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INSTITUTIONS FOR INFRASTRUCTURE IN DEVELOPING COUNTRIES: WHAT WE KNOW... AND THE LOT WE STILL NEED TO KNOW*

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Abstract:

The paper surveys the very heterogeneous economic literature on the scope and limits of efforts to match institutional constraints and needs, on the one hand, and infrastructure policy and project designs, on the other, to increase the odds of improving service access, affordability and quality. It includes a categorization of the main theoretical approaches and a brief summary of what they each contribute to the design of institutions. It summarizes the most robust conclusions from theory and evidence as well as the main gaps between theory and practice that would justify a solid research agenda. The information collected is also used to assess the possibility of conducting sector diagnostics.

Institutions matter for growth and inclusive development, but despite increasing awareness of the importance of institutions on economic outcomes, there is little evidence on how positive institutional change can be achieved. The Economic Development and Institutions – EDI – research programme aims to fill this knowledge gap by working with some of the finest economic thinkers and social scientists across the globe.

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1. Introduction

This paper discusses the theory and analytical empirical evidence on the matching of policy and projects designs with institutional constraints and needs, in order to achieve the desired outcome in terms of access to infrastructure services, the quality of these services and their affordability to users and taxpayers. Infrastructure, as defined here, covers public services as diverse as water, electricity, sanitation, public transport and telecommunications. This diversity may often be the first matching challenge.

The heterogeneity of the production structure of these services can, indeed, be enormous within any country, across countries as a well as across sub-sectors and within subsectors. Infrastructure services are often best provided by large networked systems characterized by large economies of scale and scope, if the goal is to minimize average production costs. In those cases, the investments projects tend to be lumpy and demand significant lead times in construction. A new road or a new power generator demand large investment commitments and does not get built in a few months but may take a few years instead. This has fiscal and political implications which are both influenced by the institutional context of the sector.

Most sub-sectors also demand a long term budgetary commitment to maintenance of the assets.[2] Potholes on roads and regular power outages are visible indicators of poor maintenance and of poor budgetary practice. But under-maintenance can also reveal an underestimation of demand in the design of the projects. More generally, maintenance underperformance reveal institutional arrangements unable to ensure that the investments are of the right size, at the right price and quality, at the right location and at the right fiscal level. The issues can arise at the subnational, national or supranational level. [3]

Because standardized public decision processes fail to deliver the cost efficient solutions, often, in poor countries, the basic technologies end up being quite different. Process failures lead to institutional and technological Darwinism. Less capital intensive production alternatives are adopted and more local implementation and monitoring mechanisms get put in place. These mechanisms define the production differentiation needed to meet local initial conditions and preferences, ranging from technological choices matching skills to contractual arrangements matching local and national legal traditions and local cultural norms. Usually, these mechanisms are subject to more subtle incentives structures largely ignored in "macro" efforts to improve performance in the sector conducted in the last 25 years.

These "macro" efforts have pushed for standardized approaches under the assumption that to achieve the main performance goals faster and access the latest production and management technologies, it made sense to import institutional designs from developed countries into developing countries and to adapt them as needed. The idea has often been that the local populations would adjust to any change in their interactions with providers because it would be in their best interest to do so. It did not work out that way.

So far progress has continued to be slow but the evidence also shows that sticking to the old "pre-reform" model would have not made much of difference, in terms of access notably. Indeed, for those sticking to the old "business as usual " public enterprise approach or those trying alternative options by choice or by force (because they could not

[2] It is easy to forget that social rates of return are computed under the assumption that maintenance will take place over the lifetime of the assets, not just randomly subject to political decisions to allocated resources to budget or not.

[3] The degree of complexity is not linear in the level of government. For instance, supranationality complicates the optimal design of regulation but not always predictably (Auriol and Biancini (2015)).

try out the private option because not private operators were not interested), the achievements have been just as modest. Under most institutional models, success stories have been partial and/local rather than high profile national experiences that can easily be adapted to other contexts.

Simply put, after 25 years of trials and (often) errors, access and quality progress has been slow and gaps are, simply, still huge. A glance at the infrastructure statistics available on the web site of any international development agency shows that about 0.9 billion individuals (roughly 1 in 8) in the world still lack access to safe water. Over 80% of sewage in developing countries is untreated. Around 1.1 billion don't have access to electricity, and a third of the developing countries go through at least 20 hours of power outages/month. Almost 3 billion still cook with polluting fuels (kerosene, wood, charcoal or dung). And close to 1 billion do not have direct access to an all-season road. With the increased urbanization, mobility is also increasingly rationed in cities when investment does not follow since for 1,000 new inhabitants, roughly, an added 350 extra daily trips are generated.

The *de facto* limited access to basic infrastructure is not a minor issue either since it hurts human well-being (Lipscomb et al (2013), McRae (2015) or Torero (2015). But it is not only about that limited residential access. Infrastructure is also broadly seen as essential to growth and competitiveness (Calderon and Serven (2014) and Alby et al. (2013)). It matters to regional integration (Kuroda et al (2007). It matters to job creation (Dinkelman (2011) or to food security (Blimpo et al. (2013)).

The sector's enormous social, and hence political importance, imposes its own set of constraints on institutional options. Residential demand is strong and low income users are willing to pay up to 30% of their income for these services. Their residential consumption is characterized by low long term demand elasticities to both price and income. But many users are also quite emotionally sensitive to price increases, even when they are the only alternative to subsidies for a given service level. In the water sector, for instance, households tend to be unwilling to pay tariffs that cover costs even if they are willing to pay up to 5% of their income on the water and sanitation services (often more in fact).

Its strong social dimensions, added to its importance as input into growth processes and as a source of environmental externalities, makes it hard, at first sight, to understand why infrastructure is underfinanced. And yet it is. Most estimates of the foreseeable annual infrastructure expenditure gap (including the costs of greening the sector) are in the US$1.5-2 trillion range. At the country level, this is about 3 to 10% of GDP depending on the country's development level. The poorer is the country, the higher is the gap. For some African countries, infrastructure investment and matching maintenance expenditures needed to support the growth rates demanded by efforts to erase poverty are as high as 15% of GDP. In many upper middle income countries, these needs drop to around 4-6% of GDP, which is still quite significant. Rationing resulting from this macroeconomic infrastructure expenditure gap hits the poor the most. And the gap also forces many firms to invest in costly alternatives or give up on potentially competitive activities. The slower the progress in improving access to reliable infrastructure, the slower poverty reduction is. Time matters and delays hurt.

For now, on average, only 50%, at most, of the expenditure needs find financing in developing countries. From this, roughly 80% is from public sources (including international and bilateral donors) and 20% from private sources, on average also but with a very large variance. The poorest countries do not get any private financing while BRICS get a much larger than average share. Only 45% of developing countries get some significant form of private sector participation and about 25% face a similar situation in their electricity sector.

The infrastructure challenge is actually probably bigger than these figures suggest. It is likely that the gap estimations are lower bound because they assume that the projects are

selected to minimize their costs throughout the project cycles. The assumption is a strong one, since the scope for costs cuts from procurement improvements and productivity gains are quite significant in most countries.[4] Poor project selection, poor maintenance and poor ability to improve access overall are all linked to institutional choices. And the financing gap gives an idea of the cost of getting policies and institution wrong.

As the rest of the paper argues, a mismatch between policy and financing decisions on the one hand, and institutional weaknesses or preferences on the other, has helps explain the sector's financing problem. Since the mid-1990s, the most "popular" institutional and policy decisions to increase financing have often been anchored in a conviction that, limiting and refocusing the role of the public sector, would be enough to: (i) open the flows of private money to the sector, and (ii) provide the right maintenance incentives for the normally long life assets.[5]

Implicitly, the idea was that public money would no longer be needed or only be needed for the few activities the private sector would not be interested in. *"Get the investment climate right by implementing the right institutional reforms and the rest will follow"* has been, and continues to be, a common message that also applies to infrastructure. In many cases, *"right"* has been taken to simply mean "*less"* rather than *"better"* government intervention. In other words, since the instruments to achieve the policy goals have largely been pre-set, the remaining thing to do is to adapt the institutions to these choices. This ignores a possible two-way causality between institutions and policy.

A common (explicit or implicit) argument in favour of this approach is that, since it worked for telecoms, it should also work, for other infrastructures. But this is misleading. The telecoms success is largely driven by an exceptional continuous technological transformation since the 1980s which has cut costs (and hence eased cost recovery) in a way that no other sub-sector has been able to observe. Insitutions are essentially only needed to make sure the rents are shared. For now, all stakeholders (users, taxpayers, workers, investors and providers) are better off than they were in the 1980s in telecoms.

In the other sub-sectors, the distribution of payoffs is less predictable. Even in reform cases relatively well prepared, many of the institutional changes were incomplete and failed to deliver on promises to some of the stakeholders (e.g. job creation or gross of tax average service price cuts). Often, the failure was rooted in the adoption of over-standardized institutional changes across subsectors and across countries. This is despite the fact that these were constrained by very different broader non-sector specific capacity, governance, and political constraints. Mexico's constraints are not Mali's constraints and yet the initial broad reform approaches were similar because the international community underestimated how serious the differences were in many dimensions. We now know they are important but we did not know then. This was also true in the case of transition economies. The similarity of approaches across the region was based on the assumption that markets orientation would automatically lead to the adoption of market oriented institutions (von Hirshhausen and Waelde (2001). This is likely to explain the poor performance of the power sector in that region now (Nepal and Jasmab (2012)).

What many casual analysts of the sector had in mind (and still have in mind) when they thought about institutional reforms in infrastructure was: (i) deregulation, (ii) privatization broadly defined and corporatization of the residual public enterprises and (iii) the creation of autonomous independent regulatory agencies (i.e. autonomous from their

[4] Infrastructure productivity gains could cut financing needs by $1 trillion/year (McKinsey (2013)). Flyvbjerg (2014) reminds us that, over a 70-year period, for a sample of 258 transportation infrastructure projects in 20 countries spread of five continents, road projects averaged cost overruns of 20.4%; bridges and tunnels averaged cost overruns of 33.8%; and rail projects averaged a 44.7% cost overrun.

[5] See Rodrik and Mukand (2016) for a broader discussion of ideology in policy marketing.

ministries and independent from political interference). But institutions cover more players in practice and these are often ignored. An increased role for consumer associations, cooperatives, NGOs and other civil society actors should also be part of the institutional toolkit. In addition, procurement, market design, market coordination and planning are all essential components of this toolkit to produce the institutional adjustments needed to improve governance in this traditionally highly corrupt sector. And for urban infrastructure, ignoring the relevance of the design of decentralization to allow subnational and local governments decide themselves among the options for local public services, would be a major omission.

The recent policy literature also points to a variety of omitted institutional dimensions much harder to target through policy. Yet, they should be controlled for in the design of policy and in the choice of instruments. These include the legal tradition, the colonization history, ethnic heterogeneity, religion, culture, other traditional norms, including those that define informal institutions for instance. In the context of infrastructure, many of these dimensions have often been better studied by non-economists than by economists. For instance, anthropologists have quite effectively documented how these factors impact infrastructure and how they can help or slow institutional reforms because of some of their non-economic implications. (e.g. Larkin (2013)). They see infrastructure as enabling the circulation of "political" power control as much the circulation of people or goods.

Finally, both evidence and theory now emphasize the role of politics in the sector, with the benefits of insights granted by experience (i.e. usually mistakes). The initial reason is the incompatibility between the long term commitments to be made to long-lived assets that require maintenance, and the short political life cycles. Many of the ex-ante cost benefit analysis for these projects imply long term commitments to maintain. These commitments are routinely violated when fiscal constraints become binding or resource allocation priorities change. Unsurprisingly, ex-post evaluations then find the returns on the project to be lower than anticipated simply because service levels are poor, or because rehabilitation costs needed to address under-maintenance explode. Part of this is linked to a second bias politics often introduced: white elephants. To get approval for projects with high political payoffs but uncertain economic value, strategic underestimation of costs ex ante and overestimation of demand is quite common. It happens because there is little political accountability for political interference or lasting bureaucratic weakness.

This paper focuses on the recent evidence from economic research. This is a limitation as some key constraints identified by non-economists are only mentioned rather than discussed. This means also that some effects may be underestimated, such as the relevance of changes in power structures driven by changes in ownership or space management. Another weakness is linked to the fact that the test of time has not been friendly to a lot of the empirical evidence on the impact of institutional restructuring of infrastructure over the last 25 years. While the purely theoretical research is still quite useful in guiding analysis, much of the empirical evidence has established correlations rather than causality. This reduces the robustness and some of the relevance of this evidence.

With these limitations in mind, the paper is organized as follows. Section 2 reviews some important concepts on infrastructure and some technical and political dimensions relevant to the institutional challenges and options. Section 3 summarizes the theories dealing with the sector's institutions. Section 4 surveys the empirical evidence on the impact of institutions. Section 6 discusses what policy could learn from academia to draw the sort of checklists needed to conduct proper institutional diagnostics. Section 6 discusses core knowledge gaps to inspire a research agenda. Section 7 concludes.

2. Some technical background

This section provides the unfamiliar reader with some basic concepts and a brief reminder of the main institutional changes that have been observed in the sector in the last 25 years. It briefly reviews most of the institutional dimensions that are typically covered as part of project preparation or policy development in the sector, including planning, procurement, regulation and the assignment of responsibilities across government levels and government agencies. Its main purpose is to highlight the changes in the relative importance assigned to the various dimensions over time.

In public policy, the concept of infrastructure covers: (i) the physical constructions and components of the utilities and transport networks of an economy (e.g. energy generators and transmission lines, airport runways and terminals, sewage collection networks, telecommunications wires), and (ii) the services they provide. Utilities cover energy, water, sanitation and telecommunications while transport covers airports, bridges, ports, rails, roads, buses, subways and trams networks, and waterways.[6] The investments, also known in the regulatory literature as capital expenditures or *Capex*, are needed to ensure the delivery of the services. Over time, the accumulation of Capex adds up to large infrastructure assets which need to be operated and maintained to deliver the services properly. The operation and maintenance expenditures are known as operational expenditures or *Opex* in the regulatory literature.

Very roughly, the *Capex* are needed to ensure access to a service and to deal with technological concerns such as minimization of climate change effects or optimization of the use of labour in the development and/or delivery of the services. The *Opex* drive the marginal cost of increasing usage and service quality (e.g the more a road is used, the more it needs to be maintained). *Opex* also end up impacting the average service price when the costs are to be recovered to reduce or avoid subsidy requirements.

This is not anecdotal in a development context since the higher the average price expected by investors interested in recovering their commitments, the more challenging it is to ensure affordability under the most popular forms of price discrimination adopted by operators and regulators (without subsidies). In practice, the main challenge is linked to the recovery of the connection charges rather than the usage charges. When set too high, it limits actual access to services provided by large utilities in the sector (e.g. water may be available but if some consumers can't afford it, they, de facto, have no access). But to recover costs and risks, it often has to be set higher than what many users are willing or able to pay, if there are no subsidies.

This average price is also linked to institutional capacity through cost and demand forecast mistakes. The investments in the sector are usually not only lumpy but also characterized by a high degree of investment specificity (i.e. sunk investments). When a road expansion costs US$50 million, a 5% mistake (from a wrong traffic forecast or procurement process) does not have minor consequences for the users or taxpayers.

Mistakes can actually also be a proxy of the size of the potential rents stakeholders fight for in the context of a project or a policy reform. They tend to be large and repeated, which illustrates the size of the rent for firms and/or politicians if mistakes are intentional and possible because of weak institutional capacity. [7] This is why getting institutions right from the procurement stage to the regulation stage is essential. The infrastructure industries typically demand significant government supervision and eventually a commitment to deliver residual needs.

[6] Housing and irrigation facilities are sometimes also included in the concept, but they will be (largely) ignored in this paper.

[7] And the potential size of this rent explains why there is a bias in favour or large projects.

The record makes some institutional sources of performance failure predictable. Procurement design and implementation, unclear property rights (i.e. ownership confusion), imperfect contract designs, monitoring and enforcement weaknesses, and captured or incoherent regulation have all hurt since the early 1990s (Clifton et al. (2014)). However, the gap between what can be learned from failures and what policy makers decide, is still large. Who should do what in the public sector (which public agency or ministry at which government level with which policy tool?) and what should be left to the market (which market?) or to private operators with market power, is an unfinished debate, with policy implications still tainted by ideology (dressed up as best practice).[8] Missing from these diagnostics, as discussed later, is the basic question emerging from the diversity of sources of market, government and institutional failures: "*under what circumstances to achieve which objective should which institutional and policy instrument choice be made?"* (Estache and Wren-Lewis (2009)).

Answering this question requires, first, a good handle on the characteristics of the market, in the specific sector, of the specific country, region or community, trying to improve performance, as hinted earlier. The size of the sector and the sense of entitlement to the services, combined with the difficulty of monitoring providers with market power and better informed than the authorities on their costs and on their customer, all matter to the institutional choices. The less transparent the market is: (i) the more--international or national--politics interfere with otherwise technical decisions (think of procurement), (ii) the more complex the scope to replace weak, easy to capture, institutions by institutions capable of ensuring efficient, fair and financially/fiscally sustainable infrastructures. In the Acemoglu-Robinson (2005, 2012) view of the world, unless the mechanisms that maintain extractive institutions are better understood, the harder it is to create inclusive institutions.[9] This is why answering the question also requires a good diagnostic of the main institutional characteristics of the sector with an emphasis on the potential political and administrative/bureaucratic weaknesses.

With the benefits of insights of the last 25 years of experience, we know that the challenge is particularly complex when governments suffer from a weaker technical or political capacity to engage in negotiation with some of the large international players who dominate some significant parts of the sector in parts of the developing world. This is not anecdotal or just conceptual (Clifton et al. (2016)). There are few potential providers in key infrastructure services and, in addition, are quite regionally specialized. The combination is an easy recipe for abuses and conflicts in sectors in which high profit margins linked to poor regulation imply high average tariffs or high subsidies. For instance, in April 2016, the French government launched an investigation of a major French transport player in Africa (Bollore). Similar investigations of key international infrastructure players have been conducted by Germany, Norway or Sweden.

The monitoring and supervision functions have been particularly sensitive issues, symptomatic of governance challenges. Since the efforts to rely more on the private sector through competition *in and for* the markets launched in the 1990s, it has been central to the case made to ensure the regulators' independence from political intervention. Historically, one of the main purposes of this decision was to reduce the risk of *de-facto* partial expropriations resulting from politically motivated or corrupt interferences with contractual commitments made by governments to private investors and operators. We

[8] Rodrik (2010) makes this case for a much broader range of policies.

[9] Inclusive economic institutions create incentives and opportunities for the majority of the population and inclusive political institutions are those allowing broad participation and imposing limits and accountability on politicians. In contrast, extractive economic institutions create incentives and opportunities for a few and extractive political institutions concentrate political and economic power in the hands of a few, without accountability.

now have enough evidence to assess the extent to which the decisions paid off. As discussed later, the record is quite mixed but it is quite sensitive to the initial conditions that lead to this institutional restructuring as options for new reforms are being considered.

The efforts to try to attract private financing and know-how have also resulted from other institutional changes aiming at liberalizing key aspects of central public management. Key functions, traditionally public, have been passed on to the private partners. Passing on procurement responsibilities for the key investments and services as well as the due diligence typically associated with many large projects to ensure proper consultation with all the stakeholders has not been problem free. For instance, allowing the private operator to run key parts of the procurement process to minimize corruption risks can also open the door to poorly supervised transfer pricing techniques between the private service provider and some of its spinoffs turned into service or good providers. More generally, the evidence collected in recent years suggests a mis-match between the institutional constraints and the institutional development needs and the limited margin for adaptation of the procurement design largely imposed by international organizations as a condition for their willingness to finance the sector.

Some traditional functions have been de-emphasized but they are part of the institutional drivers to be considered. An example is the downsizing of public sector indicative planning capacity within each sub-sector. It has largely been the result of a justified concern for past government failures linked to weak governance leading to white elephants in the sector. Unfortunately, as discussed later in the paper, it also largely ignores some of the costs of not planning at all. This includes misspecifications of objectives and lack of clarity on the strategy to be adopted to implement the vision. In increasingly demanding policy environments, policies need to aim at multiple objectives (population coverage, competitiveness, environmental concerns, fiscal concerns, social concerns, …). Too often, these objectives end up implicitly being defined by the available sources of financing rather than by efforts to manage the multiple goals.

The blurred vision and the lack of planning are particularly damaging in infrastructure because of the long life of the assets. Most projects evaluations conducted by donors assume that the public and private discount rates are the same but they are not. Most omit the cost of raising public funds as well (Auriol and Warlters (2012)). Many projects ignore the coordination of investments decisions within sectors. Think of the management of the growing concern for water scarcity (e.g. Taher et al. (2016)) or of the diversification of the possible sources of energy with efforts to favour renewables. With the desire to ensure increased autonomy to private operators in investment and operational choices, in many countries (developed and developing), the planning role has lost its effective significance with important consequences. In developing countries, it has often resulted in cream-skimming driving the optimal sequencing of investment and other system improvements. In developed countries, the failure to coordinate, in the electricity sector, transmission investment decisions with the greening of generation nicely illustrates one of the consequences of this institutional change.

Hopefully, this section convinced the reader of the multiplicity of dimensions to jungle with, in the design of policy and projects in this sector. This multiplicity can be quite overwhelming in practice and explains why getting it right often requires a bit of luck as much as skill. It also should convince the reader that research can help but it is unlikely to be able to tackle this multiplicity of dimensions easily as discussed in the next section.

3. The place of institutions in policy oriented research on infrastructure

The mainstreaming of the concern for institutions is relatively recent in the "technical/analytical" academic infrastructure development literature. Getting a sense of the historical evolution is useful because it shows how different schools of thoughts have focused on different parts of the multiplicity of dimensions and produced a very broad and heterogeneous volume of results. It also shows that they have relied on approaches which are complementary rather than substitutes, with a bias of all approaches in favour of a number of narrow high profile policy issues.

Institutions are a relatively recent focus of mainstream infrastructure research. They only started to get a recurring profile in the early 1990s. Until then, they did not appear much on radar screens except when arguing that public enterprises were failing and that performance or management contracts were the solution (Gomez-Ibanez (2007)). Between the 1950s and 1980s, a lot of policy oriented research focused on pricing, project evaluation and more specific technical issues published by the aid (and theory) community (e.g. Hirshman (1958) for one of the earliest systematic discussions of the specific role of infrastructure in development and Jimenez (1995) for a stock taking survey).

The next wave, from the mid-90s, focused on the infrastructure-GDP empirical link. This was catalysed by the 1994 World Bank Annual Report on infrastructure as it extended Aschauer's work (1989) on the importance of infrastructure in the US to poorer countries (e.g. Canning, Fay and Perotti (1994), Ferreira (1996)). This also launched an interest in infrastructure as an impediment to growth convergence *within* countries (e.g. Demurger (2001) for China). It is during this research phase that institutions started to become recognized as being empirically relevant with Esfahani and Ramirez (2003) evidence on the infrastructure-growth-institutions nexus convergence from large panels of countries. But the modelling of incentives was too general to be able to get to sector specific recommendations since it was relying on very aggregate approximations of institutions at the national level rather than sector specific characterizations.

The real interest of academic economists in the sector specificities of infrastructure started in the late 1980s-early 1990s, as a reaction to the early evidence of the effects of the British privatization wave of the 1980s and as follow up to Argentina's and Chile's equivalent experiments. Development economists started to focus on this possible form of institutional change as a way to improve upon the incentive effects of performance or management contracts. In the privatization debates of the 1990s, the analysis of the role of institutions complemented the broader conceptual concern for the developing countries specificities to be picked up in regulatory designs. This included the horizontal as much as the vertical unbundling of the various dimensions of the infrastructure business (procuring, financing, delivering, regulating). The extent to which regulators should be independent and accountable is easy to analyse in this framework. So is the nature of the financing of the various activities.

Much of our current understanding can still be credited to the late Jean-Jacques Laffont and several of his co-authors (e.g. Laffont and Meleu (2001)), Martimort (1998, 1999)). Laffont, as many then, discussed the relevance of the privatization debates for developing countries but also addressed the relevance of market structures, risks, financing options and their inter-actions with regulatory design. With his co-authors, he also looked at the internal organization of institutions (e.g. the accountability incentive built-in the delegation of functions within and across ministries and agencies.). He synthesized most of the conceptual and supporting empirical literature in his last book (Regulation and Development (2005)) which may still be the best synthetic diagnostic of the incentive issues and their institutional implications for developing countries.

The "*Laffont view"* of development infrastructure, essentially anchored in agency models and incomplete contract theory, has been, and continues to be, influential in both academic and policy work when information asymmetries dominate and multiple agency problems characterize complex institutions. It emphasizes the incentive issues and is quite detailed on the distortion of incentives by regulation and institutions at a high cost to performance in terms of access, quality and/or affordability. It has also been quite good at showing how the optimal design of regulation and various types of institutional constraints interacts. It points to limited accountability, credibility, commitment, technical and fiscal capacity as institutional constraints that can be determinants of performance outcomes under various regulatory designs.

Since it starts from a modelling of the sector specific institutional weaknesses as well as country wide weaknesses as constraints, it is essentially a second best approach on the optimal choice of regulation. Some of the analysis conducted within this view also shows how some constraints can be alleviated and thereby change the optimal design of regulation and financing of the sector. More generally, the approach shows that when the full set of constraints typical of developing countries are considered, drastic approaches such as full privatization or the creation of autonomous regulatory agencies can counterproductive. Reform packages, which deal in much more targeted institutional changes, have better chances of enjoying sustainable success and establish the credibility of reformers.[10]

But is not the only theoretical approach, nor the first, to look at institutions in infrastructure. There are at least seven other approaches relevant to the institutional debates on infrastructure in developing countries. Clearly, this classification is subjective, but it may be useful to highlight similarities and differences as well as the value added of more recent perspectives as complements to the earlier perspectives. [11]

The other views on institutions in infrastructure. The sector's institutional policy issues have also been analysed with other tools. Besides the "Laffont view", there are at least seven other takes on how to address institutional issues in infrastructure relevant to policy discussions in developing and emerging economies:

(i) "Spiller or transaction costs cum politics" view advocated today by Spiller and his followers;
(ii) the broadly "legalistic" view documented today by Shleifer and some of his colleagues at Harvard ,
(iii) the "public choice" view with a lot of overlapping with the public administration perspective on the sector,
(iv) the "anthropological" view voiced today under different disguises by Bardhan, Platteau or Wade,
(v) the "dynamic political", more historical, view argued by Acemoglu, Robinson or Rodrik,
(vi) the emerging "behavioural" view of institutional design; and
(vii) the "empiricist view" which is made of a large number of papers with little theory but lots of interesting and generally useful data.

The "*Spiller view"* has strongly influenced our collective understanding of the extent to which institutions, in particular political institutions, impacted and constrained

[10] For recent discussions and examples relevant to debates in developing countries, see Auriol and Picard (2009, 2011, 2013), Iossa and Martimort (2015, 2016) and Estache and Wren-Lewis (2009, 2015)).
[11] Trillas has a useful perspective on the institutional debates on network industries on his blog. See http://realprogressinenglish.blogspot.fr/2016/05/second-best-and-new-institutional.html

Latin American infrastructure reforms (Savedoff and Spiller (1999, 2013), Guasch and Spiller (1999), Spiller and Tommasi (2005)). Spiller started working on the limits of government in the sector at around the same time Laffont started to extent his joint work with Tirole to policy diagnostics in developing countries.

Spiller's perspective has been also particularly influential in assessing the optimal organization of the production structure as a way to minimize transaction costs (bundling vs unbundling). Although the first steps should probably be credited to Williamson (1979) and to some extent Coase (1992), Spiller is actually quite close to Laffont's view, both in terms of concerns and to some extent in the modelling of the sources of institutional failures, although Spiller and his followers tend to focus on the impact on private investments in the sector and on the importance of the lack of public sector commitment. Although it is not as concerned with optimal regulation as Laffont's view, the Spiller view is just as interested in the relevance of rents and of their sources. And similarly to Laffont's school of thoughts, Spiller's argues that the desirable institutional mechanisms will be different across countries, and match institutional initial capacity.

Its main impact may have been the emphasis of the need to identify incentive and transactions costs issues associated with government opportunism (essentially the ability of governments to change the rules of the game and extract quasi rents from investors) and third party opportunism (essentially the questioning by NGOs, civil society,…). Politics should, thus, be central to regulatory assessments because it defines the governance of public-private interactions. The approach is closely related to arguments made by political scientists such as Heinisz and his co-authors Bergara et al (1998), Heinisz (2002), Heinisz et al. (2006). Their main message is that political stability makes infrastructure investment easier and instability slows or biases investment in the sector.

A closely related school of thought working on the institutions in network industries is linked to the Florence School of Regulation (which to a large extent could have been just as well labelled the Sorbonne School since so many of the Florence voices were initially anchored in Paris). Its members are also anchored into the transactions costs view of the world which can also be read as view that points to the failure to internalize the coordination costs associated with the development of governance as suggested by Dixit (2009). However, they but tend to have broader views of the issues and are quite effective at discussing the hybrid organizational outcomes of transactions cost in regulated industries (e.g Brousseau and Glachant, eds. (2008), Finger and Künneke, eds. (2011), Menard (2011), Glachant et al. (2013) and Saussier, ed. (2015)). They also have a stronger sense of the technical implementation details of the regulation of these sectors than usually addressed by Spiller and his co-authors. Some of them are particularly good at unbundling the characteristics of contracts and in highlighting their relevance for performance (e.g. Chong et al. (2015) and Saussier ed. (2015)). Although most of their research is on OECD countries, many of their insights have broader implications. Their main conclusions are: (i) once size does not fit all when it comes to regulation and contract design; (ii) details matter at all stages of the production process; (iii) coordination is particularly challenging in hybrid institutional models. They are not unexpected and validate earlier results but the fact that they reach them from another angle is reassuring.

The legalistic view or "Shleifer" view. The law, its origin and its implementation also matter. The relevance of the legal system to the performance of the economy has a long record which may have started with the early Coase papers (e.g. Coase (1992)). The global empirical comparative evaluation of legal approaches owes to the early research championed by Shleifer and his various co-authors (e.g Djankov, Glaeser, Laporta, Lopez-de-Silanes). Their point is that legal biases have an impact on economic outcomes because they influence and often shape laws, legal processes and dispute resolution in case of

conflicts. The main distinction they make is between the (French) civil law tradition and the (English) common law tradition. Their evidence has been extensively used in infrastructure discussions as it raised the concern that civil law increases the riskiness of investment as compared to common law. They blame the outcome on excess formalism under civil as compared to common law (which increases transaction costs, fuelling Spiller's perspective). It results in slower judicial proceedings, lower consistency and fairness in decisions, as well as more corruption. The idea has now been turned into a legal origins theory which is leading a questioning of some of the original conclusions as discussed in the next section.

The broad public choice view. Just as in the case of the Laffont and Spiller views, incentives tend to matter in the public choice view, but its focus is mostly on politics and on the state internal organization. Political and bureaucratic biases and failures, common in infrastructure, have long been the concern of public choice theory in its various perspectives. They have also been central in the public administration literature. Politics, rent seeking, capture risks, internal organization of government are all part of the bread and butter of this view of infrastructure. Even if pure public choice research has not analysed many developing countries infrastructure case studies, many of its insights have influenced some of the other theories. They are often also essential to force reformers to think through essential process dimensions. This is particularly true in the analysis of regulatory capture and of the resulting failure of the sector to minimize the risks of massive inefficiencies and cost overruns. Most of the widely quoted evidence on infrastructure cost overruns has actually been produced by the public administration literature in its efforts to detail the bureaucratic failures identified by the public choice researchers (e.g. Flyvbjerg (2009, 2014) for recent overviews).

Another important contribution of this literature is to show that the specific dynamics of political parties matter. Partisan theory and pork barrel politics, in particular, argue that the relative importance of infrastructure may be used strategically to favour or penalize regions, cities or other parties. This explains why the level and /or quality of infrastructure at the country level or across regions can be impacted by changes in political majorities (e.g. Costa-i-Font (2003) for Mexico)).

The overall pragmatic lesson of this research directly relevant to the efforts to improve infrastructure performance may simply be that the importance of political institutions, rules, and practice is easy to underestimate. It also points to the fact that many of the institutional challenges of the sector are not in the sector. This, in turn, implies that first best solutions for the sector may be naïve and unsustainable if and when they ignore these higher level constraints.

The anthropological or "Ostrom" view. To economists, Ostrom may be the face of this non-economic view of the sector. With her followers, she has provided insights not initially dealt with in the economic literature, in particular in the context of rural infrastructures. With her co-authors, she also validated the importance of the internal governance of institutions already suggested in Laffont's concerns for the multiplicity of principals and agents in organizational designs as well as the concerns for the optimal organization of production argued in Spiller's visions. She was also among the first to point out to economic audiences that incentives for the delivery of key infrastructure services can be influenced by social norms and culture. Since then, the role of culture, norms and other values has enjoyed quite a broad recognition among economists (e.g. Alesina et al. (1999, 2003, 2015), Bardhan (2000), Bardhan and Ray (2006), Platteau (2000), Wade (2003)). As discussed later, these dimensions are currently revisited, and often validated, by behavioural economists relying on lab and field (quasi-)experiments (e.g. Torero and some of his colleagues at IFPRI).

But there is more to the impact anthropologists are having on institutions such as processes. The ethnographic interest in infrastructure—from public toilets to municipal water systems to roads—is relevant to the management of consultation for instance in the preparation of projects. The design of consultations and of the matching processes explain the growing feeling of entitlement in interactions with providers, public or private, but also with all the actors associated to the infrastructure production process. Harvey and Knox (2015, 2012) and Harvey and Knox (2011) focus on highway building in South America to show how large public infrastructural project implementations are driven by the degree and speed of state formation, by social relations, and politics. They show how local populations are concerned with the key choices, including routing, and with the distribution of benefits. This is where institutional strengthening starts when bottom up approaches are deemed necessary but this is not something economic policy tends to internalize enough. The main message of this approach is that processes matter to institutional building.

The "Acemoglu-Robinson-Rodrik (ARR) view". It is somewhat of a stretch to already include this political/historical perspective in the theories relevant to understand the institutional perspective on infrastructure since it has not yet really been tested for the sector. But it is important because it validates the importance of politics for the sector already identified by the Laffont and Spiller views when they look at capture risks in the sector. Laffont (2000) and Laffont-Tirole (1998, 1999), for instance, already discussed how rules shape incentives, including in regulation, through political processes.[12] What the ARR view may add is a longer term historical perspective on the relevance politics, including the relevance of the colonial past matters as well and more so than simply through the legal tradition. Acemoglu and Robinson (2005 and 2012) as well as Rodrik (2000, 2007) help focus on the issue, even if none of their papers/books actually looks at infrastructure institutions per se.

Their plea for a more systematic assessment of the role politics and political economy in explaining lasting market failures rings a bell in infrastructure. This is, indeed, a sector in which competition is easily limited or biased. Gomez-Ibanez (2006), for instance, had already been very effective at showing how the infrastructure reforms of the 1990s and early 2000s were all characterized by relatively easily identifiable winners and losers in a political sense. This is consistent with a more political diagnostic argued by ARR.

One of the interests in applying Acemoglu-Robinson (2012) to infrastructure is to take a view on the ranking of stakeholders in terms of their political (bargaining) power. The anecdotal evidence would suggest that for developing countries, the winners may have been foreign operators, the banks financing their investments and incumbent politicians when procurement and regulatory processes had been in place.[13] Users and taxpayers would be short term winners when growth in access rates, improvements in efficiency or cuts in subsidies requirements were delivered by the reform. However, renegotiations leading to higher user fees or increased subsidies would imply longer terms losses for, at least some of, these players. These losses are the outcome of their lack of power in sector specific property rights enforcements. They also reflect the capture of regulators supposed to control excesses by power-holders keen on maximizing rents. In their terminology, infrastructure performance has continued to be influenced by extractive institutions. And reforms ("the privatization cum competition revolution" as they could have called it) have failed to turn them in inclusive institutions able to rely on competition to minimize the

[12] Roughly, their view is that how much politicians care more about the service provider profit or the consumers depends on whether their constituency is dominated by pro-shareholder or pro-consumer voters. The details of regulation are then designed to hold public officials accountable to the dominating group rather than to society at large.

[13] Various authors have documented the growing concentration of market power in international markets by former national monopolies. See for instance, Bonardi (2004), Chari and Gupta (2008) or Clifton et al (2011).

scope for rents. These reforms seem to have transformed the old extractive institutions into new extractive institutions. Is it because the reforms were revolutions narrow based rather than broad based, Acemoglu and Robinson (2012) argue?

To be fair, the heterogeneity of the sub-sectors outcomes suggests that telecoms and part of the power sector (generation) have been able to benefit from increased competition to increase the number of local players while most of the activities that had to rely on competition for the market have not been very successful at this. It would be potentially useful to analyze with the tools of this theory why the foreign actors in this game have been able to replicate their ability to join the extractive group in a wider variety of countries. For instance, large water deals tend to be operated by a few numbers of French, German and American companies present on most of the bids organized by international organizations (Estache and Iimi (2011)). It would also be interesting to look with this analytical perspective into the reasons why the infrastructure reforms and policies have not always had the same impact on performance in infrastructure. And related, it would be useful to look at the extent to which the fact that different types of colonization policies created different sets of institutions can help explain the differences in impact of these infrastructure reforms in Francophone and Anglophone Africa.

The emerging behavioural view. This approach focuses on different forms of non-pecuniary motivation, also known as intrinsic motivation. The need to account for sources of non-rationality (e.g. anchoring, framing, endowment, present, hindsight, self-serving, loss aversion, status-quo, attribution biases) in the behaviour of infrastructure consumers, investors and operators in developing countries is now an increasing concern for regulators in the definition of rules and processes. It is also increasingly becoming policy relevant in some debates, showing the relevance of consumption and production alternatives. Moreover, it provides a better sense of the scope for nudging consumers and producers into social welfare enhancing behaviour (World Bank (2015)).

One of the main early contributions of the applied research in the field is evidence on the relevance of institutions on the incidence of social context and environment for key policy decisions. As pointed out by Ceriani et al. (2009), Clifton et al (2011) for European consumers for instance, preference heterogeneity matters to the optimal choice of public service provision and regulation. Similar evidence has been provided for the water sector in developing countries for instance in the context of the willingness to pay for water storage systems (Price et al. (2016)). This emphasizes the demand side of the market as a driver of the choice of institutions, including key processes in an environment in which the production and financing side tends to dominate the institutional choices. Social, cultural or other sources of differences all contribute to demand heterogeneity and this matters to public services just as much as it matters to many consumption goods. These sources thus need to be diagnosed as well.

The behavioural research evidence on social dimensions also shows that fairness matters to optimal consumption and production decision (Kahneman et al. (1986). This confirms similar conclusions reached by anthropologists (Larkin (2013) or Harvey and Knox (2015)). It adds that, to be fair, processes matter and are easy to underemphasize or underestimate in practice when implementing policies. This had also been identified in the Laffont view in any multiple principals, multiple agents settings (which are the norm in regulated industries (e.g. Laffont and Martimort (1998, 1999)). Frey et al. (2003) point to the importance of "procedural utility" in environments in which we care not only about outcomes but also about the procedures leading to those outcomes. Ultimately, much of this is making a strong case to empower consumers, as suggested by Clifton et al. (2011). In environments in which ethnical, tribal, religious or other historical drivers of social

interactions matter, this observation is particularly relevant, and yet largely ignored in the design of infrastructure policies.

This view thus argues for an explicit assessment of the extent to which all consumers and producers enjoy equivalent opportunities to make the most of reforms. This links back to the inclusive-exclusive dichotomization proposed by Acemoglu and Robinson (2012), to the diagnostic case made by Rodrik (2009) or by the winners and losers perspective emphasized by Gomez-Ibanez (2006) for infrastructure diagnostics. As pointed out by Clifton et al. (2011) in the case of consumers, social, cultural and economic environments all matter and yet tend to be ignored in the design of policy. This is increasingly well documented in the context of research on energy poverty in the UK for instance. The most vulnerable tend to be at a disadvantage in their ability to make the most of increased competition and the matching increased volume of information on options, for instance (George et al. (2011)). In sum, this perspective on institutional design suggests that empowerment, as implemented by default, may be biased and typically regressive from the consumers' viewpoint as discussed later.

All this shows that the assessment of non-pecuniary motivations is just as important in understanding the incentives to account for when trying to influence producers, consumers and financiers of the sector. They also impact selection and organization design. There is, as yet, no unified approach for studying these issues and our knowledge is quite fragmentary. However, examining these issues is now a part of the mainstream agenda in economics.

The empiricists view. This last view regroups the research that concentrates on running regressions, most of the time on panels, to test if the creation of a specific reform (e.g. the creation of a separate regulatory agency, the opening of a market to private operators) makes a difference or not. These are usually modelled as binary variables (yes vs. no) and the outcomes are measured in one or more of few performance indicators for which long enough time series are available. Typically, the quantity variables focus on access rates and network size. The quality variables tend to be approximated by technical measures (e.g. service interruption, safety incidents, maintenance costs). There are many papers focusing on various types of efficiency measures (e.g. allocative, technical, technological) but many of these actually measure labor productivity as a proxy because there is little reliable data on capital and other inputs to do better.

The first generation of papers suffered from major endogeneity issues (i.e. the omitted or mis-measurement of variables identified by some of the theories). Many of the papers ignored cream skimming issues in the design of policies when relying on biased samples, erroneously assumed to be random. This is often what allowed a focus on the positive impacts of policies such as privatization. It ignored the "collateral damage" not picked up in the sample. For instance, looking at the impact of reform in a city is not the same as looking at the impact of that reform for a region in which rural areas are losing financing or access to scale and scope economies. Finally, it tends to ignore the many interactions between the various characterizations of infrastructure institutions. This is quite problematic since the two dimensions may be complement in some setting and substitutes in others. [14]This is why estimates of the academic assessment of the relevance of institutions is often seen by practitioners as at best biased, at worse irrelevant.

Despite the limitations due biased representations of the pure economic trade-offs between efficiency, equity and/of financial viability of a policy, many of the contributions tend to be quite useful. The correlations they document may not be directly useful in policy

[14] A typical example is the debate on the extent to which contracts are regulatory instruments needed by regulatory agencies or whether contracts and agencies are substitutes.

evaluations but help identify additional research interests to settle the debates. Much of the empirical research in this field has been produced by international organizations staff or financed by them because of their privileged access to the required data—by the way, why is this data so often not public? And as data and techniques improve, the level of uncertainty on the predictability of reform effects seems to be increasing. This implies that we may be have been overconfident on outcomes from institutional reforms until not too long ago. One of the main lessons of the most recent papers is that initial conditions matter a lot more than the early empirical papers assumed. And this is hard to pick up well multi-country multi-year data panels. This is, in turn, makes the case for relying on case studies to complement or validate the stories emerging from the treatment of panels.

This is, partially, why looking at case studies through impact evaluations has become popular but, at least in the context of infrastructure, it has its own set of problems. The first is that most of the research related on infrastructure focuses on the water and sanitation sector. In the 2015 3i database of impact evaluations, infrastructure only claims 258 of them, that's about 6% of the total, half for water and sanitation and about a quarter for ICT. There are only 10 covering the transport sector. The second is that most of these evaluations are about targeting and tools, not about institutional designs.

The interest in behavioural approaches is now leading to new evaluations to cover characteristics such as norms, culture and similar dimensions linked to institutional constraints and opportunities. But we are only at the beginning of the use of this new approach. A lot more is needed to test and validate some of the institutional characteristics the various theoretical reviews have identified as relevant in principle.

Summing up. The preceding overview of academic contributions leads to list of dimensions that should be expected to be of some relevance in diagnostics of institutional quality of the sector. This list is the first column in Table 1 which is also an attempt to give some credit to the various theories and their empirical support, for their contribution to this list in the context of infrastructure. [15]

Table 1: (Rough) Overview of the Main Institutional Dimensions Picked Up by the various theoretical visions

	Laffont (Agency)	*Spiller (Transaction costs)*	*Shleifer (Legal)*	*Public Choice*	*Ostrom (Anthrop.)*	*Behavioral*	*ARR (History)*	*Empiricists*
	Primary focus							
Ownership	X	X	X	X	X	X	X	X
De-and re-regulation	X	X	X	X			X	X
Accountable regulatory autonomy	X	X		X	X			X
Contract and regulatory design	X	X		X	X	X		X
Politics	X	X	X	X	X	X	X	X
	Other focuses							
Enforcement	X	X	X	X	X			
Internal governance of agencies and of government	X	X		X	X	X	X	X
Processes	X	X	X					
Technical skills and staffing	X			X	X	X		
Monitoring organization	X			X	X			X
Financing of institutions	X	X		X	X	X		X
Legal norms		X	X	X			X	X
Non-pecuniary norms	X				X	X	X	X
History			X		X		X	

[15] I am well aware that this table is a very personal interpretation of the contributions of each theory. Some readers are likely to disagree with my attributions but its main purpose is to highlight the perceptions of a dedicated follower of research and practice unable to review every single paper, book or fact.

Three observations stand out. First, not a single theory covers every dimension, not even the empiricist approach even if many empirical papers are not married to a single view of institutions in infrastructure and use all of them as an input in the choice of explanatory variables when assessing sector performance. The second is that almost all theories have a view on the relevance of ownership, de- and re-regulation, politics and on the internal organization of institutions (e.g. bureaucratic organizations, board nomination and composition, …) or of the state (i.e. decentralization and inter-ministry and inter-agency coordination). This suggests that these items, covered in common country diagnostics, are indeed necessary. The third is that most of the gaps in coverage concern the relevance of processes, accountability, staff skills, non-pecuniary norms and history. Most, if not all, of these dimensions tend to be influenced by country characteristics rather than by sector specific characteristics. Ignoring legal and cultural processes and norms as drivers of incentives is essentially ignoring internal governance issues, and underestimating the administrative and financing feasibility of institutional changes. And yet the evidence suggests that this is part of the break or make of institutional reforms as they define the equivalent of the growth diagnostic bottlenecks.

What the table does not show is that too much of the evidence and theoretical analysis is often stuck in "ceteris paribus" assumptions. This comes out more often in case studies (e.g. Gomez-Ibanez (2006)) and country specific diagnostics. Ignoring what is hidden in that assumption is what makes it so difficult to come with a predictable sign on the effects of reforms on key performance indicators as discussed in the next section.

4. On the empirical evidence of the relevance of institutions in infrastructure

Many casual observers of policy debates in the sector may believe that the main academic research on institutional options in the sector are about deregulation, privatization and the creation of independent regulatory agencies since it has been a key concern for most of the theoretical and dogmatic debate on the sector. The outcome of the review of the research conducted in the last 10 years or so suggests otherwise. What emerges, instead, is a sense that many empirical researchers have been able to synthesize at least part of the various theoretical approaches. It feels as if each research team had come up with its own set of relevant institutional dimensions. The outcome is an impressive heterogeneity of partial sets of institutional characteristics credited with being performance drivers in infrastructure. The following is an (heroic?) attempt at reporting the main dimensions covered and the main lessons learned on each dimension.

Market structure and deregulation. The early 1990s research and policy discussions documented the potential payoff of unbundling the vertically integrated historical operators of the sector. Unbundling was an opportunity to increase competition and where competition in the market was limited, it was also an opportunity to introduce competition for the market at a time when auction theory was becoming a popular tool among policymakers in OECD countries. Competition in the market and for the market both implied the need to open the business to private providers. The reviews of experiences suggest that competition tended to be good most of the time as it opened the sector to new technologies and new sources of financing (e.g. Zhang et (2008), Besant-Jones and Vagliasindi (2013)) or Jasmab et al. (2015) for recent overviews for the energy sector, Berg et al (2011, 2012) for the water and sanitation sector in general or Mande Bafua (2015) in the African water context and Bel et al (2015), Beuran et al. (2015), Soomro and Zhang (2011), Percoco (2014) or Raballand et al. (2010) for the transport sector).

The enthusiasm for competition for the market has been toned down somewhat, however, as a result of a more careful monitoring of a few variables. The first is the

accumulation of experiences in which the risks perceptions may have been underestimated by market designers. In environments in which the ability to pay is limited and the financing costly, competition can be excessive if it fuels the risks perception levels, in particular when alternative financing options from donors are available. Higher risks imply a higher cost of capital which in turns implies a higher average tariff. This is usually a politically difficult outcome in general, and more so in poor countries.[16] One option is to limit competition to ensure some minimum market size to the operators or to reduce the degree of unbundling. This is what happened in Cameroun, for instance, where after a first failed attempt at relying on competition in the market by unbundling the electricity sector, the country decided to try again with a vertically integrated firm. The next attempt managed to attract foreign operators and most importantly foreign investors. A second limitation of market restructuring is linked to cherry picking by operators. Even when required to deliver full coverage of population, many private operators tend to leave high costs customers for last in their investment plans. This explains why progress has been much lower in rural or peri-urban areas than in other areas. Finally, as suggested by Vagliasindi and Besant-Jones (2013) for the energy sector, there is evidence a threshold system size and per capita income level below which unbundling of the power supply chain is not expected to be worthwhile. In other words, unbundling may be a bad idea sometimes.

Over time, both academics and policy advisors realized that there was a real case to rely on alternative small scale suppliers such as cooperatives or alternative local technologies providers. Torero (2015), for instance, shows that in rural areas where most of the so-far non-electrified population live, because demand is very low, it makes sense to limit electrification through grid extension programs and to consider off-grid dissemination programs. In other recent research (at least in the context of infrastructure), the scope for community or NGO managed supply has started to enjoy supporting conceptual and empirical evidence (e.g. Benett and Iossa (2010)). This research shows that there are many conditions under which supply by not-for-profit providers can lead to greater investment and hence coverage than poorly regulated private provision focusing on profit.

The main point to retain may be that alternative institutional arrangements (e.g. small scale operators, NGOs, …) should be seen systematically as relevant options of sector design and planning. They are often pragmatic options to diversify risks and may speed up access. This is particularly important when large size investments are too slow to be able to speed up access rates.[17] But for institutional option menus to account for the various constraints, detailed regulatory dimensions such as service obligations and limitations to service exclusivity need to be addressed. Competition from alternative providers has to be considered, just as the complementarity between these providers and classical providers may be useful. In practice, developing this menu of options is quite feasible and depending on the specific constraints and concerns to deal with, it is usually possible to come up with hybrid solutions in which large scale producers are instructed to work with small scale providers or less formal providers to ensure timely delivery of services. Some of the Spanish (Union Fenosa) or American (AES) electricity companies have been quite effective at these mixed institutional solutions to ensure improved rural coverage in Latin America. But this requires a willingness not to rely on pre-packaged market design solutions.

[16] Vagliasindi and Besant-Jones (2013), in the most exhaustive quantitative diagnostic of reforms in the electricity sector so far find that, between 1999 and 2009, a higher private sector participation share significantly raises the level of tariffs to be able to attract private participation in distribution.

[17] A common argument to prefer large scale operators is their decreasing average costs and their low marginal costs. This is fine in a static view of the world. When time becomes pressing, the rate of time preference matters and so does the discount rate. For villages having to wait for 10 years to get access to the services offered by a large utility, it is not necessarily the case that the cost-benefit analysis will favour the large scale lowest cost but slow operator over the small scale higher cost but fast operator.

The role and design of procurement is a subtle but essential complement to this discussion of deregulation and regulation. Competition for the market is one of the key sources of competition in the infrastructure sector and in developing countries, it is largely dominated by rules defined by the international organizations. These rules are widely seen as being constraining for many of the payoffs expected from better auction designs aiming at developing more effectively local players in the development, delivery and operations of infrastructure (see for instance Engineers against Poverty (2006) for an operational perspective and Estache and Iimi (2011) for estimates of the cost of constraining procurement and the payoffs to various potential changes in practice in developing countries). For instance, encouraging (local and foreign) fringe bidders to actively participate in the bidding process while maintaining the quality of the projects can cut costs (and hence average tariffs) significantly.

Today, some of these potential players are excluded from common procurement practice. They eventually participate in service delivery through subcontracting, but this simply increases the profit margin of the bids' winners, without benefiting the users or taxpayers. Similarly, the division of large contracts into smaller contracts or contract lots is an important policy choice for auctioneers to cut costs. Deciding how far to go on this requires a much better (yet feasible) understanding of costs drivers (scale and scope) than is currently accounted for in standard procurement practices.

The evidence shows that, in general, there is a tradeoff between competition in auctions and size of contracts. Larger projects could benefit from scale and scope economies, but large contracts may undermine competition. Bidder entry is actually endogenous because it is determined by the auctioneer's bundling and unbundling strategy. If ease of auction justifies water treatment plant and distribution network works bundling in a single lot package, it comes at a price if it raises public procurement costs of infrastructure because of a lack of competition.

The evidence also shows that developing countries have a limited capacity to properly negotiate the allocation of risks in infrastructure. Blanc-Brude and Makovsek (2014) have been able to put together a unique data set that reveals that project sponsors almost completely avoid construction risk when comprehensive risk transfer can be achieved through credible contracts. This is probably a good news to increase the prospects of private participation, but it also implies that someone else is picking up the risk. Typically, the users or the taxpayers do when procurement contracts are not negotiated to ensure a reasonable risk allocation.

Given the importance of activities which are going to continue to be subject to these procurement practices, any effort to do better in the sector needs to factor procurement reform in its agenda, with or without international organizations. It really boils down to this: the better use of auction design options would cut total infrastructure development procurement costs in the developing world by at least 8-10% (Estache and Iimi (2011)). Just to put the relevance of this matter in context, it may be worth to emphasize that this concerns at least 80% of the expenditure of the sector which represent over 10-15% of GDP in the poorest countries and about 6-8% in the middle income developing countries.

Public vs private. The public and the private sector are complementary and the degree of complementarity depends on a number of market specific characteristics typically linked to demand, supply and financing risk factors. This is why there is a continuum of options available on the scope for public-private partnerships (PPPs) which can unbundle to various degrees finance, construction, and operation into separate long-

term contracts with private firms and focus on the sharing of risks between the investors, the users and the taxpayers.[18]

The debate on the case for increasing the private sector role in infrastructure may be one of the oldest. It has been divisive, characterized by often excessively polarizing views rather pragmatic ones. It is still not settled because as it is often the case, conclusions are the results of incomplete assessments. In their meta-analysis of the water and solid waste sectors in mostly OECD countries, Bel et al. (2009) offer a useful illustration of the difficulty of arguing that privatization leads to lower costs on average. They show that the conclusion does not hold up when they control for sample size, publication bias (papers obtaining significant cost savings are more likely to be published), timing bias (cost differences are less likely in more recent studies), service characteristics and geographic area. Most of these factors are just as likely to be relevant in the assessment of privatization in developing countries.

A lot of research on PPPs in developing countries also recognizes the importance of the risk allocation between the partners. This is an essential component at the deal level. It is also important when considering the total risks level picked up with the government since it can add up to a significant fiscal risk. And there are many other dimensions which may not have been picked up so far well enough in the evaluation of the choices relevant to the assessment of the scope for institutional strengthening of the sector.

The discussion on institutional design options has been biased by (at least) five weaknesses in the evidence produced. First, a lot of the literature has been tainted by *selection biases* in the subsectors used to represent infrastructure. This matters because focusing on the most successful sector (telecoms) leads to overestimation of the potential of a policy while focusing on the least successful (water) may lead to underestimation of this potential. As mentioned earlier, the telecoms sector may have been the most common illustration for the claim of superiority of the private sector even if it is not representative of the typical infrastructure market structure. In telecoms, the private sector has indeed been quite effective at improving efficiency and access to services but largely because of technological innovation combined with reasonably effective competition policies. The same cannot be said of the other subsectors.

The evidence suggests that the switch to the private sector is often, even not always, a good strategy to improve efficiency in the short run, much less so in the long run when regulation is weak. In practice, in the short run, it usually functions like shock therapy because it breaks historical political, business, tribal, family or labor privileges inherited from long term interactions full of inertia.[19] The evidence also shows that it is often the result of improvements in labor productivity, one of the main criticism of the International Labor Organization (1998, 2003) for instance because it is happening in labor abundant countries with underemployment issues.[20] A 1990s referendum in Uruguay suggested that, in that country, consumers preferred to pay 30% more for their water to maintain the overstaffing of the sector.

[18] Saussier (2015) is an impressive collection of surveys of each of the key dimensions on PPP. The current version of the book is in French, but a translation in English is due soon. An additional extremely useful source if the PPIAF sponsored web site body of knowledge which is essetnally an encyclopedia updated on a regular basis (http://regulationbodyofknowledge.org/)

[19] The privatization of the Argentinean transmission electricity company raised on a number of conceptual issues since transmission is the natural regulator of the sector but it had been captured by complex political interests and its privatization was a way of cleaning its board from interference with the proper operational decisions on investment and maintenance.

[20] The fact that many of the studies assessing the improvements in efficiency linked to privatization or tend to focus on labor productivity as a proxy for efficiency is quite revealing.

A second, somewhat related bias is the common practice of restructuring markets to make them more attractive to the private investors. This repackaging is designed to allow "cream-skimming" or "cherry picking". While this approach is often successful at attracting private financing and operations, it ignores the residual negative fiscal consequences since the public sector can no longer rely on intra-sectoral cross subsidies (Estache and Wren-Lewis (2009)). Yet, historically, these cross-subsidies have been particularly important to be able to finance progress in high costs and rural areas for instance. This is not picked up by most of the empirical modelling of reforms even it has been highlighted by the conceptual research (e.g. Laffont (2005)). Most empirical papers simply look at whether the private sector enters or not, but ignores the fiscal consequences and the rationing it implies in the ability of the public sector to finance sector specific and other expenditures. This is a common mistake in the modelling of the effects of water privatization for instance when urban needs are unbundled from rural needs as was done in various Argentinean provinces. Privatization can look good when it can focus on the low cost, low risk business segments.

A third common bias is the fact that the complementarity between PPPs options and regulation as well as other more specific characteristics (e.g. market size, market design, governance quality) are not systematically picked up. When researchers do account for it, they show that poor regulation with PPP can be counterproductive (e.g. Mande (2015) for the water privatization experience in Africa or Vagliasindi and Besant-Jones (2013) for electricity in general). Indeed, where regulation has been weak (which is the norm in developing countries), access improvements associated with privatization have been slower than expected or sometimes not statistically different from those achieved with public provision. When the importance of regulation and competition is poorly approximated or ignored in the interpretation of the results on privatization, it tends to overestimate the PPP payoffs. Laffont et al. (2002) documented this early when they emphasized the importance of government and regulatory commitment capacity for the effectiveness of reforms. And it has been more recently extended in the context of an assessment of the importance of regulatory credibility by Iossa and Martimort (2015).

The fourth bias results from a lack of precise data on the relevant institutional characteristics. The difficulty of measuring institutions emphasized by Glaeser et al. (2004) and Woodruff (2006) for instance is just as common in infrastructure as in other policy areas. Standard databases measure institutional quality in infrastructure such as risk of expropriation, government effectiveness and constraints on policy design. But these can be 'outcomes' of broader institutional and political dimensions rather than sector specific institutional characteristics driving performance. Many studies fail account for the fact that there is a continuum in the PPPs since most model the presence of the private sector as a binary event and many more ignore the relevance of informal institutions that make contracts enforceable or not. The problem with this bias is that it is not clear whether it underestimates or overestimates the role of the private sector.

A final bias is the omission of the relevance of politics in the process that leads to the decision to increase the scope for private sector involvement in public service. This is has been a recurrent theme in the analysis of regulated public services (e.g. Laffont (2000) or Spiller (1990)). Ignoring the fact that politics plays a role which can be both positive (when it leads to pragmatic decisions accounting for complex contexts) or negative (when it is associated with capture (e.g Sheely (2015) or clientelism (e.g. Wantcheckon and Vicente (2009)) is at best naïve. When considered in some detail, the evidence on its impact is usually quite revealing of the diversity of its impacts. For instance, Blimpo et al. (2013) find support for the argument that political factors affect the location of roads after controlling for the economic importance of the areas, as well as many other factors. Politically marginalised areas have significantly fewer roads and this affects basic needs such as food

security. In sum, ignoring politics can lead to a mis-estimation of the impact of the role of private participations (see also Bel et al. (2015) for a series of OECD case studies of local public services). This is not new (Bergara et al. (1998), Heinisz (2002)) but it has taken some time to trickle down to mainstream economic assessments of PPPs and regulation.

Many of the biases in econometric results could be fixed by unbundling some institutional proxies to refine the dimensions accounted. This is exactly what more recent studies try to do (e.g. Vagliasindi (2013)). But it is not straightforward. The most recent studies have done a better job at taking this into account but it is hard to be as complete as necessary. Vagliasindi and Besant-Jones (2013) show for the energy sector that the market size and design matters to the impact of the introduction of the private sector. When the systems are too small, they find that an increased private participation can be counterproductive. But they also suggest that other variables may be important.

Ultimately, the evidence suggests that the combination of deregulation, re-regulation and privatization has often delivered costs reductions (and other efficiency gains) but: (i) it has never been a guaranteed success, (ii) it has often been counterproductive in terms of performance, (iii) it has often benefited more operators, investors and taxpayers than users, and (iv) one size does not fit all. Most of these are the outcome of political choices or incompetence. When they are choices, there is not much that can be done besides making sure the voters understand. But when they are the result of incompetence, there is scope for intervention. We know now that the water sector, and to a lesser extent in the transport sector, ownership switches has had a hard time delivering as much as expected under most of the contract types that have been tried. This is so much so that, between 2000 and 2016, many toll roads have been renationalized, water companies are now increasingly relying on management contracts and many small private ports and airports are not doing any better than their public counterparts. Under all scenarios, it should also be clear to all interested parties that getting it wrong also has equity consequences. The evidence on the social costs of poorly regulated privatizations in many of the poorer countries is quite strong (e.g. Trillas and Gianandrea (2007) and Calderon and Serven (2014)).[21] And this is something that can be managed better.

In sum, privatization without sound regulation has not been a success story and is increasingly being rejected by policymakers unconvinced of their ability to regulate properly. With sound, transparent and focused regulation, it can turn into a powerful and useful instrument in the interest of users, taxpayers and investors happy with a return matching their costs of capital for the long run, rather than excessively focused on rents and high short term dividends.

With or without separate regulatory agencies. The debate on regulatory agencies started with the debate on privatization (see Gomez-Ibanez (2006) and Castaneda et al (2014) for complementary discussions). In addition to PPPs and a matching adequate regulation, a common institutional reform is the assignment of the regulatory function to a separate autonomous institution. By unbundling and isolating regulation from other government interventions, the reformers expect to send a signal to potential investors demonstrating their commitment to minimize the risks of political interference in the management of the sector.[22]

Under this approach, the newly created agencies take over from the ministries the main regulatory functions (e.g. contract enforcement, tariff, cost and/or quality monitoring,

[21] Note that these conclusions are quite robust to the contract type used to privatize since they have been observed under concessions contracts, asset ownership switches or more general targeted contracts (e.g. build-operate-transfer (BOT) and equivalent).

[22] Bardhan (2009) looks at the decentralization of public services with similar concern for both autonomy and accountability.

fining). In developing countries in particular, their margin for discretionary decisions is often limited because most of the regulatory rules are usually specified in a contract and sometimes in a matching sector law. The re-politization of regulation in that context often results from the incompleteness of the contract. This incompleteness in turn is often the result of their excessive standardization. Contracts are indeed too often transferred from one country to another with as much adaptation as seen fit but this process often leaves gaps and sometimes incoherence. The process saves time and money but implies costs when conflicts arise. And these conflicts are common.

Guasch et al. (2014) remind us that 68% of the PPP contracts are renegotiated within a year (87% for water contracts and 78% for transport contracts vs 41% for electricity contracts). The monetary costs of these renegotiations are often linked to the need to mobilize experts. This boils down to outsourcing institutional capacity. The non-monetary costs are linked to the risk of political interference in regulation from all donors, including foreign governments supporting the interests of the operators exposed in conflicts. The French and Spanish governments were quite active during the Argentinean 2001 crisis for instance and they had already been active in project specific conflicts. The French government has traditionally been quite interactive with African governments when French firms were at risks and this has trickled down to all regulatory conflicts.

The real issue with respect to autonomous regulatory agencies is, thus, the extent to which they are really independent from political interference. The odds of being independent from political interference are higher when they can rely on their own sources of funding (i.e. regulatory fees), when they are accountable to the parliament rather than to the ministries or when the commissioners are named to minimize the risk of capture by politicians or by operators. The evidence available suggests that increasing the financial autonomy of regulators increases the odds of a good performance of the sector for a given level of accountability (e.g. Cambini et al. (2014)). And increasing accountability further improves performance. Reducing political interventions minimizes the risks of capture of efficiency gains achieved in the sector.

Another omission is linked to the lip-service often paid to technical or human capital limitations (Estache and Wren-Lewis (2009)). The technical limitations result from the adoption of the western idea of independent agencies by countries without the matching accounting and process tradition that allows the supervision of the regulated firms. Regulatory and cost accounting continue sources of tensions between regulated firms who want to maintain as much as possible of the information asymmetry they enjoy from the lack of accounting tradition. And for regulators keen on reducing the information asymmetry, two issues often arise. The first is that it takes time to build up the financial, accounting and technical tools needed to assess the performance of the operators. As long as the institutions do not have the right tools, their ability and credibility are limited. In practice, this issue is often solved by outsourcing regulatory work (O'Rourke (2003)). There is indeed a solid consulting industry helping countries, set tariffs, measuring efficiency or implementing the most technical dimensions of regulation (both in developed and developing countries). The second issue is that enthusiasm about their desire to increased performance transparency may result in a temptation to micro-manage. And this is just as much of an issue, in particular when the operational and financial expertise is limited in the regulatory agency.

The concern for corruption, capture or collusion is another margin in deciding on the desirability of an autonomous agency as well as on the specific form of regulation to adopt. This is quite a well-documented concern when considering the design of institutions and their rules (Dal Bo and Rossi (2007), Auriol and Blanc (2009), Auriol and Straub (2011), Seim and Soreide (2009), Soreide and Rose-Ackerman and Soreide and Williams (2014),

OECD (2015)). It is also a serious concern when considering the acceptability of institutional reforms. For Latin America, Martimort and Straub (2009) show that the degree of corruption that prevails in a society responds to changes in the ownership structure of major public service providers. In cases in which privatization opens the door to new corruption (say through opaque procurement processes or regulatory decisions), public dissatisfaction may increase to the point of rejection, even though the reforms can be credited with positive effects on access.

Despite these limitations, most countries today have adopted some form of autonomous regulatory agency. Most count now on an energy regulator that is no longer a unit of a ministry for the energy sector. There are fewer water specific regulators although there are many multi-sector regulators which cover both water and energy and many of the those cases, introducing a regulator did not do much (Berg (2013)). Some countries have also created transport regulators, but when intermodal competition is strong enough, competition agencies tend to monitor what is happening in the sector. There are, however, units in charge of the supervision of toll roads, airport, rail or port concessions in countries opened to private operators or running public operators on a commercial basis.

The debate on the ability of current institutional designs is quite intense in transport these days in view of the growing evidence of the mis-management of traffic and safety across subsectors in both facilities and services around the world. In a recent book on PPPs, Engel, Fisher and Galetovic (2014) recommend to unbundle institutions in transport to match the various stages of transport service production process. They argue that one of the problems of the sector is that all the stages of PPP contracts are governed by agencies inside the same ministry (typically, the Public Works or Transport Ministry). They suggest that this centralized institutional arrangement should be replaced by at least three agencies as independent from government as possible: one to select the project (this is about procurement), another to enforce the contract (this includes monitoring) and a panel of experts to adjudicate controversies in renegotiations (this is about dealing with the consequences of incompleteness in contracts).

In general, the econometric evidence suggests that these regulatory agencies have a positive impact on access at least (Andres et al. (2013), Cubbin and Stern (2006), Guasch et al (2012), Gassner et al (2014), Vagliasindi (2013) or Mande Bafua (2015)). More generally, the mere existence of an autonomous regulator is correlated with improvements in the overall performance of the sector in particular. The impact is stronger when combined with the possibility of entry by private operators to compete for the temporary right to operate a firm or a sector. Note that the modelling of the importance of agencies is usually done as binary variable (agency yes/no) and it should be clear by now that this is quite imperfect, since there is a wide range of possibilities in terms of the degree of financial, legal and political autonomy that an agency can enjoy. This is a recurring theme which has been hard to tackle in practice (e.g. Trillas and Montoya (2009), Trillas (2010) or Eberhard (2007)).

In concluding this quick overview of the role of agencies, it is important to add that the existence of an agency is not a necessary condition for performance improvements. Independent regulation are often a useful option, but the credibility and legitimacy of regulation depend just as much on the tools and the transparency of the use of the tools and of the decisions. Hybrid models that combine traditional visions with more modern approaches can be better to match the local country context, including in terms of governance and capacity. Improvements in regulated sectors can be achieved *without* creating a new agency when the business to supervise is specific or narrow enough.

In some cases, as for single railway deals in West Africa, small units within the Ministries of Transport have done just as well as any agency would have done, since the

main content of the job is to supervise compliance with a fairly detailed contract. In other cases as in the restructuring of the Rumanian Water sector, the local institutional weaknesses have been compensated by the nomination of a panel of external foreign experts conducting scheduled regulatory audits. In addition to these options typically managed by the authorities, there are other options which give a voice to consumers organized or not. Public hearings or consultations are now part of the institutional tool kit of many regulators.

When concerns for independence are serious, NGOs can have a significant monitoring and voice role, although the challenge is to define the limit between a monitoring/watchdog role and the advocacy/activist role that some of these organizations also aim at. These not-for-profit organizations can have a role as watchdogs (Aldashev et al. (2015). The punishment inflicted by the NGO if the misbehavior of the firm is detected can take the form of active interference with the production process (organizing worker revolts or destroying some parts of the firm's production lines), which implies that the firm has to spend resources for continuing to produce normally. This is somewhat different from the channel of influence of Baron and Diermeier (2007), where NGO conducts boycotts or reputation-damaging activism. For the sake of simplicity, we assume that the NGO campaign against the misbehaving firm has a sufficiently strong effect to serve as a credible threat for the firm (Baron, 2010).

The implicit, quite rational, philosophy in these alternative solutions is to outsource key institutional responsibilities around clear contractual and "monitorable" specifications of commitments made by all parties. This achieves the expected accountability in environments in which it is going to be hard to achieve independence. It minimizes the risks of negative consequences associated with excessive discretionary powers in an environment in which these powers are not needed and local capacity to exercise them without interference limited (e.g. Bitran et al. (2013) and Guasch (2014)). As Spiller et al. (2015) put it, the idea is to find a way to minimize the transaction costs associated with regulation. And often, the simplest accountable system is the best one because it is also the most pragmatic solution in complex institutional environments.

With more or less decentralization and other local participation mechanisms. The unbundling of policy design, mandates, expenditure responsibilities and regulatory functions to increase local responsibilities and accountability is an additional instrument and there is evidence that it can pay off. For instance, Kis-Katos and Sjahrir (2014) find that, when expenditure decentralization in Indonesia created two new layers of subnational governments, investments in public infrastructures in districts with little initial infrastructure increased. Pal and Wahhaj (2012) had already shown that this decentralisation had led to an increase in the share of spending on physical infrastructure, as well as a convergence in spending across communities with different types of local institutions. This payoff is often credited to the increased accountability of government from a closer leverage of citizens on their politicians (Bardhan and Mookerjee (2006)).

This is also related to the growing literature on community-based monitoring and on the pay-offs from increased empowering of local communities over their service providers (see Mansuri and Rao (2013) for a recent survey). This is largely about the payoffs to empowerment when interactions with politicians and operators take place on a repeated basis. In that perspective, empowerment makes it easy for consumers and voters to use access to new information.

Empowerment through better access to information in the sector in which projects costs are often quite large is particularly effective to minimize local elite capture. This is also true in democratic environments and is typically anchored in national audits (notably as part of anti-corruption programs) targeting the risks of local elite capture. For Brazil,

Ferraz and Finan (2008, 2011) show that providing information improved outcomes. Moreover, increasing the odds of audits reduced the rents captured by local officials. For Bolivia, Yanez-Pagans and Machicado-Salas (2014) showed that the central role of grassroots organizations in increasing access to information allows reductions in bureaucratic delays within the allocation of small infrastructure projects.

Thinking of decentralization broadly defined as an additional institutional instrument is thus a realistic option, but as in all of the institutional reforms reviewed so far, it does not guarantee success. For instance, Olken (2007, 2010) finds that grassroots participation in monitoring of a village road construction program in Indonesia had little average impact but that monitoring by an external agency makes a difference (i.e. independent audits equivalent to those done by an independent regulator helps). Casey et al. (2012) find similar results, looking at an infrastructure project in Sierra Leone involving both relatively large grants and the application of processes to enhance local empowerment and participatory governance in the planning and implementation phases.

There are many explanations for the dispersion of outcomes from this institutional approach. For instance, it can depend on the homogeneity of populations. A strong ethnic diversity at the local level worsens public good provision. This is because this diversity makes social sanctions become more difficult to enforce across different ethnic groups (e.g. Miguel and Gugerty 2005). Ethnic or social heterogeneity may also favour ethnically or socially biased targeting. Chattopadhyay et al. (2005) showed that in India, lower-caste areas receive more local public infrastructures when the village leaders are lower-caste politicians. There is also a risk local elite capture of local public goods associated with decentralization. And in this context, Alatas et al. (2013) document an Indonesian case study in which they find that local officials seemed to capture some transfers in villages where transfers are large relative to private consumption, although the rent produced is less than 1% of the welfare produced by the social programs. This is linked to the relevance of the design of intergovernmental transfers for the performance of the sector (e.g. Frank and Martinez-Vazquez (2014) or Goel and Saunoris (2016)).

The economic incentives built in transfer design are well understood in general and in particular with respect to the operation and financing of infrastructure services. The relevance of the importance of political incentives emphasized by several of the theories discussed in section 2 also impacts the effectiveness of decentralization of infrastructure mandates when these mandates are shared across government levels rather than assigned to a single government level. This is quite common in the water sector, for instance, where provision can be local but sanitation, because of the spillover effects, has to be coordinated at the regional or national level. For instance, Estache et al. (2016) show for Brazil, where sanitation supervision is split between the state and the municipal governments, that water treatment efforts are sensitive to political alignment across government levels. When elections led to mis-alignments of political affiliations of authorities across government levels, they also caused a deterioration of water treatment. The main point here is that in democratic systems, the implementation of shared mandates forces a pragmatic look at the desire to increase local participation in all stages of the production chain of a public service in environments in which political mis-alignments are recurring outcomes of democratic processes. Delegating to maximize local involvement may end up being counterproductive simply because politics matter to efforts to collaborate.

Before concluding the discussion of decentralization, it is important to also point out that there are many very practical technical details that can easily be a source of trouble in infrastructure decentralization. For instance, urban planning rules that differ across municipalities can make it difficult to deploy telecom infrastructure (ducts, and trenches, or antennas).There are many more instances in which local governments can block

activities decided by national agencies. This can be solved in theory by minimizing the delegation of activities with spillovers. Bu this ignores that local permits requirements for any type of construction work essentially gives a veto right on any project including those assigned to higher government levels.

In sum, an increased role for local actors in the institutional design of infrastructure policymaking, design, implementation and monitoring is certainly yet another good idea. But as for all institutional options, it would be a mistake to ignore its limitations. And there are many such limitations, including in particular, the increased complexity linked to local politics in environments in which they interact with an ethnic context, uncoordinated elections shorten decisions cycles and political diversity can create intergovernmental tensions leading to the sector's mis-management.

Civil law vs common law. The implications of the differences between the two main approaches to law have long ruled the advice on how to improve the investment climate (and hence the scope for improvements in the access to private financing of improvements in access rates to infrastructure services). Recently, however, the empirical foundation of this policy vision has started to be questioned. The main criticism stems from the excessive dichotomization in the modelling of the legal institutions. Guerriero (2011a, 2011b, 2015, 2016) has recently produced infrastructure specific evidence documenting the relevance of the endogeneity of the legal system in environments with weak political institutions. He shows that it impacts the trade-offs between the flexibility of judges (or regulators) in common law and the rigidity of decisions under civil law. It turns out that in very uncertain environments with weak governance, civil law may lead to more efficient outcomes. The same can be concluded if preferences are homogeneous rather than heterogeneous.

Clearly, the jury is still out on the actual relevance of the legal tradition for outcomes simply because there seems to be much less inertia in the practice of law than suggested by the early diagnostics. The debate is however essential to developing countries having to assess the optimal choice of contract and regulatory design in infrastructure. For now, we know that PPPs have resulted in hybrid legal environments in which incomplete contracts (e.g. *contrats d'affermage*) inspired by civil laws traditions are assigned to regulators with mandates and power inspired by common law tradition (e.g. *concession contracts*), rather than to a higher tribunal (e.g .Conseil d'Etat in France or the Tribunal de Cuentas in Spain).

A lot of anecdotal evidence suggests that the combination is risky. In too many cases, PPP contracts signed in Africa, Asia, Latin America or Eastern Europe during the transition, were simple adaptations of American, British, French, German or Spanish contracts, with at least some incompatibility with local legal practice and enforcement willingness and capacity. For instance, the large number of contract renegotiations or cancellations in Latin America or Africa can partially be blamed on an imperfect reconciliation of the legal approach of investors with strong bargaining power and the local legal approaches (e.g. the common vs civil law practice). They are linked to the difficulty of ensuring a robust matching between the new types of contracts and local preferences and capacities (e.g. Albalate, D. et al. (2015)). Too many of the matching efforts are based on extrapolated data because there is no systematic assessment that accounts for the endogeneity of the legal norms in infrastructure. They are sensitive to predictable characteristics on preferences for technologies and to the nature and intensity of governance and political uncertainty. If this turns out to be relevant empirically in developing countries, it questions the systematic push for a growing role for common law as suggested by Guerriero (2016).

Accounting for norms, processes and non-pecuniary motives or not. In retrospect, one of the most underestimated damaging dimensions of institutional reforms in infrastructure

may have been the decision to import institutional arrangements from OECD countries without much consideration for the role of local cultural norms, preferences and capacities. Bardhan (2004) made an equivalent point quite forcefully over a decade ago to an African audience. The same speech could have been made in any region of the developing world on almost any sector.

The bias is common and not neutral to outcomes as often argued by behavioural economists in particular (World Bank (2015)).It may indeed drive many of the small failures in process which resulted in lower than expected effectiveness of reform. For instance, it may explain why many well-intended consultations of users to decide on projects, programs or policies often lead nowhere. Process matters in much more subtle way that the formal interactions modelled on OECD practices.

On a more positive note, a growing pool of academic evidence discussed below is documenting how and how much ethnic, religious, accounting, financial, legal or other equivalent preferences matter to maintenance efforts, to willingness to pay for basic public services or to technology choices. This research also shows that accounting systematically for these dimensions may improve performance.

Most research has focused on water because of the relevance of norms on the way common pool resources are shared, used and operated. So far, this literature has revealed insights with much broader implications for the other infrastructure sectors and for the design of institutions designed to make the most of the resources used in any sector. Guiteras et al. (2015) study the behavior of households living within compounds in slums of Dhaka, Bangladesh to document how non-pecuniary incentives could help improve sanitary conditions. They find that behaviour change messages designed to elicit disgust and shame can promote treating drinking water and hand washing with soap in low-income urban housing compounds more effectively than classic public health messages talking about germs. Shaming works in many cultures. How the shaming takes place matters though. It works better if the community cares enough to sanction norm-breakers by rewarding someone who sanctions a norm breaker with social approval and lowering the status of those failing to sanction norm breakers.

Waddington et al. (2015) survey the factors influencing sustained adoption of safe water, hygiene and sanitation (WASH) technologies. The conclusion opens research doors. Few have assessed the determinants of long-term, sustained WASH practice. Hulland et al. (2015) complement this survey by focusing on the few studies that looked at individual psychosocial factors, such as perceived benefit and self-efficacy, as well as interpersonal factors like social norms. They seem to strongly affect continued WASH behaviours. Age and gender are also strong determinants of good behavior. The broader lesson is that policy should account for this dimension more systematically. Demand management is an important complement to the focus on the technical dimensions (supply side). Project supervision should invest in longer-term behaviour maintenance. It seems that we are only in the early stages of the potential offered by behavioural economics to build on basic human characteristics to improve policy and it may be easier than anticipated in many cases. Ahtiainen et. al. (2015), for instance show that individual and perceptions-based status quo alternatives can relatively easily be documented in choice experiments for readily-observable and familiar goods such as water quality.

More generally, one the broadest lessons so far is that the users' heterogeneity (in language, culture, norms, social cohesion, social status) should matter more to the design of policies and the choices of technologies. There is a significant scope to improve our collective understanding of this work in all subsectors as argued for the longest time by many authors (Bardhan (1984), Bates (1974, 2000), Ostrom (1994), Baland and Platteau (1995) or Platteau (2000)).

Heterogeneity can also explain how norms differences make it easier to accept or reject specific institutional arrangements as shown by Athias and Wicht (2014). They look at differences in preferences for public vs. private provision of public services by French and German speaking Swiss citizens. They find it is harder to convince the Francophones to go for private provision. Ethnic heterogeneity such as differences in language or caste among irrigators impact cooperative behavior more generally.

The relevance of ethnic politics as a potential problem for the supply and distortion of public goods allocation in many developing countries has become a recurring concern in development economics and has tended to focus on the fact that ethnicity leads to benefit insiders at the expense of outsiders in public resource allocation quite generally (Miguel and Gugerty (2005), Anderson et al. 2015, Burgess et al. 2015). It is also a major concern in the management of infrastructure needs in post-conflict situations (Anand (2005), Azam (2010), Mardirosian (2010)).[23] In a recent paper, Munshi and Rosenzweig (2016) propose a model with direct implications for infrastructure. The inclusive dimensions of ethnicity favour collaboration which can be positive for public good delivery because every ethnic group wants to be represented as well as possible in multi-ethnic settings. However, there is also an exclusionary dimension that hurts the sector because it results in efforts to capture public resources rather than to optimize resource allocation. They test their model on India to show that the exclusionary effect dominates.

The concern for the challenges of designing infrastructure in an environment in which ethnicity matters is not new. Earlier, La Porta et al. (1999) and Alesina et al. (2003) had also established a negative correlation between ethnic fragmentation and infrastructure quality, among other indicators. Experimental and behavioural research is also pointing in similar directions in their assessment of trust, norms and altruism. The implication of this heterogeneity for the creation of institutions may be that it increases the transactions costs of putting them in place as well as the risks of failures.

But the research on norms defined broadly is now increasingly diversified, both in theory and in sector coverage. Aldashev et al. (2012a, 2012b) provide infrastructure relevant insights on the interactions between laws and norms, and on the extent to which new laws can ease norm changes. However, they also show how inherited norms can lead to the poor implementation of new laws. Stimulating social interactions can improve policy effectiveness across sectors. Tanguy and Torero (2015) find that in Ethiopia, in a household decision to connect to a new electrical grid, “keeping up with neighbours” mechanism can be just as important as social learning of the benefits of electricity or direct externalities of one’s connection on others’ wellbeing. Aker and Mbiti (2010) reach an equivalent conclusion for mobile phone penetration in Africa.

There is also research on norms and privatization which can be translated into infrastructure related concerns as suggested by Baland and Francois (2005). They point to some of the limits of privatization in the context of commons which are quite closely related to the literature on access pricing and rules for shared facilities such are rail tracks or transmission lines. All solutions are indeed arbitrary to some extent. The challenge is to match this bias with local preferences rather than imposed (often imported) preferences.

Religion is an easily underestimated dimension of preferences in the sector increasingly recognized as a performance driver (Aldashev and Platteau (2014)). In recent

[23] The importance of infrastructure in a post-conflict context deserves a detailed survey in itself. In this context, the reconstruction of infrastructure institutions is part of a much broader reconstruction effort. There is an on-going effort to document the issues and the options sponsored by PPIAF and lead by the Public Utility Research Center of the University of Florida which will provide a useful complement to this survey. Until this becomes available, Anand (2005) offers a very thorough review of the issues.

research it has emerged as an explanation for underinvestment in infrastructure. Pal and Wahhaj (2012) provide evidence for Indonesia of heterogeneity in preferences for public goods across communities linked to religious practice. They find greater spending on schools and health centres in communities which observe traditional *adat* laws (which promote an ethic of mutual cooperation).[24] They also find lower spending on roads, public transport, communications etc. in communities concerned with giving outside options to their members to avoid the risks of deterioration in intra-community cooperation. What this line of research shows is that resistance to change has its motives (e.g. the desire to preserve the indigenous identity) seldom factored in policy design. But it fails to show that the extent to which those aiming at preserving this identity may be ignoring or managing the complementarity between social goods which preserve the identity (linked to health and education) and the infrastructure goods. This is a policy relevant research area on which little is known at the local level for instance.

Finally, there is also growing evidence that the early theoretical insights on the relevance of processes provided by several of the views summarized earlier apply across norms and across institutional reforms. For instance, Yanes-Pagan and Machicado-Salas (2014), as mentioned earlier, document how bureaucratic delay within the allocation of small infrastructure projects in Bolivia influences outcomes. But they also show through a randomized field experiment, how monitoring tools designed to promote transparency and accountability through access to information by grassroots organizations can improve public service delivery outcomes.

Summing up. A lot of evidence has thus been collected over the last 25 years or so and there is a lot we have learned collectively from this. We have identified many institutional characteristics that matter as discussed above. In addition, we now also have a fairly predictable set of performance indicators for which we have some proxies which can be used to assess some of the main impact of changes (at least on average). These indicators include access rates, affordability, average costs, operations and maintenance efforts, service and technical quality, net fiscal cost, various financial indicators.

Table 2 summarizes my reading of this evidence. Keep in mind that I focus on the average sign that emerges by the various studies that have tested the impact of a specific institutional change on a specific performance indicator. Unless I missed out of major results, this qualitative meta-analysis of impacts of key institutional characteristics on key performance indicators calls for humility .There is little we can easily predict when it comes to the effects of institutional changes.

Table 2: Prediction of correlation between the main institutional characteristics and key performance indicators

	Performance indicators						
Institutional dimensions	**Access/ Investment**	**Affordable**	**O&M**	**Quality matching**	**Technology matching**	**Production Cost**	**Long term Fiscal Cost**
Privatization/ outsourcing	**?**	**-**	**?**	**+**	**+/?**	**Worse/?**	**Worse/?**
Deregulation	**+/?**	**?**	**?**	**?**	**+/?**	**Worse/?**	**Worse/?**
Independent regulation	**+/?**	**?**	**Better**	**+**	**+**	**+/?**	**?**
Decentralized	**?**	**?**	**?**	**+**	**+**	**?**	**?**
Shared Mandates	**?**	**?**	**?**	**?**	**?**	**?**	**?**

[24] *Adat* is the traditional law of the indigenous peoples of Malaysia and Indonesia among other countries in East Asia. It governs all aspects of personal conduct from birth to death and serves in as a conflict resolution instrument. It is over 500 years old but it has significantly transformed by the adoption of Islamic codes and of European legal systems.

The first observation to stand out is that, very little is predictable from past experiences. Neither privatization nor deregulation has proven to be bullet proof sector organization reforms for all sectors. Independent regulation seems to deliver good technical outcomes but probably at a cost which has implications for affordability and may scare off some investors keener on capturing regulators than on proper contract enforcement. Decentralization is just as an uncertain option, in particular when mandates are shared across government levels when politics are mis-aligned.

Unfortunately, the accumulated evidence reviewed here suggests that the correlation between performance and implementation characteristics such as contract commitment and enforcement strength, credibility, civil servant and regulators skills, governance and norms is mostly unpredictable. The only indicator systematically benefiting from improvements in these institutional characteristics is the investment level. And there, political interference or legal and local norms can actually impact either way. There could be a presumption of improved quality and technology matching with local preferences when local norms are accounted for, but the evidence is still too rare to be able to argue more convincingly.

Moreover, most of these results are obtained from partial equilibrium models and we have no idea how the interactions between institutional changes reinforce or weaken, on average, the common usually discussed in policy. As mentioned earlier, there are too many interactions and too much endogeneity to be accounted for to be able to come up with general statements of the desirability of specific reforms. But it should be able to use all this when going through a sector specific diagnostic in a specific country to assess the possibility of unexpected effects.

5. So where does policy go from here?

A positive or optimistic interpretation of the results summarized in Table 2 is that research has produced a lot of insights on how institutional options can impact infrastructure performance and which performance indicators to focus on. The optimism probably needs to be tempered because many insights cannot yet be turned into a coherent framework to provide analytical guidance for sector diagnostics similar to the growth diagnostics suggested by Hausman et al. (2005). They could be used to produce a robust check-list focusing on institutional weaknesses similar to those used by credit rating agencies to assess countries or firms for instance but this has not been done yet in a coherent way. Yet this is probably the minimum that one could expect from this research when poor performance calls for institutional action to support or drive policy. And it is all the more important when the initial weakness of institutions may also be important to the optimal policy tool choice (e.g. Ashraf at al. (2016)).

So far, the donor community has been quite helpful in pushing for partial performance diagnostics that focus on outcomes. In the last 10 years or so, the international development agencies have indeed provided extremely useful partial regional diagnostics of the sector (e.g. The African Infrastructure Country Diagnostics (AICD) or equivalent ones for all the other developing regions). They have also developed sector specific indicators data bases which have already been used quite extensively by academics to enhance policy analysis (e.g. The International Benchmarking Network for Water and Sanitation Utilities or IBNET).

Thanks to these efforts, we now have a better sense of the technical and to some extent the economic, financial and social performance of the sector. This also helps to appreciate the role of key regulatory policies (e.g. tariff design, rate of return estimations, asset valuation, …) in explaining this performance thanks to basic correlations. PPIAF, a

multi-donor agency hosted by the World Bank, has also financed many studies which have largely contributed to the empiricist approach and this has proven to be useful as well. [25]

But in most cases the focus is on outcomes, not on the details of policy and institutional choices. Outcomes can motivate the reform decisions and choices, but they don't characterize them. The institutional characterizations available widely from benchmarking exercises are too basic to be useful in practice in the context of an assessment of bottlenecks to performance enhancements. As mentioned earlier, having or not an independent regulator or privatizing or not, may simply be irrelevant if other governance and institutional issues are not tackled first. Worse yet, it is often hard to predict if they will help or hurt without a more thorough appreciation of a wide range of details which include norms and politics for instance.[26] The sequencing of reforms matters in this sector as in any other. But this has not really been looked at in details enough in infrastructure.

This bias against looking into the institutional details identified by research is not really unexpected since, as discussed in the survey earlier, politics matter in a tremendous way in defining institutional performance and most of the international development agencies avoid politics (at least formally). There is a realistic concern that they could be blamed for interfering with sovereignty if they were to take a formal position on the impact of politics in the sector performance.

This constraint should however not be too limitative. It has not been an issue for growth diagnostics (Hausman et al (2005)). It can simply be seen as another potential bottleneck and internalized in the efforts to match the institutional design options with desired expected performance outcome based on evidence available on what works and what doesn't. Without getting into politics, Ostrom's research has usually been quite good at looking into these details for local water projects. More recently, Vagliasindi (2008, 2012, 2013) has been able to get into highly relevant institutional details and produce new insights for the power sector. Any sector infrastructure institutional diagnostic would want to be able to produce the same level of details at the country level and then use it to link it to the scope for performance improvement.[27]

Building on the partial check-lists and evidence available, it is thus realistic to adapt the macro growth diagnostics to more detailed sector diagnostics. At that level, they should be able to identify and address the many interactions between various institutional dimensions which more technical empirical work has not been able to address. These diagnostics would also make it easier to match desired outcomes with institutional and policy options. A reasonable bet is that this assessment would lead to packages of reforms

[25] One of the problems with these multi-country diagnostics is that most of the data collection efforts financed by these institutions benefit mostly the donor agencies coordinating the data collection. Besides the usual data on access and output which is public for all organizations, the data produced for more detailed policy papers is essentially a private good which cannot be accessed by the academic community, even if financed with public money. The notable exceptions are the AICD diagnostic, the PPIAF privatization database and the World Bank water and sanitation performance indicator databases which have made all the data (in addition to the reports) available on dedicated web sites (e.g. http://www.ib-net.org/) . A simple test will make my point clearer. The reader may want to find the data on the proxies used to assess whether a country has an independent regulator or not, a variable which has been used in almost all studies in the last 10 years. It is not public.

[26] For some countries, with financing from the Spanish and French development agencies, the World Bank delivered more thorough infrastructure diagnostics linking policy to outcomes to policy choices and to some extent to institutional choices (i.e. the World Bank REDIs or Recent Economic Developments in Infrastructure available on the World Bank website). But this effort was short lived and when the funding disappeared, so did the country specific diagnostics.

[27] Ideally, the impact on performance of the composition of the regulatory authority (i.e. lawyers vs. engineers vs fiancnial analysts vs economists vs accountants) or the hierarchical structure (i.e. board of commissioners versus presidential structure) should also be documented?

rather than specific reforms that accounts for all relevant characteristics, including interactions between institutions, and will be significantly less standardized than some of today's policy decisions continue to be.

To increase the transparency of these characteristics in a more encompassing perspective, Table 3 puts together the information discussed earlier. It focuses on the most relevant from an operational perspective. It is a very rough attempt at using the information available on the relevance of institutions to conduct a preliminary institutional diagnostic of the sector. Each column is a policy area to be covered by the diagnostic. The items below the heading in each column list the specific characteristics that would have to be covered by the specific policy area.

Table 3: A minimal check list of variables to be included in a country infrastructure sector diagnostic.

Sector vision	Market structure	Ownership structure	Financing structure	Regulatory structure	Pricing	Quality
Global Strategy , incl. coherence across government levels and across regions	Degree of unbundling	Public vs private vs outsourcing vs NGOs…	Financing needs assessment	Allocation of mandates across national and subnational agencies and ministries and compatibility with local mandates and preferences	Level and structure	Match of Technical options with willingness and ability to pay
Investment plan	Choice of competition	Mandate allocation	Public vs private vs non-commercial private With vs without guarantees	Degree of independence and accountability , including composition of the board, salaries, decision and voting rules, transparency of votes, and revolving door issues	Relevance of ethnic divisions, cultural or religious norms in willingness to pay	Degree and forms of customer orientation as indicator of service quality
Financing plan	Formal vs informal provision	Risk allocation as a function of financing sources	Local vs foreign	Incentive basis of regulatory design process to assess it including its impact on incentives to accelerate or delay investments	User fees, subsidies and cross subsidies	Coherence across policy area (e.g technical& service vs. environmental quality)
Formal and informal Legal support	Urban vs rural	Relevance of ethnic divisions, cultural or religious norms in willingness to share ownership and matching responsibilities	Risk level assessment and allocation across stakeholders	Staffing (including outsourcing) Tooling (financial modelling, efficiency analysis, audits, …) and financing (budget allocation vs regulatory fee) Fining rules and allocation of fine revenue	Other forms of local financing such as microfinance or other solidarity based options	
Procurement rules for public & private	Degree of decentralization		Leveraging	Contract dependence and coherence		
	Extent of shared mandates and assessment of political alignment		Cost of capital and process for its update	Accounting requirement, required processes and technical data access		
	Employment		Asset valuation, and process incl. its impact on investments speed	Organization of consultation processes		

There are clear gaps. For instance, an explicit "politics" column could have been added. It would force the evaluators to go through a checklist of political constraints on the development and improvement of infrastructure management. But already as it is, it will force the evaluator to identify political bottlenecks within each of the more standard concerns of policy design in the sector. Ideally, this would be matched with the timing of the various requirements over the regulatory life cycle and with an assessment of the potential bottlenecks.

Ultimately what this exercise delivers is the equivalent of a general equilibrium perspective on the role of institutions in a sector. It highlights the multiplicity of local incentives but begs for an assessment of the interactions between these local incentives. These interactions help better understand the endogeneity of institutions argued by so many theoretical authors in a very operational and concrete way within a sector. But our conceptual knowledge of the interactions mechanisms and of the joint relevance of the initial sector and country conditions are not yet well understood. And this is where additional theory has to help.

6. And where could research go from here to help policy?

There are at least four broad research areas that would help the policy effectiveness of academic work on infrastructure based on the implicit gaps revealed by this survey of what we know on the relevance of institutions for infrastructure performance (in terms of access and coverage rates, pricing and affordability, efficiency, technical and service matching with willingness and ability to pay, cost recovery and financial and fiscal viability). These have to do with:

(i) Theory
(ii) Cross country empirics
(iii) Randomized controlled trials and other impact evaluations
(iv) Data.

On theory. The theoretical coverage of the institutional dimensions of the sector is already impressive, although somewhat atomized across theories. The sum of it all is useful, but incomplete. There are four more research areas that help fill the gap the importance of institutions for policy effectiveness in this sector.

The first gap is a general positive theory that figures out a way of picking up many of the interactions of the various institutional dimensions. The ceteris paribus assumption is really costly in this sector. It misses out on interactions and yet it has justified the focus on single policies in a field in which policy packages make a lot more sense because of these interactions. Too much of the empirical evidence would suggest that the theory on the sector should also account for interactions with institutional constraints which are not sector specific. The evidence shows that the general initial conditions in a country, a region or a city can explain differences in outcomes associated with the same sector specific institution.

The second gap may be the lack of attention paid to the dynamics of the sector and to the interactions between institutional dimensions. Infrastructure investment is central to improvements in access rates and to countries' ability to deal with climate change and other environmental concerns. Moreover, the institutional matching game is a moving target so a lot of the empirical can only validate a snapshot, not the adequacy of this snapshot on the path to adjusting institutional and other conditions. The right institutional context is, indeed, probably an adjusting institutional context which can follow several paths, depending on the interactions within the sector and with other segments of the economy. The matching is a dynamic challenge. And yet a lot of the theory used to justify

institutional choices is anchored in partial static models. This is partially because it is quite difficult to get to simple first best policy recommendations in a world in which time inconsistency linked to policy uncertainty and asymmetric information appear jointly. The only thing we know for now is that the fear of regulatory hold-up on irreversible investments (i.e. arbitrary cut ex-post on promised return on that investment) may lead to under-investment…ceteris paribus.

The third gap may be the underestimation of the interactions between institutional choices in infrastructure and financing options. Regulation theory has not been very effective at linking finance and optimal regulation choices to begin with, but when it has made an effort to do so, it has ignored how this choice could bias institutional dimensions.[28] Are institutions in the sector captured because of financial constraints or is the weakness of institutions the driver of the optimal financing solution? The modelling of optimal regulatory choices is too basic to allow an identification of the main relevant trade-offs. It also ignores the possibility of a two-way causality between infrastructure institutions and financing options. Policy deserves a better effort to consider more explicitly the relevance of finance for the design of incentives in the sector, accounting for institutional strength and weaknesses.

The final direction in which theory could improve its current contributions is with respect to the relevance of politics. Politics is everywhere in this sector and academics have been good at picking it up. But it has not really been internalized as operationally as needed to sort out the matching challenge in a political sensitive context. This line of research would merge the traditional optimal regulation theory with insights from the political economy of reform in a dynamic perspective. This would account for the details of the full project cycle of the investment decisions in infrastructure, including the key earlier stages in the decision making process (including for instance the role of planning and procurement limitations). It would also look into the ways in which the margin for political capture can reduce or offset the concern for holdup. Lim and Yurukuglu (2015) provide a dynamic model that looks into this possibility for US electric companies.

On cross-country empirics. On the empirical front that can be useful to policymaking, there are a few low hanging fruits (although time consuming) that should be picked by the academic community. The first is the need to close surprising knowledge gaps on what we learned from the dispersion of results on the impact of institutions over countries/regions and over time. A meta-analysis such as the one conducted by Bel et al. (2010) on the impact of reforms on public services for OECD countries, has not been conducted for developing countries in any sector. Most of the surveys available tend to be like this one: a qualitative interpretation of the dominating factors. These are quite useful already and the 3i NGO has been very effective at making these sorts of overview available to wide audiences. But they leave too much margin to subjectivity in deciding what matters and what does not. This can be avoided for papers following roughly comparable approaches to the analysis of a problem through a meta-analysis.

Note that it would also be useful to a have a few surveys or meta-analysis of price and income elasticity per income classes and user types, accounting for various dimensions such as initial economic, social and institutional conditions. These are the sort of surveys that help turn qualitative discussions on reforms into quantitative ones. This is potentially just as important as having information on capital or labor supply to taxes. Yet it is much harder to come by.

[28] The main exception is a reasonably solid literature on the impact on the cost of capital of regulated industries of the regulatory regime (Alexander et al. (2000), Jenkinson (2006) and a matching literature on the relevance of regulation for the choice of leveraging (e.g. Spiegel (2994) and Moore et al. (2014)).

A second quick payoff empirical piece would be a more systematic tracking of the degree of concentration of international players capable of influencing procurement of infrastructure markets. As mentioned earlier, there is a lot of anecdotal evidence showing that market concentration is penalizing the development of formal local players in the sector. There is equivalent evidence of excessive operators' profitability as a major political issue in view of the wide diversity of feelings about the potential role of the private sector. There seems to be a good reason to document this more precisely. This concentration was a documented issue before the global crisis (Benitez and Estache (2005)). It would make sense to assess whether the crisis has increased the bargaining power of large players in the sector or not. And quite frankly, it would also make sense to have a look at the extent to which aid money is captured by the big players in the sector simply because procurement rules have a biased in favour of large historical players in the sector.

A final potential area of research with lots of externalities would be a good think piece on how to deal with the endogeneity of institutions. The multi directional causalities that case studies and anecdotal evidence points to are an econometric nightmare. Few of us have taken the time to think them through. Yet, this would certainly improve the quality of empirical work on the role of institutions in the sector.

Randomized controlled trials and other impact evaluations. This is where there is scope for more creativity.[29] There are already a large number of experiments being produced on the water and sanitation component of infrastructure. There is much less work on the scope for nudging accounting for norms, culture and other behavioural biases for electricity and transport. In transport, for instance, assessing the extent to which these biases often built in institutional characteristics can help promote modal switches, safety, speed control, reductions in fraud in the use of public transport are all important research topics with strong potential payoffs in developing countries. Of particular interest in this context is the relevance of the heterogeneity of preferences and perceptions of the value of services. This will have implications for the effectiveness of traditional policies but also for the scope to improve the financing of a highly subsidized sector.

The approach is also quite useful to look the role of informal and local solutions as well as the role of NGOs, cooperatives and other community anchored arrangements in water and energy. This would be particularly useful when considering the possible relevance of institutional designs and market organizations in minimizing the risks of quantity-quality trade-offs in environments in which budget constraints are quite binding. It is also relevant to assess the opportunity alternative modes of organization and provision cater to the sense of urgency which is seldom reflected in the discount rate used for most project assessments. Time matters in a way not reflected in the standard 8-12% discount rate used by the donors community in assessing its projects. This is just as true for infrastructure in general as it is for climate change related projects. Randomized trials should help demonstrate this more systematically.

We also need to have a better sense as to how fairness is perceived in the context of all infrastructures activities with common goods dimensions. Extending Ostrom to look into the roles of religious and ethnic solidarity to all local infrastructure is a potential field of interest to the sector as well. An equivalent assessment of the reasons why supranational projects are not as popular as rationality anchored in scale economies would suggest is another, somewhat related research field.

Finally, this tool is also potentially quite useful to assess the extent to which local norms can turn incomplete contracts into complete contracts without having to rely on

[29] For up to date inventories of infrastructure related experiments, see 3ie or PPIAF (http://www.ppiaf.org/page/ppiaf-impact-stories)

formal regulators. Local arbitrations are quite common and may be much less costly than imported institutions, at least for local public goods.

Each of these suggestions should be framed in an effort to learn to improve the optimal matching of institutions, instrument choices and performance goals. It is a way to validate some of the intuitions generated by cross-country empiricisms. It is also a way to document more effectively the institutional sources of potential trade-offs between the different performance indicators.

On data. Data gaps on infrastructure are a recurring theme that the international community has not yet managed to close to any decent level of satisfaction to either academics or policymakers. [30] As mentioned earlier, regional benchmarking exercises have been conducted and there are a few international sector specific data sets. But most of this data suffers from three major issues.

First, it is biased towards information relevant to engineering supervision, not to financial, fiscal, social or institutional supervision. Second, the data is often missing, incomplete or too aggregated to be able to deliver solid basic diagnostics without some creative econometric or other statistics treatment. Better and more detailed data will also allow more robust tests of the interactions. The next time a regional or sectoral diagnostic is conducted by the international community, it would make sense to add a questionnaire producing standardized information on many of the variables identified here and summarized in Table 3. Third, a lot of the data is a de facto private good controlled by donors with limited access to the international community (WB, Regional DB, OECD, IMF and all bilaterals + PPIAF + IEA). In some cases, the agencies actually sell the data even if this data has already, de facto, been paid by the international community. Just as for academic publications, when there is no reason to claim a confidentiality clause, the data collected should be a public good. Infrastructure data is no different from national accounts, public finance, health or education data which are all of easier access.

An additional data issue to address to improve the quality of project specific data available from the major development agencies is implicit in Kilby (2000, 2015) where he demonstrates quite a bit of creativity in processing the data on the quality of project preparation to assess its impact on project outcomes for international organizations. For now, he argues, it is possible for project outcome assessments to inform the evaluation of preparation (or other aspects of donor performance) which points to a major endogeneity issue. The more general data issue is that the monitoring systems in place are a problem that deserves a more detailed look. Kilby (2000) circumvents the feedback between performance and supervision by examining the link between supervision over a given year and the subsequent annual change in an intermediate measure of project performance.

7. Concluding comments

Notwithstanding the many limitations identified in this paper, the accumulated evidence provides some robust policy, research and dissemination messages.

The first is, unsurprisingly, that *institutions do indeed matter in the sector, both formal and informal ones.* They matter probably much more so in developing countries than in developed countries because access gaps are strongly correlated with weak institutions and not just financing constraints. The institutional challenges are also more complex in poorer countries because many of the ad-hoc solutions adopted to compensate for the lack of more modern approaches are often location specific rather than national or regional. These

[30] Berg (forthcoming) makes the point for the water sector in quite a systematic way.

pragmatic solutions developed to ensure a reasonable sector performance include options anchored in cultural, religious and other equivalent preferences as well an increase of the role of NGOs and other similar organizations created to give a voice to civil society. Complexity is not only part of the problem as sometimes claimed by "one size fits all" reformers, it can also be part of the solutions to performance enhancements efforts.

Second, *there is no magical solution that suits all situations and initial conditions matter much more than pre-packaged approaches acknowledge*. These initial conditions drive not only the optimal timing and sequencing of change but also the specific institutional choice that makes sense at that time. The sector diagnostics suggestion to focus on the identification of the main bottlenecks to target policies and institutional changes can be useful to design and rank options with a dynamic view of progress for the sector. There has to be a matching of institutional constraints with policy, financing and regulatory options which has not really taken place systematically enough in this sector

Third, in cases of lasting undesirable rigidities—i.e. when complexity is a problem rather than a solution--, *change for the sake of change is sometimes useful*, i.e. a shock therapy. This is particularly relevant if change is designed to deal with bottlenecks to minimize the risks of further long term change (most notably to break "captured" institutions). This is a pragmatic perspective often used in practice to buy time. Time may indeed be the most underrated dimension of efforts to improve sectors and institutions in a sustainable away. To work for the long run, the short term shock therapy needs to be coordinated with the vision of the sector. This has proven to be a problem in an industry which has largely dropped serious planning from its toolkit. The therapy also needs to account for an assessment of the long term dynamics of the political economy of the sector. And this can be quite challenging as the winners and losers of the changes tend to evolve throughout policy and project cycles. This is what time consistency of a policy means in practice in the politically charged infrastructure sector but is often omitted by shock therapists.

Fourth, *the margin for policymakers and their advisors to use theory to improve performance is significant*. This is particularly obvious in the fact that current often outdated procurement and project management practices have not been able to stop or even slow the continuous flow of new corruption, capture and collusion cases at a time procurement theory has enjoyed an explosion of policy relevant creativity. Similar conclusions could be reached on the use of regulatory tools (e.g. efficiency measurement under data incompleteness, asset valuation techniques, …). Implementation details and accountability for these details matter to the performance of the sector, to the size of the rents linked to the sector and to the distribution of this rent between users, foreign and local investors, taxpayers and …politicians. They matter at all stages of the project or policy cycle if the basic information asymmetry problem is to be solved. In this sector with so many limitations to competitive presses, procurement and regulation theory have made the case for the adoption of cost accounting systems designed to improve the monitoring of the residual non-competitive segments of the sector. This case has largely been ignored in the developing world in interactions between governments and large scale operators. [31]

Fifth, *the margin for academics to get a better sense of the realities of the sector is also still quite large.* This is largely what defines the research agenda discussed earlier. There is, indeed, a lot we still don't know about how institutions can be improved to make the sector work better, including with respect to the interactions between formal and informal

[31] The theory has also other more detailed concrete implications. For instance, without professionally dedicated teams equipped to plan, evaluate ex-ante projects, organize the procurement process and supervise or regulate as needed and that can be audited by truly independent third parties, many policies and projects are less likely to be effective.

institutions in the slow transitions towards more costs effective solutions to increase affordable and reliable access to these services. The research agenda is not small and is much needed to close some significant knowledge gaps identified in this paper. This include an explicit assessment of the role of institutions as a function of context (i.e. infrastructure has a different role in poor, post conflict, middle income countries). Ideally, it should make room from inter-disciplinary learning as already argued by authors such as Bardhan, Ostrom or Platteau. In this contexts, we can learn a lot from the cases in which changes were counterproductive or resulted in excessive rents captured by operators and sometimes shared with dishonest politicians. The policy focus tends to be on best practice, but bad practice can teach at least as much.

Sixth, despite the many knowledge limitations and gaps, *we know enough to be able to start conducting pilot institutional diagnostics of the sector in a somewhat structured way more effectively than less than 10 years ago*. From a research perspective, these diagnostics are also useful because they will highlight the black holes on policy relevant dimensions deserving better analytical assessments, some probably omitted by this survey. But it is not enough to close the knowledge gaps. It is worth emphasizing, to conclude, that any effort to close these gaps should: (i) be anchored in a commitment of the main aid agencies to coordinate their efforts to collect decent data on the sector on a regular basis, and, just as importantly, (ii) ensure the sharing of the data with all the stakeholders rather than simply sharing the results of the analytical data treatment. This could be achieved with a closer collaboration between these agencies and the academic world which would allow early academic inputs in the process and allow some scope for a larger number of independent analytical perspectives on the lessons to be drawn from the data.

Without an effort along those lines, it will be hard to improve our collective assessment of the matches between institutional constraints, options and outcomes. We will be stuck with the very useful but incomplete sporadic large scale efforts covering multiple countries, at a fairly aggregate level, and with digestions (like this one) of many partial, not necessarily comparable, case studies. This is useful, but the scope to do so much better is significant. Why not try it?

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