in urban areas (Ravallion 2002).

The choice between urban and rural infrastructure is an empirical question (Reardon 2001; Sahn, Stifel, and Younger 2003; Lall, Harris, and Shalizi 2006), hence data matter. Living Standard Measurement Surveys (LSMS), Demographic and Health Surveys (DHS), and household consumption surveys do not provide the required data to address the issue well, for several reasons. First, the infrastructure sector is generally not well covered in these surveys. Second, there are significant differences in the quality of the data available for urban and rural areas. These data problems are important but can be circumvented. Lokshin and Yemtsov (2005) complement community-level panel data from a regular household survey with a special module, which they use to measure the impact of infrastructure rehabilitation projects in Georgia between 1998 and 2001. The analysis yields plausible rankings of welfare gains from different types of projects at low data costs for a specific country. The approach can be useful in assessing the impact of large-scale community-driven microprojects or government-run decentralized investment programs.

Researchers have failed to address some fundamental questions. How consistent are the ideal strategies implicit in these differentiated needs assessments for the rural and urban poor with strategies to maximize the odds of meeting the MDGs’ poverty target? Where is the poverty reduction from an additional dollar of investment higher, in highly dispersed rural populations or in highly concentrated urban or peri-urban populations? There are clear trade-offs depending on the cost of technology (low unit costs in rural areas versus low average costs from economies of scale in urban areas). Unless the needs of rural and urban needs are disaggregated across the MDGs, rural poverty reduction will probably not receive the priority it deserves. The fact that the MDGs do not distinguish between persistent and transient poverty favors the rural poor. This implies that strategies designed to accelerate growth to achieve the desired reduction in an overall index of poverty may be better than those that benefit the persistently poor (Gaiha 2003).

Fiscal and Other Financing Options as Challenges

The main policy and academic debates on the fiscal dimensions of the sector are about the macroeconomic limits to public and private sector financing. The most important source of disagreement is probably the extent to which there is fiscal space to finance the major increase in infrastructure expenditures demanded by growth and concerns for poverty alleviation. The debate is particularly sensitive because private participation is increasingly associated with implicit fiscal commitments, because of
The debate is also fueled by the fact that it is increasingly clear that in the poorest countries there is a limit to full cost recovery that can be imposed on the poorest. This implies that direct or cross-subsidies are likely to be part of the financial equation.

Fiscal requirements and financing options are closely intertwined. They are two interconnecting sides of the same issue: how much budget expenditures can be allocated to achieve a certain level of growth depends on the extent to which the users can cover the costs of the investments or operations and the level of affordability.

The core of the debate is about the importance of the design of macroeconomic fiscal adjustment programs for the level of investments in infrastructure. Standard fiscal rules adopted to ensure debt sustainability as part of macroeconomic adjustment programs are increasingly being criticized as excessively binding constraints on appropriate countercyclical action. Moreover, there is widespread concern that these rules may permanently reduce the public sector’s contribution to capital accumulation, particularly in infrastructure. Under a wide range of circumstances, compression of public investment in infrastructure can be—and has been—associated with lower economic growth and less efficient poverty alleviation. This, in turn, has fueled fiscal insolvency, the main concern expenditure cuts were supposed to address.

This debate has been intense in Europe as part of the assessment of the Stability Pact (for overviews, see Turrini [2004] and Buiter and Grafe [2004]). Recently, it has emerged in developing countries in the context of the search for an increased role of the private sector in the financing of infrastructure. Raised in a book edited by Easterly and Serven in 2003, it has now been mainstreamed in the policy arena. In 2004 both the Brazilian and Pakistani heads of state mentioned the need to find alternative solutions to fiscal adjustment that do not penalize infrastructure projects while recognizing that the new rules have to avoid white elephants.

The debate can be summarized as follows. Standard IMF adjustment programs want to ensure that public expenditures, including sectoral allocations of expenditures, are consistent with (a) the short-term liquidity constraint faced by a country, (b) the short-term aggregate balance (no inflationary pressure due to excess demand), (c) medium-term debt sustainability, (d) the need to avoid endorsing excessively costly or inefficient levels of public expenditures, and (e) the promotion of private participation in infrastructure. There is disagreement on how to address each of these concerns, because there is no agreement on the specific measures to consider; for each issue, there is some scope for sensitivity analysis.

There is concern that the liquidity constraint provides a lower bound for all the expenditure levels to consider rather than a precise indicator. While this constraint is a useful indicator, it needs to be complemented by an upper bound. This upper bound comes from three sources: (a) the definition of liquidity (Easterly and Serven 2003 for illustrations from Latin America), (b) the time horizon during which this liquidity needs to be considered and averaged out, and (c) the level and type of expenditures to be included.

In addition, it seems reasonable to have a better sense of the relevance of the level, origin, and timing of the financing sources for the assessment of the desirability of
infrastructure investment, in view of the fact that this has been one of the most creative areas in infrastructure policy over the past 10–15 years. The basic questions that need to be answered include the following: Which kind of financing sources, fiscal or quasi-fiscal, need to be covered? Do the sources (international financial institutions, bilateral donors, or others) matter? When are guarantees part of the quasi-fiscal deficit? What share of private participation throws the project off balance? Will this choice lead to cream skimming in the design of projects? Should it be driven by risk-sharing levels or by something else?

Many related technical issues go beyond the scope of this article. One, however, deserves highlighting. What needs to be recognized in estimates of the fiscal space is that solvency is by definition an intertemporal concept. Indeed, solvency has to rely on the present value of both assets and liabilities. Many academics have pointed out that it does not seem correct to assess the strength of fiscal accounts only from the time path of gross financial liabilities.38

Of particular interest in this context is the fact that infrastructure has an unusual cash flow, with high short-term costs and high long-term returns. Standard fiscal accounting ignores this and introduces a bias against any project with a cash-flow stream that is initially negative, with costs incurred in the present and returns accruing only over time. This bias leads to excessive compression of investment as well as operation and maintenance expenditures, particularly during the transition toward a deficit target; it can be particularly damaging for expenditures that help enhance future growth. Indeed, any analysis of infrastructure needs to distinguish between recurrent and capital expenditures and rate them according to their contribution to the growth and social agenda. The two are linked, but their relative importance varies significantly across sectors. Yet cuts tend to be across the board—with brutal (including regressive) distributional consequences (Calderon and Chong 2004).

Recent research is widening the debate. Engel, Fisher, and Galetovic (2006) suggest that it is useful to look at the fiscal cost of private participation in infrastructure from the viewpoint of the relevance of the financing modes to the public sector accounts. They establish an “irrelevance result,” arguing that under a reasonable set of circumstances, the deficit should not be influenced by the financing mode of infrastructure. This irrelevance result does not hold in many situations, but the point of the authors is that it should be possible to forecast the sign of the impact based on the specific situations to be addressed. Tirole (2006a) widens the debate into politics and the need to link the assessments of incentive problems in the sector to their fiscal consequences.

This is not only an accounting problem. Over the past 20 years, political decision making has replaced economic criteria in determining the allocation of resources in developing countries. Twenty years ago all the multilateral development institutions had their own manuals of economic cost-benefit analysis that were supposed to be used as part of annual public expenditure reviews. These reviews were supposed to guide the allocation of resources across sectors. Sectoral allocations and intrasectoral investment decisions were easy to implement, because they were driven by economic rates of return. The changes in the resource allocation process over the past 20 years
have resulted in less investment in infrastructure (to a large extent because of its lower profile in the overseas development assistance agenda), without much regard to the economic returns.

At least as important for some sectors and for some countries, there is a need to monitor the allocation of resources to the maintenance of assets. Rioja (2003) shows that in some countries, maintenance may actually be more important to growth than investment. Using an infrastructure-led growth model, Kalaitzidakis and Kalyvitis (2004) show that the durability of public capital is endogenous and varies according to its usage and the level of maintenance expenditure. They also show that changes in total expenditures and the maintenance share drive the steady state and the dynamic behavior of the economy. Allocation rules that imply lasting fixed proportions between investment and operational expenditures can thus have dramatic long-term consequences.

Whatever the outcome of ongoing research on the sustainable degree of public expenditures in infrastructure and its allocation between maintenance and investment, for many countries, partnerships with the private sector will continue to be a rational option. The most challenging dimension of these partnerships will probably be assessing the impact of risk allocation between the public and the private sector on the decision of the private sector to enter a deal.

Despite the relevance of the design of risk-allocation mechanisms, there is relatively little innovative theoretical infrastructure-specific literature on this topic from researchers specializing in the modeling of agency problems. This is surprising, as the theoretical literature on the scope for public-private partnerships is large.39

This research has generated several useful insights. The first is that regulators must arbitrate between risk levels and their distribution, the efficiency levels that can be achieved in infrastructure, and the rents that remain with operators. In other words, to be viable, a financing mechanism and a regulatory regime may need to rely on a risk allocation that does not yield the most efficient outcome in service delivery. This means, for instance, that when risk levels are perceived to be very high, rate of return regulation may be more effective than a price-cap regime in attracting private capital. More generally, this literature argues that the characteristics of developing countries should often lead to recommendations quite different from those for infrastructure restructuring in industrial countries. Indeed, the limited enforcement capabilities in developing countries are significant and, along with unusually high risk levels, one of the main reasons why one size does not fit all when reforming infrastructure. This literature also hints at the relevance of many other institutional issues, including the relevance of the degree of capital market development.

A second strand of research focuses on issues associated with the degree of development of local financial markets. This strand typically deals with more than just the financing needs of infrastructure (see Bortolotti and Siniscalco [2004] for a recent survey on the world experience and von Hirschhausen [2002] for an insightful discussion of the interactions between institutional development and infrastructure reform in Eastern Europe). The main message—that institutions matter—is now the bread and butter of many aid agencies.
A third area of research focuses on the optimal distribution of risks among the players in the financing game. It involves the development of innovative risk-mitigation products and applications to foster private capital mobilization for infrastructure development (see Esty [2004] for a broad review; Irwin [2003] for an application to infrastructure). The literature offers four main lessons to policy makers:

- From a strictly financial viewpoint, the financial structure matters—in ways that are relevant to the design of financing strategies in developing countries. Of particular interest is the importance of the governance structure associated with the financing of infrastructure projects (Tirole 2006b).
- Improved risk-allocation mechanisms addressing currency risks and regulatory risks can help reduce uncertainties faced by private investors assuming infrastructure-related risks (Irwin 2007).
- Credit providers can gain from coordination to reduce everyone’s risks levels when capital markets are imperfect (Tirole 2006b).
- A growing body of research demonstrates the importance of auditing and the limits of creativity in financial designs when financial accountability is limited (Iossa and Legros 2004).

The fourth area of research reflects the fact that a large part of infrastructure development takes place at the subsovereign level, with subsovereign entities responsible for providing public services (see Freire and Peterson [2004] for an overview). Fiscal capacity is a major issue when it comes to financing subnational investment needs (see Lewis [2003] on Indonesia). Supporting the transition of these entities from sole central government funding to market-based funding where they can also access private financial markets for their needs is therefore critical to mobilizing additional private capital for infrastructure services.

While these four branches of research all provide interesting insights, the punch line is a modest one. The main lesson of this literature may be that new instruments will have to do better at generating the appropriate credit enhancement to achieve creditworthiness at the project level and often at the local government level. Very little is known about how to implement this advice in real deals with high risk levels. The next generation of infrastructure contracts between the public and the private sector will have to do a better job at allocating these risks.

Of particular interest is the development of a law and economics research agenda that would investigate the relevance of risks associated with the mismatch between legal systems and the choices of regulatory instruments associated with infrastructure reforms (that is, concession or affermage contracts and the creation of independent regulatory agencies). Francophone Africa and many countries in other parts of the world have often reacted negatively to the imposition of independent regulators that did not fit into their legal tradition. Concession contracts have passed on to these regulators rights that typically go through other channels under existing constitutional arrangements dividing responsibilities among the three branches of government. As a result, regulators are sometimes viewed as a fourth branch of government. While
much work has been done on the relative effectiveness of the various legal systems in their pure forms in terms of their efficiency, equity, or fiscal effects, few studies have looked at the cost of hybrid solutions in which sectoral legal systems, contract forms, and regulatory processes or instruments from various legal traditions are combined. Until this issue is solved, regulatory and legal risks will continue to be major obstacles to successful public-private partnerships in the infrastructure sector.

Corruption

The final broad research theme emerging from the recent reform experiences in infrastructure may be the most complex one. At face value, it is about corruption. Ultimately, it is about accountability for governance failures, but it deals with a type of failure requiring much more political commitment than skills, particularly in a sector where corruption has long existed.40

In addition to the usual explanation of low wages in the public sector, two important features of infrastructure drive the higher than average risks of corruption: projects tend to be larger than in other sectors, and services are often granted with a monopoly on delivery as well control of the information needed to ensure that there is no abuse by the monopoly.41 While these characteristics have not changed much over time for electricity and water distribution and for much of transport infrastructure, the perception of their impact on corruption has evolved. In the early 1990s, the existence of widespread corruption among public monopolies in the sector was often one of the arguments used to motivate privatization. This anecdotal evidence was supported by the theoretical modeling of corruption as the nonbenevolence of government by authors such as Shapiro and Willig (1990), Shleifer and Vishny (1993), and Boycko, Shleifer, and Vishny (1996), as well as much anecdotal evidence.42 Assuming that it is easier for corrupt politicians to control public firms than private firms, these researchers argued that privatization could reduce the control government has over the rent offered by the full control of the sector by making political interference more costly or more visible.

Many reforms later, the main debate has now shifted from the interactions between public operators and users to those between private operators and government. This can be seen in the survey prepared for Transparency International on corruption and privatization in infrastructure in developing countries (Boehm and Polanco 2003; Transparency International 2005). It is also clear in various publications by nongovernmental organizations (Allouche and Finger 2002; Hall and Lobina 2002), documenting legal events that have demonstrated incidents of corruption in the sector. Friends of the Earth (2001) and various political scientists have documented the role of corruption as a cost driver in contract negotiations and renegotiations in the sector. There is also an increasing body of academic evidence. Flyvbjerg, Skamris Holm, and Buhl (2002, 2003); Flyvbjerg, Bruzelius, and Rothengatter (2003); Naess, Flyvbjerg, and Buhl (2006); and Mitlin (2004) document undesirable practices costing practices at the project level. More conceptual research (Benitez and
Estache 2005) is analyzing the changes in the global market structure characterized by an increased domination by a few players. Celentani, Gauza, and Peydros (2004) developed a model consistent with the fact that an increase in competition in international business transactions can increase corruption in the sector.

Most of the evidence offered by these surveys is anecdotal and indirect. There is no real systematic measurement of the level of corruption in the sector. With the exception of a database compiled by Clarke and Xhu (2004) for Eastern Europe and a ranking of utilities among corrupt institutions from the Global Corruption Report published by Transparency International (2005), the annual Global Competitiveness Report provides the only comparable, quantitative, multicountry overview of corruption in infrastructure sectors, ranking 59 developing countries according to the perceived degree of corruption (based on interviews with private firms), among many other criteria.43


A very promising research area is the use of randomized field experiments. Olken (2007) reports the results of a randomized experiment in Indonesia that measures missing expenditures in more than 600 village projects. The study compares the villages’ official expenditure reports with estimates of the prices and quality of all inputs used in road construction and maintenance, each made by independent engineers. This approach allows the sample to be separated into subsamples in order to test the effectiveness of various types of policies in reducing corruption.

What do these studies show? First, the basic data analysis from the Global Competitiveness report suggests that the frequency with which firms have had to make undocumented extra payments or bribes to get connected to public utilities or to gain public contracts is, on average, negatively correlated with national income: the poorer a country is, the higher the level of corruption in its infrastructure sector. While these data are useful, they are not precise. They are based on executive surveys, which are problematic, because they tell only one side of the story and rely on fairly subjective assessments. More important, the results say little about what the government or users of the residential infrastructure think about corruption.

Second, corruption can be tracked to greater constraints on utility capacity and less competition among utilities, as Clarke and Xu (2004) find for 21 Eastern European countries. They find that public ownership in that region is more closely correlated with corruption than private ownership of utilities.

Third, corruption can be associated with higher than expected costs. The most detailed studies (Flyvbjerg and various colleagues) show that excess costs can be
attributed to procurement rules that give bidders an incentive to announce low costs to increase their chances of winning projects and then renegotiate.

Procurement rules by themselves are not enough. Auditing contractual compliance also matters. Olken’s (2007) detailed analysis of Indonesian road projects is a good illustration. He tests the potential payoff of audits and other policy instruments intended to reduce the costs of corruption. His assessment implies that announcing an increased probability of a government audit from a baseline of 4 percent to 100 percent reduces unexplained costs by about 8 percentage points. This cost saving justifies the cost of the audits.

Not all results are as expected. Most of the surprises come from indirect estimates of the effects of corruption on infrastructure services based on cross-country regression models measuring corruption at the country level rather than at the sector or project level. Mauro (1997) and Tanzi and Davoodi (1997) find opposite signs on the effect of corruption on public spending: Mauro finds that it increases these expenditures, while Tanzi and Davoodi claim that it lowers them. The two results may not be inconsistent. It may be, for example, that corruption raises unit costs and hence increases spending in one sample, while under the budget constraints dominating the other sample, it reduces the number of projects and (because projects are lumpy) decreases spending in the sector.

Estache, Goicoechea, and Trujillo (2006) offer an econometric test of the impact of the 1990s infrastructure reforms and of corruption, as well as of their interactions on access, affordability, and quality of infrastructure services in developing countries. They find that corruption reduces access rates and quality in electricity and telecommunications affordability for residential users, has no statistically significant effect on water access rates or water and electricity affordability, and increases access rates and quality in telecommunications. The explanation for these results may be as follows. In many countries, the telecommunications sector was the first to privatize. This did not happen easily, and many of the participants to these initial transactions report that the opening of the market required side payments. This does not make these right. The outcome, however, was an increase in access and quality. These improved access rates came at a cost: higher tariffs for users, in a sector in which technology keeps pushing costs lower. For electricity, corruption did not affect prices, but it did reduce quality and access rates. In sum, when corruption is about money—rather than power or other nonmonetary factors—it will eventually generate higher cash flows for the corrupt parties. It can do so by increasing revenue from infrastructure services (that is, increasing access and hence users or prices) or by cutting costs (that is, reducing quality). Both of these strategies are easier to implement for a monopoly when it is poorly regulated or when the regulators and the operators collude, as discussed in Laffont (2005) and below.

Another puzzling finding comes from Rock and Bonnett (2004). They show that while in most regions corruption has the expected negative effects, in large East Asian countries with governments with long time horizons, corruption has had positive short- to medium-term effects on growth, thanks to collusion between governments and their big business partners. Rock and Bennett note that it is not clear that the investment choices associated with corruption in these countries are the right ones for
long-term growth, because they cater to the preferences of local businesses. Corruption has long-term costs in this case as well, but these are much more difficult to assess.

What can be done to reduce corruption in infrastructure? Theoretical researchers have been pushing in four main directions for the past 20 years: privatization, regulation and related processes, increased decentralization, and adoption of participatory process in the selection, implementation, and supervision of projects. Since many countries have adopted these recommendations, there are now enough facts to analyze. This analysis is still very new, but it is already yielding interesting results.

The evidence on the impact of infrastructure privatization on corruption is not yet very substantial. Laffont and Meleu (1999) provide a general description of the interactions between the two phenomena. Looking at Africa’s experiences, they point to a U-shaped interaction between corruption and the privatization rate: up to a point, corruption facilitates privatization, but eventually it hurts it. This finding needs to be contrasted with the fact that, in a recent survey (reported in Nwanko and Richards [2001]), corruption was considered the greatest obstacle to doing business in the region. These two stories imply that for now, corruption in Africa is an impediment to the adoption of a policy that could reduce corruption.

Some of the literature on the impact of the nature of ownership on the efficiency of operators can be interpreted as a proxy for the impact of privatization on corruption when efficiency is measured by costs. Among the few studies offering this possibility is Kirkpatrick and Parker (2004), who report on several studies they have conducted. The first of interest here is their analysis of a large sample of African water utilities in 2000. They find that ownership did not statistically significantly affect costs. While the authors do not formally test the linkages between corruption and ownership, it could be argued that if costs were influenced by corruption, ownership has no impact on corruption. A second relevant study refers to a large sample of electricity companies, for which privatization needs to be coupled with regulation to have the desired impact on prices. This result could be interpreted as meaning that a well-regulated switch to private ownership could reduce corruption.

Looking at a much larger data sample and covering a longer time period (1990–2002), Estache, Goicoechea, and Trujillo (2006) provide a formal test of the interaction between privatization and corruption by assessing their impact on access, affordability, and quality of infrastructure services in developing countries. Relying on a set of interaction dummies in a model explaining these variables, they find that privatization generally does not statistically significantly interact with corruption in electricity, telecommunications, or water. This is in contrast with the conclusion drawn by Clarke and Xu (2004), who find that switching from public to private ownership did reduce the level of corruption in Eastern Europe.

The fact that this survey reports only three studies suggests that there is little formal testing of the effectiveness of privatization as a way of reducing corruption. There is a clear need to consider complementing the relatively large literature on the impact of infrastructure privatization on efficiency, quality, and equity. Research also needs to get to the core of what seems to characterize monopolies, as suggested by
Estache, Gisecochea, and Trujillo (2006). The monopolies’ objective is to maximize profits. However, researchers have tended to focus on prices, quantities, and quality independently, because data on profits are not available. They now need to start looking at how reforms jointly affect these variables, and hence profits, to offset the consequences of corruption (for example, there is no need to affect prices if quality and hence costs can be cut to increase profits).

The second instrument generally recommended by researchers is regulation. Regulation must promote (static and dynamic) efficiency while protecting consumers, in particular the poor, from potential monopolist abuses and investors and operators from political influence. Some degree of flexibility is desirable, but the track record of governments in their use of flexibility is generally perceived as having been so problematic that the rules built into various privatization instruments are designed to limit this flexibility. One of the key components of these safeguard mechanisms is the specific design of regulatory institutions and the concern for the importance of independent, autonomous, and accountable regulatory institutions for sustainable reforms in regulated sectors. A major contribution of the theoretical literature summarized in Laffont (2005) in the context of developing countries is to show that processes, particularly quantitative processes, matter much more than policy makers seem to appreciate. In practice, this means reforming planning processes to get incentives right and to make information more transparent and better audited. The adoption of regulatory accounting guidelines, for instance, is commonly omitted by reformers, even though it is central to the ability to come up with fair, efficient, and accountable regulatory decisions (Estache and others [2003]; Schlirf, Rodriguez-Pardina, and Groom forthcoming). These guidelines are consistent with the theoretical case for monitoring, auditing, and associated penalty systems (Laffont, Faure-Grimaud, and Martimort 1999; Armstrong and Rochet 1999; and Khalil and Lawarree 2001).

New databases are emerging that cover a long enough time span to generate useful information. The most promising line of research may be that pursued by Olken (2007) to test the effectiveness of regulatory processes such as audits. But this type of research takes time and resources. A complement in the short run is to rely on cross-country econometrics to squeeze as much information as possible from international databases. Recognizing the difficulty of modeling something as complex as regulation, Estache, Goicoechea, and Trujillo (2006) rely on the largest current data set to test the interaction between corruption and regulation in terms of its impact on access and prices of utilities services in developing countries. Regulation in their model is approximated by the existence of an independent regulatory agency, that is, an institutional variable to address an institutional problem. They find that these agencies have often been effective but that they do not perform equally well across sectors or regulatory objectives. Regulatory agencies have offset the impact of corruption on electricity and telecommunications access but have had no effect on water access. Regulation reduces the impact of corruption on residential phone services and on industrial electricity prices; it has no effect on other prices. These mixed results are generally consistent with partial results obtained by other authors. In their analysis
of African water utilities, Kirkpatrick and Parker (2004) find that regulation does not have a statistically significant impact on their costs. Following a rationale similar to that adopted in the case of privatization, if costs are influenced by corruption, regulation has no impact on corruption. For electricity and in a wider country sample, Kirkpatrick and Parker find that regulation can improve performance but not for all types of indicators. These results are consistent with those of Estache, Goicoechea, and Trujillo (2006). As in the case of privatization, the extent to which regulation offsets the undesirable effects of corruption on infrastructure services is also a promising research topic.

Guasch and Straub (2005) model the interaction between corruption and regulation in the context of its effects on the renegotiation of infrastructure concessions in Latin America. They find that the higher the level of corruption, the more important it is to have a regulator in place to limit the incidence of renegotiations. There is also a very large body of theoretical literature on what regulation and how regulation can help (see Laffont [2005] on what it means for developing countries). The evidence on this topic is not consistent across papers.

Decentralization is the third way suggested by theory to increase accountability and hence reduce corruption. Since the 1970s many countries, particularly developing countries, have seen a major increase in decentralization. Although a large body of economic research has been conducted on the topic in general, the results for infrastructure have been modest. Bardhan and Mookherjee (2000, 2003) offer some of the most influential findings on the topic. They focus on infrastructure, highlighting the role of local corruption on the effectiveness of public service decentralization. They show that under fairly mild assumptions, decentralization financed by user fees rather than local taxes or intergovernmental grants is superior, no matter how poorly local democracy works. More important, if user fees are not used, the superiority of decentralized over centralized service provision is no longer as clear-cut as many policy advisers seem to believe when corruption is explicitly taken into account. Finally, when ability to pay is constrained and user charges cannot be used to finance antipoverty programs, the optimal degree of decentralization depends on the degree of corruption in local and central governments. This is research that begs to be tested.

For now, there are relatively predictable tests of the impact of decentralization on the efficiency of the various delivery modes and types of infrastructure, especially in developing countries (see Shah, Thompson, and Zhou [2004] for a general survey). Very little work has been done on the interaction with corruption, however. Bardhan and Mookherjee (2006b) conducted a survey of the scarce evidence. The first relevant empirical results date from 1995, when Estache and Sinha showed that for a sample of 10 industrial and 10 developing countries covering the 1970–92 period, decentralization tends to increase total and subnational spending on infrastructure much more in developing than in industrial countries. This could imply either preferences change with decentralization or cost increase with decentralization. The models tested do not allow differentiating between the two explanations.

There is then a gap in research until 2002, when Fisman and Gatti reached similar but more specific results, using a much more sophisticated model specification
applied to a data set of 59 countries. They find a negative correlation between corruption and decentralization for 1980–95.

Faguet’s (2004) results suggest that decentralization is more of a demand-revelation mechanism than a stimulus to corruption. He shows that in Bolivia, decentralization has led to a reranking of investment programs in favor of agriculture, education, and water and sanitation. These are useful preliminary results, but they beg for confirmation. More country-specific studies such as Faguet’s or cross-country studies that perform a fuller diagnostic are needed before the claim can be made that decentralization reduces corruption.

The last interesting area of research looks at the interaction between infrastructure decentralization and privatization. Working with a data panel of 40 countries between 1990 and 2000, Ghosh Banerjee, Oetzel, and Ranganathan (2006) find that fiscal decentralization significantly affects the level and frequency of private participation, but administrative and political decentralization do not. Fiscal decentralization tends to increase private sector participation in infrastructure. If decentralization is a demand-revelation mechanism, this result is somewhat surprising in view of the increasingly loud voices against private operation of public services in Latin America.

In many ways, the recommendation of more participatory approaches to service delivery—the fourth type of policy solution to mitigate the risks of corruption—can be seen as a by-product of the literature on decentralization (see Turk [2001] on how this is playing out in Vietnam). As with decentralization, little of the published economic research focuses on infrastructure services (recent exceptions include Chuwa, Zovu, and Mbula [2002] and Ackerman [2004]). Yet efforts to promote participation in projects, programs, and policy consultations are now common in the international community. While there is nothing specific to infrastructure, many of the assessments of these approaches are based on qualitative or impressionistic rather than quantitative assessments (Isham, Narayan, and Pritchett 1995 is a notable exception). As Ghazala (2004) notes, until his own 2004 paper, not a single study had established a quantitative causal relation between any outcome and participatory elements of a community-based development project.

The main picture emerging on the effectiveness of participation gives reason for concern. Ghazala (2004) and Cornwall (2003) observe that projects claiming “full participation” and “empowerment” have turned out to be driven by particular interests or elites, leaving the least powerful without voice or much choice. The poverty reduction effectiveness of these programs needs to be measured more systematically as well. The one quantitative study of an infrastructure activity is by Olken (2007), who finds that increasing grassroots participation had little impact in reducing corruption associated with road expenditure in Indonesia. He shows that top-down monitoring may be a better solution, even in a highly corrupt environment. In other words, traditional regulatory instruments have been more effective than participatory instruments in Indonesia’s road program.

This overview of a large volume of theoretical research on how to deal with corruption suggests two main areas in which further efforts are needed. The first is data. The measurement of corruption in the sector is still approximated by the level
of corruption in the country. The second area is assessment of the effectiveness of policy instruments for the infrastructure sector. The main message of this discussion of the effectiveness of theoretical solutions may be that there is not enough evidence to get a sense of how much and under what circumstances each really matters. When evidence is available, it is too narrow or insufficiently robust. This defines an important research agenda for the sector. Finding out more about the effectiveness of the theoretical recommendations on how to deal with corruption in the sector should be a higher priority.

**Concluding Comments**

This literature review omits much good research conducted on the topics addressed here. In addition, it excludes many relevant topics, not because they are not be important but simply because they have not been on researchers’ radar screens for some. Chief among these omitted topics is the need to revisit the issue of how to improve the performance of public providers of infrastructure services when privatization is not an option. Much energy has been spent over the past 10 years in identifying what works and what doesn’t with public-private partnerships; very little has been allocated to thinking about how to manage public enterprises better (a rare exception is Gomez-Ibanez [2006]). In retrospect, this is unfortunate. The private sector financed just 20–25 percent of total infrastructure investments in developing countries and transition economies in recent years, and the trend is now declining. The good deals in energy and telecommunications have been signed; massive new inflows are unlikely for a while. In the foreseeable future, the public sector will continue to be a key actor, particularly in the lowest-income countries.

Closely related is the need to look into the regulation of public operators. Most of the research on infrastructure has tended to focus on the independent regulation of private operators. What about the independent regulation of public operators? Should the regulatory instruments be the same for public and private monopolies? Should regulatory accounting standards be the same for these two types of monopolies? Should public-private partnerships that do not require private sector investment affect the choice and design of regulation?

Also related is the need to establish a much better bridge between the theoretical research being conducted on procurement and the design and implementation of policies to procure public services in infrastructure. While auction theory has had a huge impact on telecommunications (spectrum auction), electricity (the design of power pools), and airports (slot allocations), its major contributions have hardly trickled down to more standard public sector procurement at the country level or in the procurement practices of bilateral or multilateral donors. The potential costs savings and cost controls that can be achieved from auctions are seldom considered in public sector reform. Similarly, the private sector has done a much better job than the public sector at internalizing theoretical research results in screening participants, structuring contracts, and centralizing procurement decisions. Recent research has
developed much more advanced tests to minimize the risks of collusion while continuing to benefit from the interest of a wide range of bidders.

Finally, a common point in all of the themes touched on in this article is the need to generate data. More data are needed, and not just to produce more and better research that will eventually guide future reforms and policy choices in the longer run. More data are needed above all to increase the short- to medium-run accountability of all actors involved. Trying to improve the accountability of donors, governments, politicians, operators, investors, and users through institutional reforms will not go very far until there is enough quantitative information to spell out a baseline from which progress can be measured. The MDGs are a good start, but accountability should not be only about access, it should also be about affordability, public and private costs, risks, and quality. Without more and better data on these dimensions of infrastructure service delivery, there will be no accountability in the sector, inevitably leaving the poorest users and the taxpayers to bear the bulk of the costs of poor service and corruption.

Notes

1. The concept of infrastructure has a wide range of definitions in the literature. In this article, the term refers to all facilities used to deliver energy, water and sanitation, telecommunication, and transport services. Irrigation is not included, although it is an essential dimension of the management of the water sector.

2. The last time the academic world became massively interested in infrastructure was after the publication of Aschauer’s 1989 article on the importance of public capital in the United States.

3. See Estache and Goicoechea (2005a) for a longer discussion.

4. This is not to deny the presence of the private sector. In fact, where the state and the large private sector have failed to deliver services, the small-scale, generally local, private sector has filled the gap. The evidence on their role, and details of their costs, is mostly anecdotal, however.

5. This estimate has been made independently by researchers at the Department for International Development (DFID) and the World Bank (2005). Very roughly, it has been worked out as follows. The international community has some sense of the physical capital stocks per country and can hence value them at constant prices. The change in the value of these stocks gives a sense of the total investment in the sectors. The contribution of the private sector to that investment is given by the total commitments made during the same period by the private sector according to the World Bank private participation in infrastructure (PPI) database. This is likely to be an overestimate, because commitments are not necessarily disbursed.

6. No countries have fully independent regulatory agencies. Often these agencies have some degree of autonomy from the ministry covering the sector they are responsible for. When politicians want to take over the regulatory function, they simply do so, as the Latin American experience of the past three years suggests.

7. See Estache and Pinglo (2005) for all developing countries and Sirtaine and others (2005) for Latin America.
8. Sirtaine and others (2005) provide a detailed analysis of the evolution of the cost of capital in Latin America and compare it to the rate of return that can be estimated from the balance sheet of the main infrastructure operators in the region.

9. The rejection of the infrastructure reforms of the 1990s, in particular the increased role of the private sector in the delivery of services, did not play a minor role in the wave of political change in Argentina, Bolivia, Brazil, Uruguay, or Venezuela.

10. Indeed, reforms often have fiscal costs, often generated as part of renegotiations that could have been anticipated if consistency framework documenting the sources of costs and incomes of the regulatory operators accounting for reasonable demand forecast had been adopted more widely. Of crucial importance is the need to recognize that the gap between the rate of return of the business and the costs of capital will be paid by taxpayers or users. It turns out that the taxpayer has been called upon much more often than is sometimes recognized. See Campos and others (2003) on the actual fiscal cost of the sector after 10 years of reform in Latin America.

11. Investors in Argentina would probably argue that the pesofication of the economy implemented in January 2002 is the best evidence so far of what that risk means.

12. In the Demographic and Health Survey data, the poorer and richer are defined based on an asset index used as a proxy of the welfare level. In the Living Standard Measurement Survey data, households are ranked by total per capita expenditure.

13. The most recent evidence suggests that the MDGs are unlikely to be met in many countries of the world (World Bank 2005).

14. Some countries are starting to draw their own baselines independently. India, for example, has created the very useful annual India Infrastructure Report series, edited by 3iNetwork.

15. See the analysis of Spain by de la Fuente and Vives (1995) for a perfect example of how creative empirics building on good theory can guide public investment decisions.

16. See Baldwin and others (2003) for an overview, including a chapter on the relevance of infrastructure for effective regional policies.

17. The minimum population threshold for defining urban areas varies dramatically among countries, but “urban” is typically characterized by density of settlement in a contiguously built-up area, by the structure of economic activity, and sometimes by administrative attributes.

18. Diversification of income sources is a key component of rural poverty reduction strategies and one that depends on infrastructure to be effective (see Ellis [1998] for a survey). In a survey of the literature identifying a poverty trap similar to the one observed in regions with large rural populations, as in Africa, Booth (2004) lists eight factors used by all authors to explain poverty that is mostly rural in those regions. One is poor land and sea transport infrastructure, which makes market development unusually difficult. Fan, Jitsuchon, and Methakunnavut (2004); Fan, Zhang, and Rao (2004); and Fan and Chan-Kang (2004) provide impressive evidence on the various channels through which infrastructure contributes to poverty reduction and show how different these channels can be across as well as within countries.

19. There are more than 400 cities with a population of more than 1 million—up from 16 cities 100 years ago.

20. These institutions have also generated a huge number of unpublished studies and working papers, some of which are available on their Web sites. There are too many to do them justice here.

22. This issue is not addressed here; interested readers should see Chisari, Estache, and Waddams-Price (2003); Clarke and Wallsten (2002); Cremer and others (2001); Gasmi and others (2002); and Laffont (2005).

23. For a overview of the literature on subsidies of relevance to infrastructure see Komives and others (2005).

24. For a useful review of the debate and survey of the empirical evidence, see Ravallion (2003).


26. International organizations and most countries define minimum consumption levels for water and energy. The rules of thumb are that households in developing countries should not spend more than 5 percent of their income on water and sanitation or more than 5–10 percent on energy (depending on the region).

27. Estache, Foster, and Wodon (2002) show how common this is in Latin America.


29. See van de Walle and Nead (1995); Lanjouw (1999); Jacoby (2000); van de Walle (2002); Gibson and Rozelle (2001); Renkow, Hallstrom, and Karanja (2004); Lokshin and Yemtsov (2005); and Warr (2005).

30. Many in the water community would argue against this, at least for their sector. According to World Health Organization statistics, rural areas in developing regions have 5.3 times more unserved people for water supply and 3.6 times more unserved people for sanitation than urban areas (WHO Web site). This service gap is not well correlated with the direction of the lending program of many donors. For instance, in the World Bank’s lending portfolio between 1990 and 2001, urban areas received nearly six times more in loan funds than rural areas, a difference that is not explained by differences in unit costs. The difference may reflect more strategic decisions on resource allocations. According to the World Bank Evaluation Department, each dollar spent on a rural water system provides about four times the population coverage offered by an equivalent urban investment. This could imply that more should be done to cover rural areas, at least in some regions. It could also imply that these numbers reflect a selection bias in the World Bank’s portfolio. Improving collective knowledge on this issue could be an interesting research area.


32. This is not a new debate. Lipton (1977) and Mellor (1976) were concerned with the opposite question: was the urban bias of the international community rational?

33. See Sahn, Stifel, and Younger (2003) for a more relevant approach to assessing the relative importance of infrastructure in poor people’s expenditure patterns.

34. Satterthwaite (2004) provides an interesting discussion of the data issues.

35. According to Mitlin (2004), because of the typical failure to break down urban averages (where access invariably looks much better than in rural areas, because the rich live in cities), the benefit of the doubt in most country assistance planning, including Poverty Reduction Strategy Papers (PRSPs), is being given to the rural areas. She documents her point in a review of 23 PRSPs, finding that they do not give much weight to urban areas.

36. For a good overview of the issues, see Irwin (2007).

37. Questions include the following: Which kind of public enterprises should be included? Should they have hard budget constraints? Should public enterprises be outside the budget (as in Chile)? Which kind of projects should be included? Which kind of guarantees
should be accounted for as expenditures and at what time? Should these guarantees be accounted for on a cash or accrual basis? Should recurrent and capital expenditures be systematically separated for every sector (à la Blanchard and Giavazzi [2003])?

38. For an economic discussion, see Ballasone and Franco (2000); Blanchard and Giavazzi (2003); Buitrer and Grafe (2004); and Turrini (2004). For an accounting viewpoint, see McCrae and Aiken (2000).


40. Flyvberg, Bruzelius, and Rothengatter (2003) give an excellent sense of the size of the problem in this sector, in particular in developed economies.

41. Favoritism, fraud, cronyism, patronage, embezzlement, bribes, and state capture are all concepts that have long been associated with the delivery of infrastructure services in many countries. There is extensive literature on how to define corruption and on the semantic practices of different institutions; a helpful recent survey is Lanyi (2004). For a recent survey on economic analysis of corruption, see Aidt (2003); on levels of corruption, see Kaufmann, Kraay, and Mastruzzi (2003).

42. New models include incentive structures that are consistent with the Latin American stylized facts (Martimort and Straub 2006). They show that private ownership can foster investment while increasing corruption.

43. There are also country-specific databases dealing with publics services, but these are the exception rather than the rule (see Reinikkka and Svensson [2002]; Svensson [2003]; and many of the country reports from Transparency International available on its Web site).

44. The evidence on the governance payoffs of privatization does not necessarily apply to infrastructure privatization. Privatization of competitive industries, most of them unrelated to any concept of public service, entails very different actors and interests. These differences have seldom been accounted for in the literature on privatization, which tends to extrapolate whatever is learned from telecommunications, the most competitive of the infrastructure services, to all other infrastructure sectors.

45. A notable exception is Shleifer and Vishny (1993), who argue that more vertical tiers of government tend to weaken governance.

46. A large body of public administration literature documents the impacts of various forms of public service decentralization in developing countries. Analysis of decentralized health and education services is reviewed in the World Development Report 2003 (World Bank 2002).

47. A forthcoming book edited by Dimitri, Piga, and Spagnolo may be the first since Laffont and Tirole (1993) that addresses the procurement issues thoroughly. This book also has the benefit of many more illustrations of relevance to practitioners.

Bibliography


